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NEW SERIES

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No. 1

THE ANTIQUITY OF THE BRITISH BRONZE AGE

By V. GORDON CHILDE

REVOLUTIONARY progress has been made in British prehistory during the last ten years.¹ But in interpreting their new discoveries British prehistorians have displayed excessive modesty in estimating the role of their islands in the development of civilization in northwestern Europe. In the first place they have not altogether shaken off the spell of the impressive typological system brilliantly outlined by Montelius for north Germany and Scandinavia and developed into a rigid frame of reference by Kossinna, Hubert Schmidt, and others. A wealth of richly furnished graves and hoards from Scandinavia and north Germany had been reduced to a relative chronological series by Montelius and Sophus Müller, and subsequently Kossinna and others applied Montelius' scheme to material from central and western Germany. Quite naturally the imposing system thus constructed was used as a standard of comparison to which the material from countries, less rich or less systematically explored, should be made to conform.

But as the British and Irish material is poor and monotonous in comparison with the Danish or German, the British series of typological and stylistic phases on paper looks short, and its beginnings consequently appear belated. For instance, the British Isles cannot display such a variety in ceramic styles and cultures of a Neolithic aspect as can Denmark or Saxo-Thuringia. The inference would be that the New Stone Age began later in the British Isles than in North Europe. And so the appearance of a trait there is liable to be mistaken as a *terminus post quem* for the appearance of a corresponding phenomenon in the United Kingdom.

But in reality, as Danish, Swedish, and German archaeologists² are now

¹ Summarized down to 1932 in T. D. Kendrick and C. F. C. Hawkes, *Archaeology in England and Wales, 1914-1931* (London, 1932) and for Scotland, V. Gordon Childe, *The Prehistory of Scotland* (London, 1935). Special references will not be given to points adequately documented in these books.

² Especially Nils Åberg, *Bronzezeitliche und früheisenzeitliche Chronologie* (Stockholm, 1930-35); Waldtraut Böhm, *Die ältere Bronzezeit in der Mark-Brandenburg* (Berlin, 1935); H. C. Broholm, *Studier over den yngre Bronzeælder* (Copenhagen, 1933); J. E. Forssander, *Die*

constantly insisting, the imposing array of typological periods presented by Kossinna or Hubert Schmidt is the result of inflation. Distinct cultures or groups of objects which in reality coexisted in time have been arranged in temporal sequence and converted into periods. The simplicity of cultural phenomena in the British Isles and the absence of (or failure to recognize) competing cultural groups need not mean that the record of the Stone and Bronze Ages here covers a shorter time than on the Continent.

Secondly, our most active prehistorians have made the most outstanding discoveries in Wessex, Sussex, and East Anglia, and consequently tend to interpret all the data of British archaeology from a "Lowland" angle. But Fox² has noted and explained a significant dichotomy: over against Lowland England where "new cultures of Continental origin tend to be imposed," lies a "Highland Zone," northwest of a line from Teesmouth to Torquay, where "such intrusive cultures tend to be absorbed." (This frontier line is indicated on our Figure 1 by a broken line.) Now the prehistorian of the Lowland Zone is naturally properly interested in the first instance in tracking down to their continental origins the successive cultures that were imposed on his area. And from his standpoint the emergence of such cultures on the Continent does really constitute an upper limit in time for their arrival in Britain.

But the importance of the British Isles in prehistoric times depended upon their wealth in minerals—gold, copper and, above all, the rare but essential tin—just as Britain's supremacy in the early years of the Industrial Revolution was based upon coal and iron. Now these mineral resources are restricted to the Highland Zone, which includes Ireland and Cornwall. Their exploitation may have attracted invaders from the Continent to Lowland Britain, but was not dependent upon such, and to judge events in the Highland Zone by the Continental standards, valid for the Lowland, will obscure any priority the former may have enjoyed.

Britain's mineral wealth itself constitutes a third factor veiling the real antiquity of culture in the islands. In Central Europe the Bronze Age gave place about 750 B.C. to the Iron Age culture of Hallstatt. But in Great Britain there is no Hallstatt period contemporary with the first Continen-

archaische Bootstichkultur (Lund, 1933) and *Der ostbaltische Norden während der ältesten Metallzeit Europas* (Lund, 1936); C. A. Nordmann, *The Megalithic Culture of Northern Europe* (Suomen Muinaistutkimusten Aikakauskirja, Helsinki, 1935); Paul Reinecke, in *Germania*, Vol. 15, 1931, pp. 196, 304, Vol. 17, 1933, p. 11; G. Schwantes, *Geschichte Schleswig-Holsteins* (Vol. 1, Neumünster, 1935-36); Ernst Sprockhoff, *Die Kulturen der jüngeren Steinzeit in der Marz-Przedeczburg* (Berlin, 1926); *Zur Handelsgeschichte der germanischen Bronzezeit* (Berlin, 1930).

* Cyril Fox, *The Personality of Britain* (Cardiff, 1932).

tal Iron Age, only a belated arrival of peoples with a Hallstatt culture. And in the Highland Zone even the second Iron Age or La Tène period is

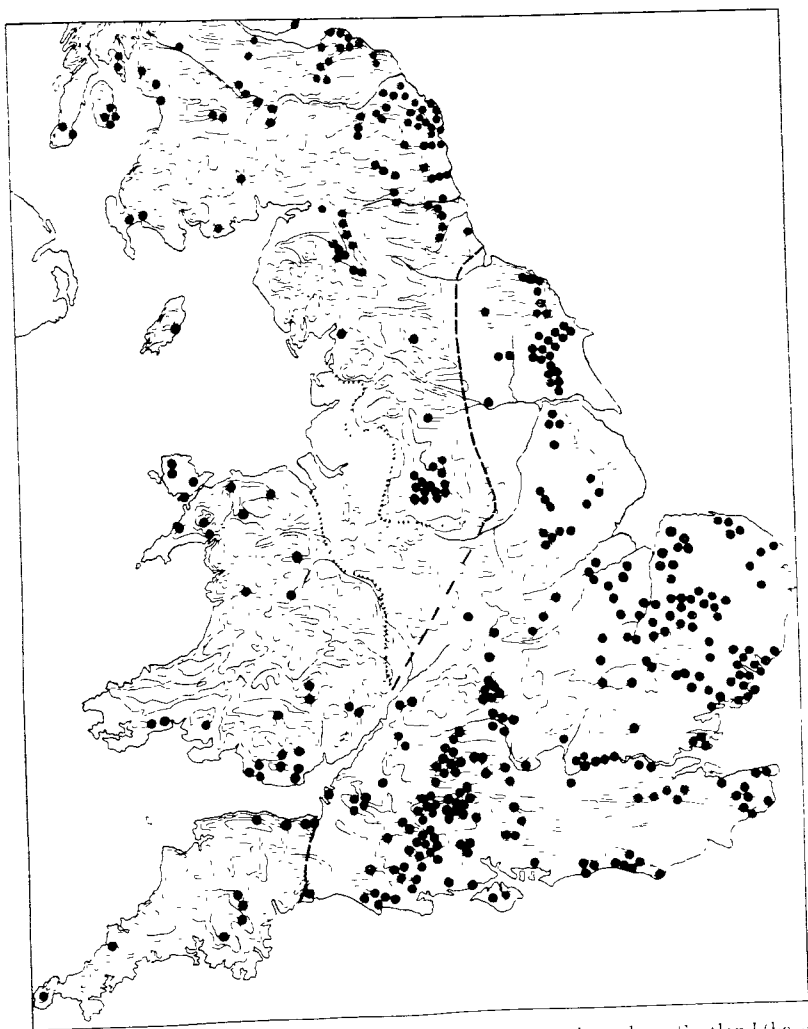


FIG. 1. The distribution of Beakers in England, Wales, and southern Scotland (based on maps published by Sir Cyril Fox and Miss Chitty and by Dr Crichton Mitchell).

poorly represented. In fact the Bronze Age in Ireland and Highland Britain lasted even later than in Denmark, much later than in southern or central Germany. To infer from this late ending a late beginning for the Bronze

Age in Britain is all too easy. But such an inference would be quite fallacious.

The principal advantage of iron as an industrial metal was its cheapness. But an almost unique wealth in copper and tin enabled the British, or rather Irish, metal workers to produce bronze in such quantities that it could compete here longer with the new metal than in less favored regions. That provides a perfectly good explanation for the long persistence of the Bronze Age in the British Isles. It provides an equally good reason for anticipating an early beginning. And that anticipation is well justified, as will appear in the sequel. Our Continental colleagues are repeatedly recognizing bronzes, imported from Britain, in graves or hoards belonging to well-defined horizons in the Scandinavian or Central European typological sequence. And through such finds it is possible to correlate more accurately the relative chronology established by internal evidence for Britain with the chronological systems valid (after deflation) for North and Central Europe. For through an import from Britain, the typological period in which it occurs becomes a *terminus ante quem* for the creation of the type in Britain.

Two obstacles, however, remain to the correlation of the cultural phases in Britain with the Continental. In Lowland Britain we have a reliable sequence of phases distinguished by grave-goods and by ceramic styles, and a still more reliable sequence based on the typology of bronze tools and weapons. But a synchronization of the two series, the one based on grave-finds, the other on hoards, is possible only to a very limited extent. And secondly the sequences valid for Lowland England cannot be applied without modification to Highland Britain and Ireland. We have therefore to present the sequences derived from graves and the typological series of bronzes in separate columns, and in each case to give distinct columns to the Lowland and Highland Zones. In the subjoined table no exact parallelism is asserted between the entries in any two columns and no two consecutive phases are to be regarded as rigidly exclusive: there are certainly overlaps. But with those provisos the scheme shown in the table may be taken as a simplified tabulation of agreed results.

The priority over Beakers of Windmill Hill ware has been demonstrated stratigraphically in southern England, but the position of Peterborough ware is ambiguous. Then A-C and B Beakers admittedly represent distinct invasions,⁴ both probably from Central Europe by way of the Low Countries though direct connection with western France is not altogether

⁴ V. Gordon Childe, *The Bronze Age* (Cambridge, 1930), p. 155, J. G. Clark, *The Dual Character of the Beaker Invasion* (Antiquity, Vol. 5, 1931), pp. 415 ff.

TABLE 1. STAGES OF THE BRONZE AGE IN BRITAIN

FUNERARY DEPOSITS		HOARDS			
I		II		III	IV
<i>Lowland Britain</i>		<i>Highland Britain</i>		<i>Lowland</i>	<i>Highland</i>
A, 1	Neolithic A (Windmill Hill)	<div> <div> <div>Long barrows</div> <div> <div>a, 1 Beaker ware</div> <div>a, 2 Unstan ware</div> </div> </div> <div> <div>Beakers (A-C)</div> <div>(B)</div> </div> </div>	<div> <div>(Chambered cairns)</div> </div>	Neolithic Bronze Age	Neolithic Bronze Age
A, 2 (?)	Neolithic B (petrified wood)				
B	Beakers (types A-C)				
C, 1	Food vessels				
C, 2	Grape cups and pigny vessels	<div> <div>Round barrows</div> </div>	<div> <div>Round cairns</div> </div>	I	1-4
C, 3	Handled urns				
D	Overhanging rim urns				
E, 1	Globular and bucket urns	<div> <div>Urn-fields</div> </div>	<div> <div>c, 3 Handled urns</div> <div>d Overhanging rim urns</div> <div>Enlarged food vessels</div> <div>e Cordoned urns</div> </div>	II, a	2, a = II, a
E, 2	Cordoned urns				
F	Iron Age A				
				III	2, b = II, b
				IV	3, a = III
					3, b V-type swords, socketed axes and developed types of 3, a

excluded. The foreign urns of group E, 1 no less certainly denote invasions from the Low Countries and north France respectively, and a third wave

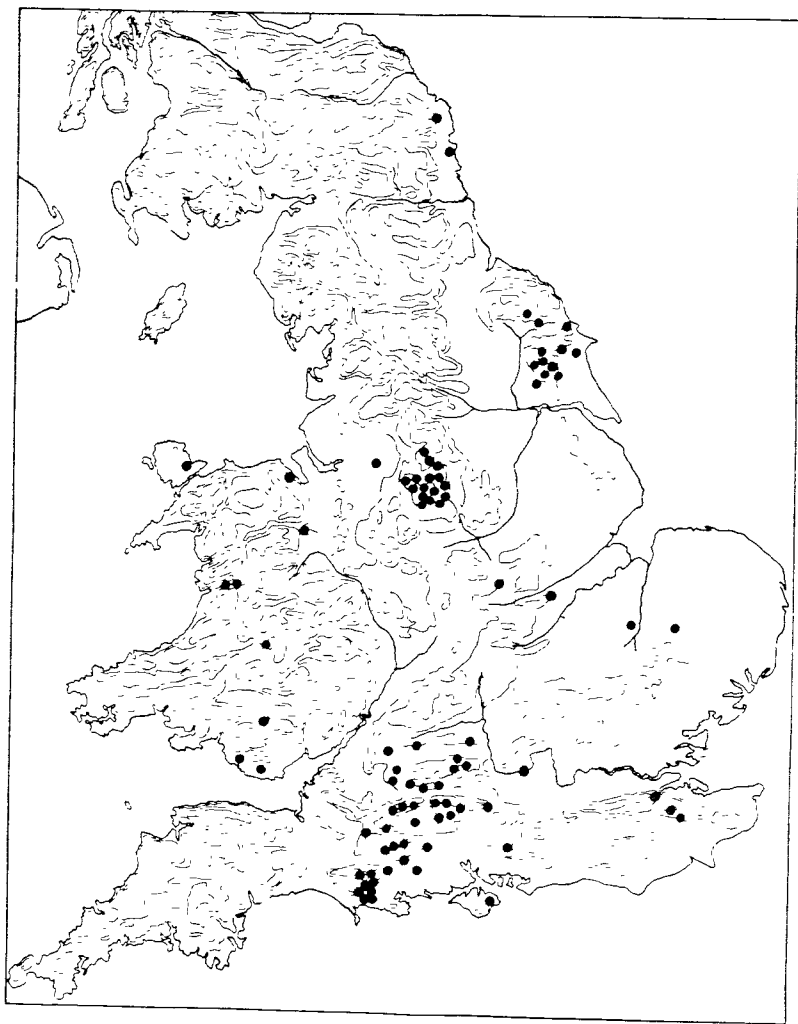


FIG. 2 The distribution of flat round-headed daggers in England and Wales (based on the map by Cyril Fox and W. F. Grimes).

of invasions brings in Iron Age A. In C, food vessels spread from Ireland and North Britain mainly into the eastern Lowlands; Grape Cups (fig.

7: 2) and Pigmy Vessels (plate 1) are liable to be associated with Cinerary Urns of group D, as are the handled urns, commonest in Cornwall. In at least one barrow an overhanging rim urn (like fig. 7: 1) is said to have preceded a beaker-burial. So phase C-D is as a whole vague and ill-defined, while phase B must be long enough to overlap with C.

In Highland Britain plain Windmill Hill ware occurs as generally as in Lowland England. In North Ireland and southwestern Scotland it is associated with Becharra ware, in Orkney with Unstan ware. Both fabrics may be regarded as Windmill Hill ware with new elements superadded. In Skye and the Hebrides beakers are shown stratigraphically to be later than Becharra ware. But beakers scarcely reached Cornwall and are practically non-existent in Ireland. Hence in Ireland the place of beakers may be taken by food vessels, and even in Scotland and Highland Britain the relative positions of beakers and food vessels may in some cases be reversed. Similarly in Cornwall handled urns should perhaps take the place of beakers (? in phase b) and food vessels in phase c. The foreign urns of Lowland phase E are entirely missing in the Highland Zone, and Iron Age A is still very ill-defined there. In Scotland urns of phase e occur in the same cemeteries as those assigned to phase d, so that the distinction of these two phases is again fluid. In Ireland the whole series of cinerary urns (d-e) looks late and intrusive, so that a prolonged survival of food vessels and a substantial overlap of phase c with d and even perhaps e, must be admitted as a possibility.

Turning to column III, the sharpest division comes with group III, the Late Bronze Age, when the first appearance of founders' hoards marks a reorganization of the metal industry. Many persons, notably Peake and Crawford, have invoked an invasion to explain the change in industrial structure and the abrupt appearance of a mass of new types such as the socketed axes and slashing swords; for these types can hardly have been developed in Britain but seem to have been introduced fully formed from the Continent. An invasion is more probable as an explanation of group IV; for hoards containing these types are restricted to Lowland Britain, while the types in question are probably of Swiss origin. This group in any case does not reach the Highland Zone. Period IV there is merely a development of period 3,a and these developed or degenerated period 3,b types occur also in the Lowland Zone. Apart from the absence of group IV from the Highland Zone and the failure of group II,b to extend north of the Southern Uplands in Scotland, the distinctive bronzes are fairly evenly distributed throughout the British Isles, and the typological periods they denote are applicable equally to both areas.

The insoluble problem is the correlation of column III with columns I and II. West European daggers are associated in graves with beakers of type B; flat triangular daggers⁵ have been found five times in graves with A-C beakers, four times with food vessels, once with a grape cup, and four times with overhanging rim urns. Ogival daggers (fig. 3: 1)⁶ were associated twice with beakers, twice with handled urns, and three times with pigmy vessels (plate 1). But here associations between funerary pottery and typologically significant bronzes cease. Rapiers and palstavs have never been found in graves, still less socketed celts and swords. In one case a spear-head with protected loops was demonstrably older than a foreign urn of group E,⁷ but socketed axes have been found with Iron Age A pottery, therefore in phase F, even in southern England.

And this impasse cannot be avoided by establishing coincidences between the distributions of bronze types and of funerary pottery. While there are regional limitations to several ceramic forms, the various bronze types overstep any such boundaries. In particular the early bronzes of group I and the late bronzes of group III are as plentiful in Ireland and the West as in Lowland England. It follows that there is no connection between the Beaker culture and the beginnings of the bronze industry, nor between the invasion denoted by urns of group E and the industry's reorganization. On the contrary, in England Chitty, Grimes, and Fox⁸ have established by maps (figs. 1 and 2) that the triangular daggers and flat axes of group I must have been imported from Ireland or from the southwest, while in Scotland Mitchell⁹ has shown that the distribution of these types approximates more closely to that of Irish food vessels than to that of beakers.

The idea that the Beaker-folk introduced bronze and bronze-working into Britain from Central Europe must be abandoned. It is in fact refuted by the absence of beakers from the tin producing region of Cornwall and from the copper-mining area of Ireland. As the late Dr Bremer¹⁰ inferred, Cornish tin was exploited by Irish metallurgists. The Beaker-folk were doubtless the first people in Britain to deposit in their chieftains' graves

⁵ Cyril Fox and W. F. Grimes, *Corston Beacon: an Early Bronze Age Cairn in South Pembrokeshire* (*Archaeologia Cambrensis*, 1928), p. 148; Childe, *Prehistory of Scotland*, p. 87.

⁶ R. S. Newall, *Barrow 35, Amesbury* (*Wiltshire Archaeological Magazine*, Vol. 45, 1930), p. 455.

⁷ R. C. C. Clay, *A Late Bronze Age Urnfield at Pokesdown, Hants* (*Antiquaries' Journal*, Vol. 7, 1927), p. 470.

⁸ *Personality of Britain*, p. 17; C. Fox, Presidential Address (*Proceedings, Prehistoric Society of East Anglia*, Vol. 7, 1933), p. 155.

⁹ Dr Mitchell kindly allows me to refer to her unpublished map.

¹⁰ *Ireland's Place in Prehistoric and Early Europe* (Dublin, 1928).

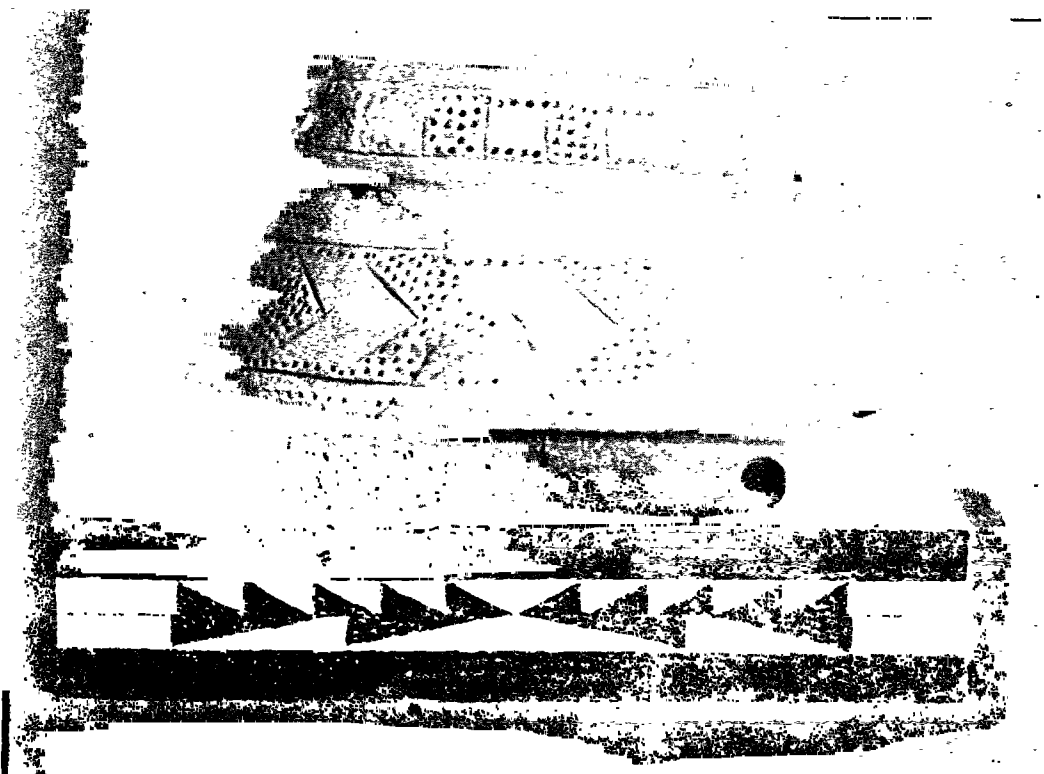
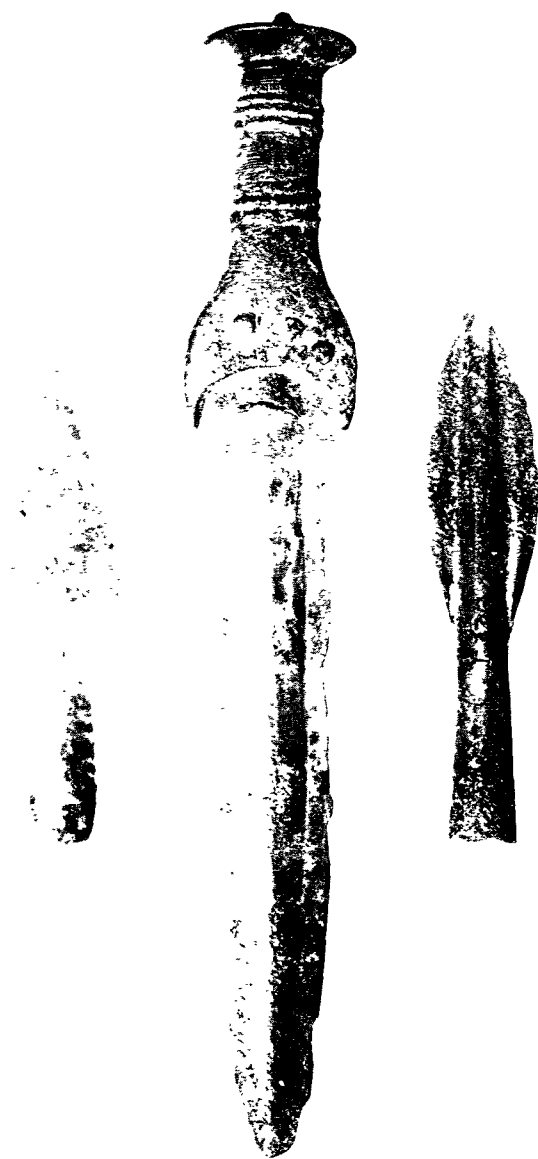


Fig. 1. Vessel and whetstone found near Camerton, Somerset, with the objects shown in Figure 3 (Bristol Museum).



Flint dagger, Nordic bronze sword, and British spear-head found together in a grave at Liesbützel, Holstein (Schleswig-Holsteinisches Museum, vorgeschichtlicher Altertümer, Kiel)

bronze tools and weapons, but the local industry that produced the bronzes may be indefinitely older than the invaders' advent.

On the other hand, bronzes are absent from the long barrows and chambered cairns that are supposedly pre-Beaker. Moreover, as we have already noted, bronzes of groups II and III are distributed in the British Isles regardless of the cultural provinces that can be defined by the variations in funerary pottery. Perhaps we should conclude that the British-Hibernian metal-workers constituted a class or guild, ritually excluded from the dominant societies and ruling classes whose funerary rituals have left us the imposing megalithic tombs and long barrows that are termed "Neolithic" and the no less imposing round barrows and stone or wood circles that belong admittedly to a Bronze Age. In Celtic Ireland written sources show that smiths did actually stand outside the dominant political organization in some such way.

The assumption of an industrial corporation, independent of prevailing political structures, renders superfluous the postulate of an invasion to explain the transformation of the bronze industry in period III—an invasion which exerted no effect on burial rites or funerary pottery! Both the reorganization of the industry and the adoption of new types and processes were economic, not political issues, questions for craftsmen, producing, as we shall see, for a "world market," not for small groups of local consumers. Whether the British metal-workers admitted new members to their guilds or served apprenticeship to the new processes when travelling as merchants is of course indeterminable. It is certain that the new types appearing with group III were not exclusively of Central European origin. The curious blind razors, for instance, seem to come from Sicily across south France and by sea.¹¹ Just so later on in period 3,b the models for Irish cauldrons were borrowed from Italy without the intervention of Central Europe.¹²

Now it is only the exportation of British products that affords an accurate and reliable indication of the relative levels of cultural development attained in these islands and on the Continent in early times. Such exportation fortunately began early in the Bronze Age. But no cultural or typological sequence established for Northern or Central Europe provides a *terminus post quem* for the commencement of that period in the British Isles. I formerly believed that by fixing the "Beaker period" in the Central European sequence we automatically obtained a limiting date for bronze-working in Britain. But we have already seen that the local bronze industry did not begin with the Beaker invasion. Moreover, beakers can no longer be

¹¹ Childe, *Bronze Age*, pp. 100, 231.

¹² E. T. Leeds, *A Bronze Cauldron from the River Cherwell* (*Archaeologia*, Vol 80, 1930), p. 27.

regarded as marking a precise chronological horizon on the Continent, as Hubert Schmidt and Åberg imagined. In Central Europe in particular Beaker burials are so numerous and the beakers display such a variety that they must have flourished for a considerable period. In the small area of the upper Elbe basin in Bohemia over 270 beakers are known,¹³ almost

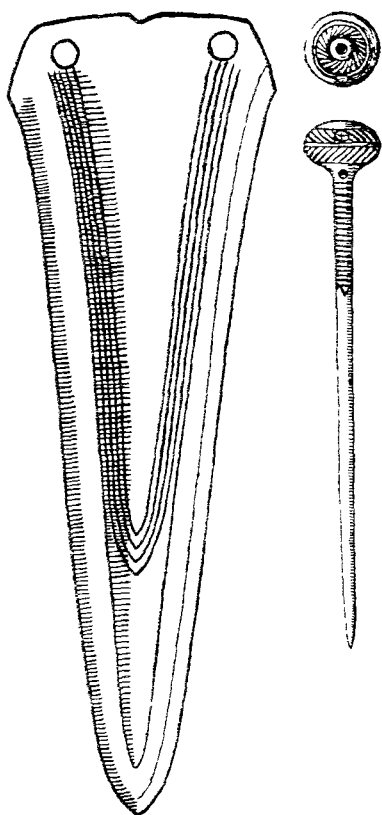


FIG. 3. Original dagger and bulb-headed pin from a barrow near Camerton, Somerset (Bristol Museum) (from a drawing kindly supplied by Mr. Stanton) (Scale 2/3.)

as many as in the whole of Scotland. In Scotland Beakers were admittedly in use over several generations. In Central Europe this truth is obscured, because the Beaker culture represents only one out of several contemporary Neolithic cultures, while in the British Isles the only recognized competitor is the group denoted by Windmill Hill pottery and its derivatives.

¹³ Alban Stock, *L: Bohème préhistorique* (Prague, 1929) listed 226 in 1925. Dr. J. Böhm kindly informs me that 53 have been discovered since then.

usually in Saxo-Thuringia and Bohemia

The Beaker culture together with those of the *Oderschnurkeramik* and the *sächsisch-sächsische Keramik* under the influence of a foreign southeastern culture were gradually fused together into the unitary ceramic group which we designate the Aunjetitz culture.¹⁴

Further southeast in Austria beakers are found in the same graves as those of Aunjetitz form, and vases normally associated with beakers may be accompanied by halberds and round-heeled daggers of full Bronze Age, Aunjetitz, form.¹⁵ In three graves, in Moravia, Bohemia, and the Rhine Valley respectively, beakers were accompanied by round-heeled triangular daggers as in our group I. More often in Czechoslovakia, Bavaria, Saxo-Thuringia, and even Holland, beakers were accompanied by small daggers of West European type.¹⁶ But these blades have generally been so reduced by frequent resharpening that they are small in comparison with the English and Irish specimens. The Early Bronze Age in the British Isles is more likely to have been parallel to, than dependent on, the Copper Age Beaker period of Central Europe.

That probability is converted into a certainty when in the first typological period of the Continental Bronze Age (Montelius' I) actual imports follow of accurate correlations with Britain. During Montelius' I the dominant metal industry east of the Rhine was that of the Bohemian Aunjetitz culture. Its metal technique was certainly of southeastern origin, associated indeed with that southeastern culture that transformed the Beaker culture of Central Europe.¹⁷ But even in central Germany imported British bronzes such as halberds and axes are associated with those of Aunjetitz type both in graves and hoards.¹⁸ Conversely a bulb-head bronze pin of Aunjetitz type, almost certainly imported from Central Europe, was found in a grave at Camerton, Somersetshire, associated with an ogival dagger of our group Ma and a pigmy vessel of group C (fig. 3; plate 1).¹⁹ In north Germany and

¹⁴ Gotthard Neumann, *Entwicklung der Aunjetitzer Keramik in Mitteldeutschland* (Præhistorische Zeitschrift, Vol. 20, 1929), p. 128.

¹⁵ Eduard Beninger, *Frühbronzezeitliche Stabdolche aus Niederösterreich* (Præhistorische Zeitschrift, Vol. 25, 1934), p. 137.

¹⁶ V. Gordon Childe, *The Danube in Prehistory* (Oxford, 1929), p. 190. Cf. also A. E. Van Grien, *Die Bauart der Einzelgräber* (Leipzig, 1930), Pls. 110b and 112.

¹⁷ See Childe, *Danube in Prehistory*, pp. 223 ff., and for fresh confirmatory evidence from Hungary, Janos Banner, *A Marosidek bronzkor: szagorott temetéseinek sirmeléklete* (Dolgozatok a M. kir. Ferencz József-tudományegyetem archaeológiai intézetéből, Vol. 7, 1931) pp. 1-50.

¹⁸ Childe, *op. cit.*, p. 242, cf. also Ernst Sprockhoff, *Eine Bronzeclasse von Münchberg auf Sachsen* (Acta Archaeologica, Vol. 4, 1933), p. 43. Note too that stone "arrow-straighteners" occur in Central Europe even before the Beaker period (Památky archaeologické, Vol. 39, 1933, pp. 50 ff.) but in England are associated with ogival daggers (Newall, *Barrow 85*, *Antiquary*, p. 450).

¹⁹ J. Thurnam, *On Ancient British Barrows* (Archæologia, Vol. 43, 1871), p. 363. Cf.

Scandinavia. British types are even more prominent, and in addition to axes and halberds we find unmistakably Irish gold lunulae.

But British and other metal types of this period are hardly ever found in graves north of the Harz and Spree, but occur stray or in hoards. The contemporary graves are still "Neolithic" if judged by the pottery and stone implements they usually contain. But the contemporaneity of such graves with the hoards is well established. In Denmark and Sweden bone copies of various types of Aunjetitz bronze pin occur in the later passage graves and stone cists²⁰ belonging to Montelius' Neolithic. Massive bronze rings have been found in graves with S-beakers at Ohlenburg, near Hamburg, and with Bernberg vases at Schottin-Molckenberg, and at Burg a ring of bronze wire was found with a cup of the Walternienberg style that is supposed to precede the Bernberg phase. A British flat axe of bronze was found with a Nordic thick-butted flint axe at Deutsch-Sagar, Brandenburg,²¹ and even a palstaf is said to have been found with an S-beaker near Zeven, Hanover.²²

Now all the grave-forms and pot-types just enumerated are reputedly typical of the Neolithic period in Scandinavia and north Germany. Yet they were contemporary with hoards of imported bronzes which the northern barbarians were too poor or too ill-organized economically to afford as offerings to their departed chiefs. But the British bronzes from the north German hoards are not of the simplest forms, but such as belong already to our phase II. a. Hence the British bronze industry had reached a mature stage of development while North Europe was still formally and economically Neolithic and while the Aunjetitz culture was growing up in the valleys of the March and Elbe.

The above view of the relation of the British bronze industry to the Central European has been strikingly confirmed by a find assigned to the next typological period (Montelius' II) of the North European Bronze Age. This is the first period when all peoples of the North began to make bronzes for themselves and deposit them in their chiefs' graves.²³ But they continued to import bronzes from the British Isles. A grave at Liesbüttel

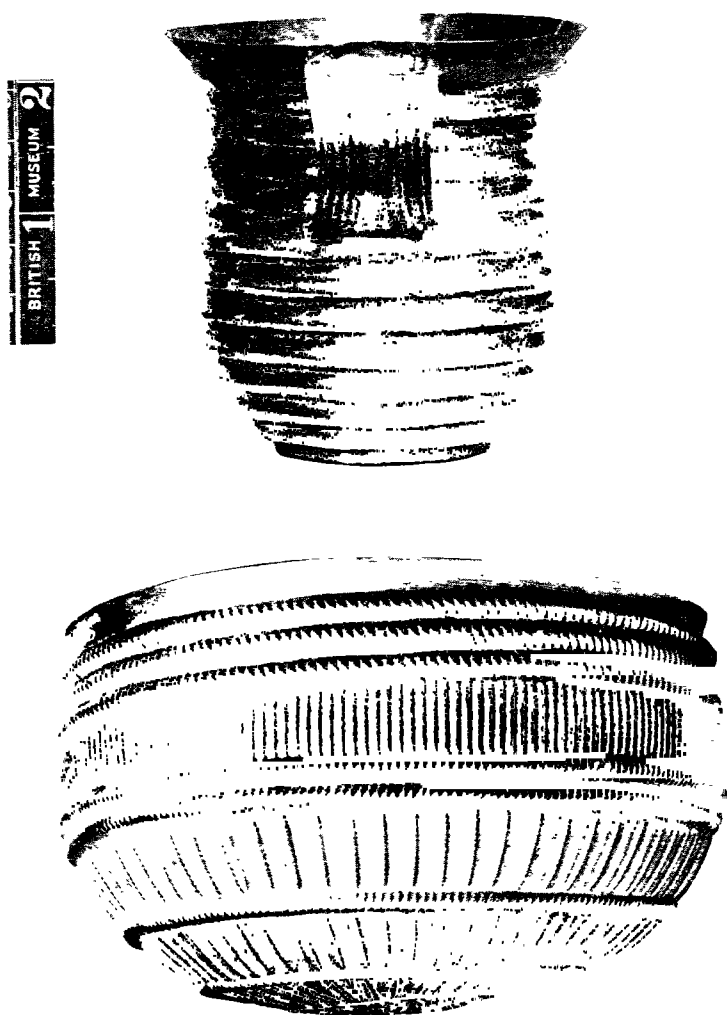
Chible, *Der Steinzeit-Praktory*, p. 250. A similar pin was found in a Danish passage-grave with objects of Montelius' Neolithic IV (Nordmann, *The Mesolithic Culture*, p. 116).

²⁰ Nordmann, *The Mesolithic Culture*, pp. 116-20. H. Håkansson, *Götlarö. Bronsåldern* (Stockholm, 1927), p. 14. Forssander, *Der ostbaltische und der Norden*, p. 100.

²¹ Bohm, *Bronzezeit March-Brandenburg*, pp. 7, 22. Schwantes, *Gelehrte Schleswig-Holstein*, p. 292.

²² Kuhn, Stumpfius, *Die jungneolithischen Kulturen in W. Deutschland* (Bonn, 1929), pp. 56, 125.

²³ For this footnote see page 22.



Upper: Gold beaker found with an iron dagger in a grave at Rillaton, Cornwall (British Museum). Lower: Gold vase from a grave at Gonnereck, Holstein (Schleswig-Holsteinisches Museum, vorgeschichtlicher Altertümer-Kreis) (Scale 2:5).

in Holstein²³ contained a flint dagger, a Nordic bronze hilted dirk, and a British spear-head with basal loops (plate 2). Typologically the daggers belong to an early phase of Montelius' II. But the imported British spear-head is proper to our group III as Estyn Evans²⁴ has proved. In other words, the initial period of the full Bronze Age in the north overlaps with the late Bronze Age of Britain! Incidentally, a similar British spear-head from an urn-grave with a Rixheim rapier and a *Cylinderhalsurne* from Wiesloch, Baden,²⁵ shows that the British Late Bronze Age had begun by the second phase of the Central European Late Bronze Age—Reinecke's Hallstatt A or even the end of his Bronze Age D.

Another aspect of the influence of British metal-work on the north European has been recognized by Menghin.²⁶ The thin Nordic gold vases from Gönnebek (Holstein) (plate 3: lower), Langendorf (Pomerania), and Eberswald, near Berlin, faithfully reproduce the form and decoration of North Irish and Argyllshire food vessels. Presumably the translation into gold of the clay or wooden vessels took place first in the gold-producing region. No such gold vessels indeed have hitherto been unearthed in Ireland, but the famous gold vase (plate 3: upper) found with an ogival dagger at Rillaton, Cornwall, illustrates British goldsmiths' skill during our phase II. a. The Nordic specimens belong to Montelius' III. It need not, of course, follow that the Food Vessel period in Ireland lasted into the Late Bronze Age, though some recent finds²⁷ make that quite possible. In any case it is only late in Britain's period III or more probably IV that influences from the Nordic province become perceptible in Scotland and Ireland. Then sunflower and cup-headed pins that turn up in hoards of the Highland group 3, b must be accepted as derivatives of Scandinavian models.

The chronological relations subsisting between the British Isles and North Europe during the Bronze Age are thus reasonably clear. And they are perfectly concordant with the view that the North European (Nordic) Stone Age ran parallel to a large extent with the British Bronze Age. With

²³ Ernst Sprockhoff, *Zur Schaftung bronzeeedlicher Lanzenespitzen* (Mainzer Zeitschrift Vol. 29, 1934), p. 56.

²⁴ E. Estyn Evans, *The Bronze Spearhead in Great Britain and Ireland* (Archaeologia, Vol. 83, 1933), p. 196, establishes the Late Bronze Age date of the type here.

²⁵ Sprockhoff, *op. cit.*, p. 58.

²⁶ O. Menghin, *Ursprung und Entwicklung der germanischen Goldgefäße der Bronzezeit* (Altshlesien, Vol. 5, 1934), p. 188. The gold sun-disc from Glusing, Norddithmarschen, is also cited as an import from Ireland (Jahrbuch für prähistorische und ethnographische Kunst, 1931, p. 36).

²⁷ E.g., H. O'Neil Hencken, *The Cemetery Cairn of Knockast* (Proceedings, Royal Irish Academy, Vol. 41, 1933), pp. 240 ff.

this demonstration the relation of the Megalithic cultures of Britain to the Nordic can be correctly estimated. The corbelled Irish passage graves of the type of New Grange, Slieve na Caillighe (Lough Crew), and Carrowkeel Mountain are generally termed "Bronze Age" because some have yielded food vessels and stone beads of a type found in Bronze Age round barrows in Wiltshire, though as I have recently shown²⁸ some have also yielded sherds allied to the north Scottish Unstan ware that is supposed to be "Neolithic," i.e., pre-Beaker.

But the admission of a "Bronze Age" date for the Irish corbelled tombs would in no sense prejudice the contention of Montelius,²⁹ restated with fresh evidence by Nordmann,³⁰ that the Danish Megalithic passage graves are rather clumsy copies of such corbelled tombs. And additional evidence pointing in that direction has come to light since 1930. While the primary burials in the Danish passage graves are accompanied by a purely Neolithic furniture, a vase of the very earliest Passage Grave style was associated at Bygholm in Jutland with flat axes, a dagger with a midrib on one face, and arm-cylinders of copper.³¹ In other words the whole of the Danish Passage Grave period is contemporary with a Metal Age in some region not too remote from Scandinavia. Indeed even the Danish dolmens may contain stone axes that patently copy metal models.³²

There is thus no sort of objection on chronological grounds to regarding the Irish corbelled tombs as typologically senior to the Nordic passage graves. Similarly the more degenerate segmented cists from southwest Scotland with the entrance closed by a septal stone only half the height of the chamber would provide admirable typological prototypes for the small Danish dolmens that stand at the head of the Nordic Megalithic sequence. From these well-founded architectural comparisons it is possible to proceed to a correct estimation of the position of British and Irish Megalithic culture in Atlantic Europe, which has been sadly misapprehended by Bosch-Gimpera.

The latter³³ has forced the material from the Iberian Peninsula into the

²⁸ V. G. Chubb, *Some Sherds from Slieve na Caillighe* (Proceedings, Society of Antiquaries of Ireland, Vol. 65, 1935), p. 320.

²⁹ *Der Orient und Europa* (Stockholm, 1889).

³⁰ C. A. Nordmann, *Studier over Gangræftkulturen i Danmark* (Aarbøger, 1917).

³¹ Nordmann, *The Megalithic Culture*, p. 129, for the chronological significance of the find see also P. Reinecke, *Ein Kupferfund aus der Dolmenzeit* (Mainzer Zeitschrift, Vol. 24-25, 1929-30), p. 58, and Forssander, *Der ostskandinavische Norden*, p. 53.

³² Nordmann, *The Megalithic Culture*, p. 132.

³³ P. Bosch-Gimpera, *Le Néo-éolithique en Europe occidentale* (Revue anthropologique, Vol. 40, 1930), pp. 244-53, *Relations préhistoriques entre l'Irlande et l'Ouest de la Péninsule ibérique* (Préhistoire, Vol. 2, 1933), pp. 198 ff.

typological framework outlined by Montelius for the North European Stone Age, at the same time transferring the Iberian corbelled tombs of the type of Alcalá and Los Millares to the latest of Montelius' phases. We thus have in Iberia the sequence shown in Table 2.

TABLE 2

<i>Iberia</i>	<i>Scandinavia</i>
Advanced Neolithic	= Dolmen period (Montelius' Neolithic II)
Initial and final Eneolithic	= Passage Grave period (Montelius' Neolithic III)
Bronze Age I	= Stone Cist period (Montelius' Neolithic IV)

To the last of these phases would belong the corbelled passage graves of Los Millares and Alcalá, and consequently the "beginning of the Irish megalithic culture (Mount Field and Lough Crew)." Now Forde has convincingly shown by a study both of the grave-goods and of the tomb-distributions how the foregoing sequence is an inversion of the truth.

... The great galleries and cupolas of Algarve, Huelva, Andalucía and Almería, the rock-cut tombs of Palmella, and the passage dolmens of central Portugal and western Spain are virtually contemporaneous. The smaller passage dolmens have a poorer but not earlier furniture: they represent a . . . provincial degradation typical of peripheral areas.³⁴

The Bygholm find comes to complete Forde's argument: for this hoard, standing at the very transition from *néolithique avancé* to *énéolithique initiale*, included a copper dagger, the nearest parallels to which come from Alcalá in Algarve and Los Millares in Almería! If then, as seems likely, in spite of arguments to the contrary advanced by Forssander and Reinecke, this imported dagger really comes from the Peninsula, the thesis that the Danish passage graves are derived from corbelled tombs like those of Alcalá and Los Millares becomes still more plausible.

Now geographically at least the corbelled Irish tombs stand midway between the Danish passage graves and the cupolas of Iberia. They should therefore be at least as old as the former. The application of the term Bronze Age to the Iberian and Irish tombs is only confusing if it be assumed to have the same chronological significance in metalliferous regions, like Algarve or Ireland, and in regions remote from all supplies of ore, like Denmark. It is correct in so far as it implies that the exploitation of the Irish copper and Cornish tin begins in the period covered by corbelled tombs of the Lough Crew type, even though such tombs have yielded less metal than the Danish passage graves.

³⁴ *Early Cultures of Atlantic Europe* (American Anthropologist, Vol. 32, 1930), p. 36. His expression as quoted is perhaps an understatement: as the cupolas of Alcalá contain no Beaker ware, they should be either earlier (on Forde's view) or later (on Bosch-Gimpera's) than the "dolmens" that do contain such; cf. p. 16 below.

To complete the account of British Megalithic culture we must turn to another series of tombs, ignored both by Forde and by Bosch-Gimpera, but already mentioned as possible prototypes for the Danish dolmens. These are the segmented cists of southwestern Scotland, North Ireland, and Man. Their southern analogues are not to be found in Portugal or southern Spain, but, as Bryce pointed out nearly forty years ago, in the Giants' Graves of Sardinia. Geographically intermediate tombs have subsequently been recognized in Haute Pyrenées (La Halliade), the Basque provinces, and Catalonia.³⁵ All these tombs have yielded Beaker ware, La Halliade also an ancestral food vessel.³⁶ But it is not certain that such pottery represents the primary furniture in the Pyrenean tombs any more than in the Scottish cists. In the caves of southern France Heléna has found pottery decorated very much in the style of the Beacharra ware that is primary in Scotland.³⁷ And this decorated ware occurs stratified *below* Beaker ware, but *above* a simple Western pottery, comparable to the English Windmill Hill ware.

There is therefore a remarkably close parallelism both in tomb types and in pottery between Britain and Ireland on the one hand and Iberia and the South of France on the other. Though no strict synchronism can be asserted, the same cultural sequence is applicable to both areas. In this sequence Brittany should occupy at best an intermediate position. In point of fact, the majority of the extremely numerous and varied megalithic tombs concentrated on the coasts of Morbihan and Finistère have yielded Beaker ware,³⁸ a few only a sort of Beacharra ware (stratified below beakers at Conguel, Quiberon);³⁹ undecorated Western pots, comparable to English Windmill Hill vases, occur in small cists that are non-megalithic and supposedly pre-megalithic.⁴⁰ Several megalithic tombs have yielded examples of the Chassey pottery that is closely allied to certain pigmy vessels (e.g., plate 1 here) and grape cups appearing in Wiltshire with bronzes of our group II, a,⁴¹ one a segmented paste bead similar to those found in graves

³⁵ Luis Pericot y García, *La Civilización megalítica catalana* (Barcelona, 1925), pp. 117, 127.

³⁶ As noted by W. L. Scott, *The Mediterranean Relations of Certain Megalithic Tombs on the West Coast of Britain* (Proceedings, First Congress of Prehistoric and Protohistoric Sciences, London, 1932), p. 134.

³⁷ Jaquetta Hawkes, *Aspects of the Neolithic and Chalcolithic Periods in Western Europe* (Antiquity, Vol. 8, 1935), p. 29.

³⁸ Forde, *Early Cultures of Atlantic Europe*, p. 82.

³⁹ Z. le Rouzic, *Morphologie et chronologie des Sépultures préhistoriques du Morbihan* (L'Anthropologie, Vol. 43, 1933), p. 248; Childe, *The Continental Affinities of British Neolithic Pottery* (Archaeological Journal, Vol. 88, 1931), Pls. 5-6.

⁴⁰ Z. le Rouzic, *Le Mobilier des Sépultures préhistoriques du Morbihan* (L'Anthropologie, Vol. 44, 1934), p. 487.

⁴¹ Childe, *Continental Affinities*, p. 52.

of group C in England, and another a gold sceptre-mount allied to a bone one found in Bush Barrow, Wiltshire, with ogival daggers.⁴²

At best then, cultural development in Brittany ran parallel to that in the British Isles. It was not at the end of a long "eneolithic" development that Breton colonists crossed over into Ireland, as Bosch-Gimpera⁴³ has asserted. On the contrary, all along the Atlantic seaboard from Orkney to Algarve, Megalithic civilization seems to develop along parallel lines. In this development southern Iberia and southern France were naturally in advance of other regions. And British metallurgy was presumably derived from Almeria and Algarve, or Sardinia. We cannot claim as British or Irish the West European daggers from intermediate regions—the Pyrenées, south France, Vendée, Brittany, or the Channel Islands. They are more likely Iberian products.

Nevertheless, the rich burials in closed chambers from the centre and north of the Armorican peninsula may contain products of British industry. The halberds and axes may of course be Iberian, but two tombs contained ogival daggers the lost hilts of which had been studded with tiny gold nails, a device recurring on two British daggers.⁴⁴ Moreover, the normal pottery from these tombs is a handled urn similar to the Cornish urns of our group C, 3. The Armorican burials would then belong to our phases II, a and C-D in Lowland Britain. But still they are partly contemporary with some coastal megaliths, since the two types of tomb exhibit a mutually exclusive distribution.⁴⁵ Brittany would then be receiving British bronzes—to which may be added an Irish lunula from the north—already during the Megalithic period, for which greenstone celts found in Britain would seem but a poor and barbaric recompense.

In north France a Neolithic economy would seem to have persisted exceptionally long. The occurrence in Normandy and Belgium of Irish gold lunulae suffices to indicate that such stray bronzes as reached this region during the earlier part of the Bronze Age included British exports. But in the Late Bronze Age distinctively British products like tongued chapes, spear-heads with basal and protected loops, socketed double-edged knives, and socketed sickles are so numerous that Breuil⁴⁶ could speak of a

⁴² Le Rouzic, *Le Mobilier*, p. 307. Cf. Devizes Museum Catalogue, Vol. 1, Nos. 152, 161. Note also the occurrence of Grape Cups and Cinerary Urns in the megalithic tombs of the Channel Islands (T. D. Kendrick, *Archæology of the Channel Islands*, London, 1928, p. 89).

⁴³ P. Bosch-Gimpera, *Relations préhistoriques*, p. 220.

⁴⁴ Le Rouzic, *Le Mobilier*, pp. 512 ff.

⁴⁵ Le Rouzic, *Morphologie et chronologie*, p. 254; C. Daryll Foulds, *The Megalithic Monuments of Southern Finistère* (*Antiquaries Journal*, Vol. 7, 1927), p. 21.

⁴⁶ Henri Breuil, *L'Âge du bronze dans le Bassin de Paris* (*L'Anthropologie*, Vol. 14, 1903), p. 517.

Brittanico-Sequanian bronze culture. The great region around the Seine basin, from Normandy to Holland, was in fact an open market for the British metal industry. The extent of this British market in France is graphically indicated by the distribution of gold-plated bronze imitations of the penannular hollow ornaments of triangular section made in Britain of solid gold⁴⁷ (fig. 4).



FIG. 4 Distribution of penannular gold ornaments of triangular cross-section (based on a map by the Abbé Favret with additions).

The later British bronzes were exported even farther afield. While double-looped Iberian or Sardinian palstavs were occasionally imported into Britain⁴⁸ by the Atlantic route late in the Middle Bronze Age, a perfectly typical British spear-head with lunate openings in the blade was

⁴⁷ Abbé Favret, *Les ornements pénnulaires creux de section triangulaire* (Revue archéologique, Vol 28, 1928), pp 16-33

⁴⁸ O. G. S. Crawford in Proceedings, Society of Antiquaries of London, Vol 25, 1912, p. 43.

dredged up from Huelva harbor in south Spain.⁴⁹ This find perhaps gives an absolute date for one phase of the British Bronze Age. For this Late Bronze Age spear-head was brought to light together with other bronze objects, presumed to have formed the cargo of a sunken merchantman. Its cargo, if this assumption be correct, comprised also Sicilian fibulae of a type which had gone out of fashion before the Greeks colonized Syracuse about 750 B.C. Period IV in Britain would therefore have begun before that date.

Indeed, if we be right in assuming that the Late Bronze Age did not begin in Britain appreciably later than on the Continent, period III may go back nearly to 1200 B.C., that being the limiting date for the emergence of slashing swords in Central Europe given by the intrusion of such swords into Greece and Egypt.

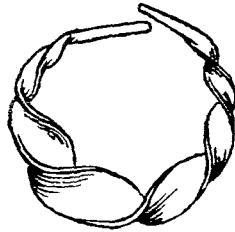


FIG. 5. Gold torque earring from Gaza (after Sir Flinders Petrie) (Natural size)

No reliable date for the beginning of the British Bronze Age can be given. My former estimate of 1800 B.C. was based on a chronology for the Central European Aunjetitz culture. Of recent years a substantially lower date—1600 B.C.—has been accepted by Reinecke, Åberg, and Tallgren.⁵⁰ On the other hand, the specialized Asiatic-Aegean types that, occurring in Aunjetitz graves, are supposed to provide upper limits for the beginning of the period, have now been found in the Orient much earlier than had formerly been expected. At Ur and Kish the relevant pins, earrings, and lockrings appear before 2500 B.C.;⁵¹ at Ahlatlibel in Anatolia ingot torques go back before 2000 B.C.⁵² The possible date for Aunjetitz has thereby been

⁴⁹ "Huelva" in Ebert's *Reallexikon der Vorgeschichte* Evans, *The Bronze Spearhead*, p. 199, shows that this type belongs to an advanced phase of our Late Bronze Age

⁵⁰ P. Reinecke in *Germania*, Vol. 17, 1933, p. 12; N. Åberg, *Chronologie*, Vols 3 and 5, A. M. Tallgren, *Zur Chronologie der osteuropäischen Bronzezeit* (Mitteilungen der anthropologischen Gesellschaft in Wien, Vol. 61, 1931), p. 96

⁵¹ V. Gordon Childe, *New Light on the Most Ancient East* (London, 1935); *Die Bedeutung der frühsumerischen Metalltypen für die bronzezeitliche Chronologie Europas* (Mitteilungen der anthropologischen Gesellschaft in Wien, Vol. 63, 1933), p. 221.

⁵² Hamit Kosay, *Ahlatlibel Hafriyatı* (Turk Tarih Arkeologiya ve Etnografya Dergisi, Vol. 2, 1934), p. 90. Flanged axes were found here as at Thermi in Lesbos, likewise in the third millennium B.C. (W. Lamb, *Excavations at Thermi*, Cambridge, 1936, p. 169)

raised towards Montelius' old estimate of 2100 B.C. But that is only a possibility: all the "limiting" types survive in the Caucasus at least down to

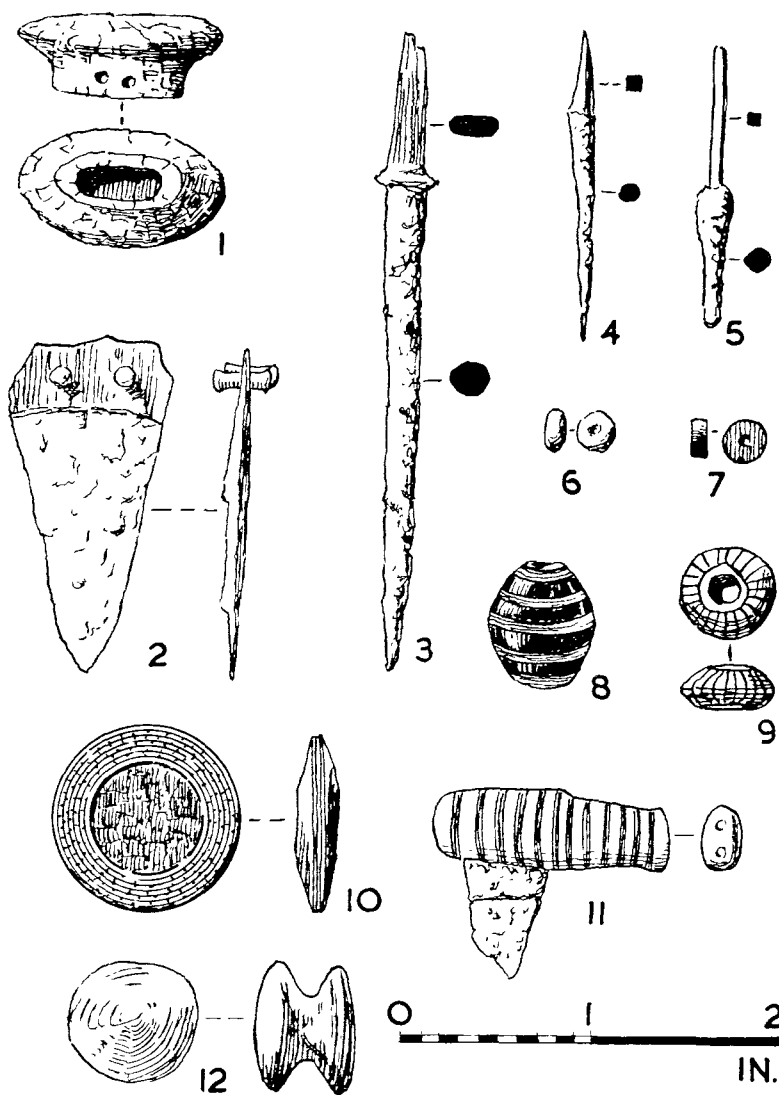


FIG. 6. Objects from a grave at Manton, Wilts. (in the possession of Dr Maurice, Marlborough, kindly drawn by Mr Stuart Piggott) 1, Amber dagger-pommel, 2, Bronze dagger; 3-5, Bronze awls, 6, 7, Specimens of amber and jet beads; 8, Jet bead with gold caps and bands, 9, Jet bead, 10, Gold-mounted amber disc, 11, Bronze blade in gold haft; 12, Pottery toggle.

1200 B.C., and there is no certain clue to decide at what point in the preceding thousand years they reached Bohemia.

In any case, as remarked above, Aunjetitz can no longer be regarded as providing a limit for the British Bronze Age. For that we should seek, rather, links with the Aegean by western seaways. Armlets of composite twisted ribbon from Troy II and an Early Helladic grave on Levkas (before 1800 B.C.) seem significantly like the Yeovil torques of Middle Bronze Age II in Britain. Earrings of similarly constructed ribbon from Enkomi (14th century) and Gaza (15th or even 17th century)⁵³ (fig. 5) are no less probably connected with similar Irish earrings. An amber bead,

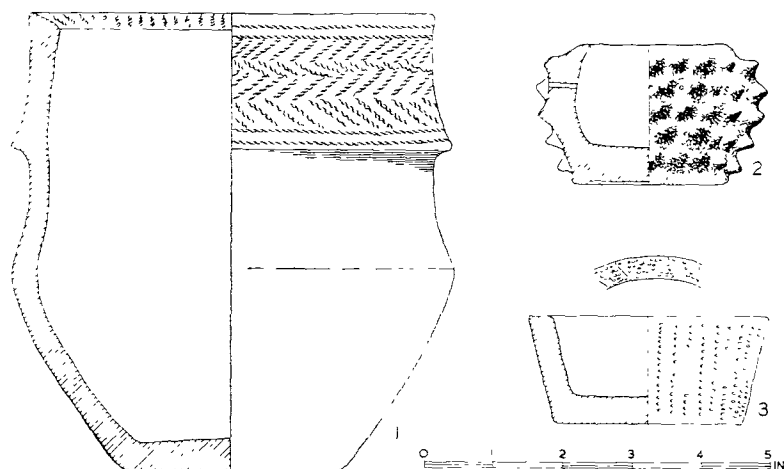


FIG. 7. Objects from the same barrow as Figure 6 (in the possession of Dr Maurice Lindly drawn by Mr Stuart Piggott) 1. Overhanging rim urn found nine feet south of skeleton, 2. Grape cup found with skeleton; 3. Pigmy vessel (the ornament on top of the rim has white inlay)

mounted in gold (fig. 6: 10), found with a grape cup (fig. 7: 2) at Manton, Wiltshire, is exactly like a gold-mounted amber bead from a Late Minoan II tomb at Knossos in Crete.⁵⁴ Segmented beads of bluish vitreous material, really similar to those found in graves of group C in southern England, occur in Egypt about 1400 B.C.⁵⁵ Such analogies do indicate maritime inter-

⁵³ W. M. Flinders Petrie, *Ancient Gaza*, Vol. 2, p. 24; Vol. 3, p. 8 (attributed to Dynasties XII–XV which Petrie dates too high).

⁵⁴ M. E. Cunningham, *Prehistoric Gold in Wilts* (Antiquaries Journal, Vol. 5, 1925, p. 69). Cf. Arthur Evans, *The Tomb of the Double Axes and Associated Group at Knossos* (Archaeologia, Vol. 65, 1913–14), p. 42.

⁵⁵ On these see H. C. Beck and J. F. Stone, *Faience Beads of the British Bronze Age* (Archaeologia, Vol. 85, 1936).

course between Bronze Age Britain and the east Mediterranean during the second millennium B.C., but are insufficient for establishing accurate synchronisms.

More definite conclusions might be drawn from the well-known comparison between the tholos tombs of Mycenæ and the corbelled tombs of Los Millares, Antequera, Alcalá, Lough Crew, and Camster. If detailed architectural agreements and a relatively continuous distribution, unsupported by association with actually imported objects,⁵⁶ be admitted as evidence of intercourse, the above series must be accepted as exceptionally impressive and convincing. But the Mycenæan tholoi do not antedate 1600 B.C.⁵⁷ Can the beginnings of the British Bronze Age and the Danish passage graves be brought down so low? The British record is not incompatible with such a curtailment of the Bronze Age.

Whatever the absolute antiquity of the British Bronze Age and its relation to Crete, Egypt, and Sumer, it is abundantly clear that during the period the British Isles scarcely lagged behind the Iberian peninsula and far outstripped the rest of northwestern Europe in industrial progress. For the benefit of those who believe in the racial interpretation of history, it may be well to add, it was neither Nordics nor Beaker-folk,⁵⁸ but Irish proletarians of unidentified physical type who thus asserted the industrial supremacy of Bronze Age Britain.⁵⁹

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⁵⁶ Of course amber beads occur in Mycenæan tholoi and a halberd in Shaft Grave VI which Evans (*Palace of Minos*, London, 1921-35, Vol. 2, p. 173) regards as contemporary with the oldest tholoi.

⁵⁷ For the chronology see A. J. B. Wace, *Excavations at Mycenæ* (Annual of British School at Athens, Vol. 25, 1921-23), pp. 283 ff., and Axel W. Persson, *The Royal Tombs of Dendra near Midea* (Lund, 1931). The Early Minoan vaulted tombs of the Mesará which have been invoked as prototypes for the Iberian cupolas lack the entrance passage and lateral cell which make the comparison with Mycenæan tholoi so impressive.

⁵⁸ Arthur Keith (*The Bronze Age Invaders of Britain*, Journal, Royal Anthropological Institute, Vol. 45, 1915, p. 16) claims that twenty percent of the British governing class belong to the Beaker type.

⁵⁹ I have to thank the Director of the Bristol Museum, the Trustees of the British Museum, the Director of the Schleswig-Holsteinisches Museum vorgeschichtlicher Altertümer, and Dr Maurice for permission to illustrate here objects in their care.

^{22a} A local bronze industry was already beginning towards the close of Montelius' I (= Neolithic IV in Denmark and Sweden) in Schleswig-Holstein and perhaps even south Sweden, but "in Zone I (Denmark and Scandinavia) the Bronze Age proper begins with Period IIIa" (Karl Kersten, *Zur älteren nordischen Bronzezeit*, Neumünster, n.d., p. 101).

AN OUTLINE OF THEORIES CONCERN- ING THE PREHISTORIC PEOPLE OF JAPAN

B. HIDEO NISHIOKA AND
W. EGBERT SCHENCK

The basis of this paper is an article written by Mr Hideo Nishioka of the Nishioka Anthropological Laboratory at Denenchofu, near Tokyo, in March 1936. After reading the manuscript and consulting Mr Nishioka, I prepared the form which appears below. Mr Nishioka makes no attempt to evaluate the theories. I have not tried to check his statements. The value of such an outline would appear to lie in the starting points it may furnish for further research by interested scholars. Hence our effort has been to list clearly the Japanese authors and books, and to give an indication of the views expressed. Less emphasis has been placed on foreign authors, which are presumed to be already known to the American reader.

Mr Nishioka makes especial acknowledgement to Dr Kenji Kiyono of Kyoto Imperial University (1).¹

I have used the term "Prehistoric" where Mr Nishioka uses "Stone Age People of Japan." The Stone Age of northern Japan was contemporaneous with historic southern Japan --
W. E. S.

PRIMITIVE THEORIES

STONE Age remains have been known and speculated about in Japan for over a thousand years (2). Primitive theories attributed them to giants, the thunder god, heavenly armies, the long-nosed goblin, the fox, that is to supernatural agencies. Other early theories were based more reasonably upon ancient literature and supposed the old stone implements to have been made and used by the ancestors of the Japanese race in the age of the gods (3). Hakuseki Arai, as early as 1725, foreshadowed later views when he claimed, in a famous letter, that the prehistoric arrowheads had been made by the Shukushin race then living in Manchuria. A further group of opinions was founded on observation of local customs and assigned prehistoric remains to the Ezo or Ebisu (old names of the Ainu). Gentatsu Matsuo (4), Kiuchi Sekitei (5), Ph. Fr. von Siebold (6), and Hikomaro Saito (7) were prominent among the many supporters of this Ainu theory which by 1877 had become widely accepted.

It will be seen that the views expressed prior to 1877 were superstitions, or, at best, the speculations of armchair scholars. From that date to 1912 some field work was done, but the conclusions arrived at were still largely a matter of historical interpretation, philosophy, and dialectics. Since 1912 material has been more extensively and scientifically collected, and biological and biometric methods have been applied. However, relatively few results of this scientific approach have been published.

(In considering all these theories of origin it should be borne in mind that this is not a topic for free speculation in Japan, that many sub-con-

¹ References are to the similarly numbered items in the terminal bibliography.

scious as well as conscious obstacles must be overcome if the scholar is to follow logic and data.

SYNOPSIS OF LATER THEORIES

In Japan one line of speculation has kept the prehistoric people primarily in view; another line, the Japanese people and their origin. Hence the resultant theories tend to fall into two groups, and may be conveniently arranged and discussed under the following heads.

A. Theories that the prehistoric people were not Japanese: I. Koropok-guru theory; II. Ainu theory; III. Ainu-Japanese confronting theory; IV. Eclectic theory.

B. Theories that the prehistoric people were Japanese.

A, I KOROPOK-GURU THEORY

Edward S. Morse by actual excavations in the Omori shellmound (Tokyo) established the existence in Japan of Stone Age sites. In 1878 (8) he asserted that such sites were pre-Ainu because the shellmound people used pottery and were cannibalistic. About the same time H. von Siebold (9) published his opinion that the Ainu were the prehistoric inhabitants of Japan.

Thus began a fierce controversy. The outstanding spokesman of the school which believed that prehistoric man in Japan was not Ainu was Dr Shogoro Tsuboi, whose opinion became known as the Koropok-guru theory. (The theory would appear to have been first enunciated by Shozaburo Hirose in 1886 [10]. The name came from an Ainu word meaning "the man under the rhubarb," i.e. a small person. The Ainu legend concerning the existence of such a people seems to have been first reported by Milne in 1882 [11].)

Tsuboi claimed that the prehistoric people of Japan were Koropok-guru; and that the Koropok-guru were racially connected with the Eskimo (12). To support his first contention he advanced the following points: the prehistoric people of Japan had many decayed teeth; the shape of their bones differs from those of the Ainu; physical characteristics indicated by clay figures from prehistoric sites are not those of the Ainu; food, clothing, and housing habits suggested by prehistoric sites are unlike those of the Ainu; many local names exist which are not explained by either the Japanese or the Ainu language; the Ainu had an oral tradition concerning the existence of an alien race called the Koropok-guru having manners akin to those deduced for prehistoric man in Japan.

To support his second contention, Dr Tsuboi cites these points: the

small physique and practice of tattooing of the Koropok-guru of Ainu legend; perforated lips; use of light filters or eye protectors; wearing of overcoats and drawers; making of stone implements and pots; living in pit dwellings; and others to make a total of nineteen points of similarity.

The theory was presented in detail by the Tokyo Imperial University in 1897-98 (13). After Tsuboi's death in 1913 it gradually declined. However, great credit should be given him because his theory suggests that the prehistoric people of Japan were an independent race (i.e. neither Japanese or Ainu), and because efforts to refute his views have caused a more careful preservation and study of prehistoric remains than might otherwise have been expected.

Summary

Edward S. Morse	1878 (8)	See text above.
John Milne	1882 (11)	Reports Ainu legend of Koropok-guru.
Shozaburo Hirose	1886 (10)	First enunciates Koropok-guru theory.
Shogoro Tsuboi	1886-1903 (12)	Exponent of theory.
Tokyo Imperial University	1897-98 (13)	Presents theory in detail.

A. II. AINU THEORY

Most of the scholars who opposed the Koropok-guru theory did so because they believed the Ainu to have been the prehistoric inhabitants of Japan. From a modern point of view, all of them lacked sufficient data to really refute the Tsuboi theory. None the less the Ainu view became predominant.

As has been stated, Siebold in 1879 (9) was the first Ainu-ist. Drs Ryosei Koganei (15) and Neil Gordon Munro (24) were probably the most famous advocates of the opinion. Their views, together with those of some of the scholars who supported them, are suggested in the summary below. More work was done by these men than the summary may make clear, but they were pathfinders rather than builders with adequate material. A basic weakness of the Koganei type of argument seems to lie in the assumption that if it can be shown that the prehistoric people of Japan were not Koropok-guru, it is proven that they were Ainu. This leaves out all possibility of some other race. For example, when Dr Montaro Adachi (23) compared the cephalic index of seven skulls from the Minamisawa and Yoyama shellmounds (79.9) with that of the Eskimo (76), he seems to show different races. But the difference between the shellmound index and that of the Ainu (77) is also great enough to suggest different races.

Summary

Von Siebold	1879 (9)	First advocate of Ainu theory
Mitsutaro Shirai	1886 (14)	Prehistoric pottery design-figures and figurines and prehistoric man's flat tibia similar to Ainu
Ryosei Koganei	1889 (15)	Assigns prehistoric sites to Ainu after studying Ainu.
	1890 (15)	After limb bone comparisons assigns sites to Ainu.
	1903 (15)	Sums up against Tsuboi theory. Common flatness of tibia proves only that prehistoric man was not smaller than modern Ainu. Believes:
		<div style="display: flex; justify-content: space-around; align-items: center;"> <div style="text-align: center;"> Ancestral Ainu \ </div> <div style="text-align: center;"> Present Ainu / </div> </div>
	1917 (15)	Found the same characteristics in Kofu (not shell-mound) prehistoric men and the Ainu.
Sho Yamanaka	1889 (16)	Assigns prehistoric sites to Ainu on basis of designs and place names.
Shitomu Sato	1889 (17)	Uses difference in pottery types to refute Tsuboi. Finger prints left on pottery show prehistoric man not pygmy, hence not Koropok-guru.
B. Laufer	1900 (18)	Affirms the first inhabitants of Karafuto (Sakhalin) were Ainu.
Zabrowski	1901 (19)	Ditto.
Ryozo Torii	1901 (20)	Reports inhabitants of northern Chishima (Kuriles) different from southern Chishima. General findings unfavorable to Tsuboi.
Kosaku Hamada	1902 (21)	Koropok-guru existence not deducible from place names. If prehistoric man not Ainu, ancestors of the Ainu a mystery, which is unreasonable in view of the many known Ainu sites.
	1903 (21)	Designs from northern Chishima fill gap between prehistoric man and modern Ainu.
	1904 (21)	Ancient Ainu used stone arrowheads
	1906 (21)	Ainu arrowhead types argue against Tsuboi.
Denzo Sato and J. Batchelor	1902 (22)	Koropok-guru were the Ainu themselves.
Montaro Adachi	1907 (23)	Compares cephalic index of shellmound people, Ainu, and Eskimo.
Neil Gordon Munro	1907 (24)	Compares shellmound crania with Ainu and concludes they are same stock.
Jochelson	1908 (25)	Koropok-guru were the ancestors of the Ainu.
Tsunekichi Kōno	1907 (26)	Shows "mistakes" of Tsuboi theory
Kenji Takahashi	1913 (27)	Prehistoric man is Ainu, called Lbisu in north, Tsuchigumo (trap-door spider) in south.

- Hikohichiro
 Matsumoto 1917 (28) Found Ainu characteristics in Tsugumo shell-
 mound crania.
 Ungai Ōno 1926 (30) Writes in favor of Ainu theory.

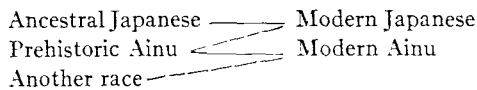
A, III. AINU-JAPANESE CONFRONTING THEORY

This theory was developed by Dr R. Torii in 1919 (20) and Dr T. Kida in 1920 (29). Torii took cognizance of the different types of prehistoric cultures that had by now become recognized. The Jomon culture he assigned to the ancestral Ainu; the Yayoi, to the ancestral Japanese. The two cultures and races were unrelated and have always confronted one another. Thus:

Prehistoric man with Jomon culture—————Modern Ainu
 Prehistoric man with Yayoi culture—————Modern Japanese

Summary

- Ryozo Torii 1919, 1925 (20) Expounds "Confronting" theory.
 Teikichi Kida 1920 (29) Prehistoric users of Jomon pottery were prehis-
 toric Ainu who were lineal ancestors of present
 Ainu. Differences noted in modern Ainu due to
 a mixture with another race. Resemblance be-
 tween prehistoric man and modern Japanese
 due to mixing of ancestral Ainu and ancestral
 Japanese. Thus:



A, IV. ECLECTIC THEORY

According to this theory the prehistoric man was the Ainu plus an uncertain and perhaps variable element.

After having actually excavated for several years in the Ōmori shell-mound, John Milne introduced the conception of a Koropok-guru race as has been mentioned (11). However, Milne believed that only in Hokkaido were the prehistoric sites due to the Koropok-guru. For northeastern Japan proper, he subscribed to the tradition which assigned prehistoric sites to the Ainu, who lived in pits and made stone implements and pottery. The inhabitants of Chishima (Kurile Islands), Karafuto (Sakhalin), and southern Kamchatka were a different race, though possibly offspring of the Koropok-guru. (Milne is here anticipating the work of later scientists who in the actual material recovered recognize different prehistoric cultures for Hokkaido and northeastern Japan.)

Before becoming the representative of the Confronting theory, Dr R. Torii had been an Eclectic. His report in 1906 (20) of his work in Chishima (Kurile Islands) states that those who left the prehistoric sites in northern Chishima were the Koropok-guru, who were the ancestors of the natives there, while those who left the Hokkaido sites were the ancestors of the Ainu. No notice is taken of the prehistoric sites of Japan proper. Such truth as lies in this theory seems to have been more developed from the point of view of Japanese origins. Dr Koganei was its principal antagonist. He flatly disputed Torii's views, claiming that the natives of both northern Chishima and Hokkaido were equally Ainu, and that the prehistoric sites of Hokkaido and Japan proper were all sites of the Ainu with the same culture, which seems to be an unsupported assertion contrary to fact.

Summary

John Milne	1882 (11)	See text above.
Romy Hitchcock	1890 (31)	Chishima Ainu of Shikotan Is. different from Hokkaido Ainu.
A. H. Lander	1893 (32)	Koropok-guru racially similar to Eskimo and came from northeast to southwest through Chishima to Kushiro in Hokkaido. No relation between Koropok-guru and natives of Shikotan Is.
Snow	(33)	Koropok-guru were a northern race that came south and were driven back north by Ainu, leaving sites in Chishima, Karafutu, Kamchatka, Aleutian Islands, Hokkaido.
G. Dumontier	1893 (34)	Prehistoric sites in Hokkaido are Koropok. Shell-mounds at Ōmori and Okadaira are remains of one group of Ainu. Natives of Chishima are another group of Ainu, not Koropok.
Raisuke Numata	1903 (35)	Believes Koropok equivalent to Kurile-Ainu.

B. THEORIES THAT THE PREHISTORIC PEOPLE WERE JAPANESE

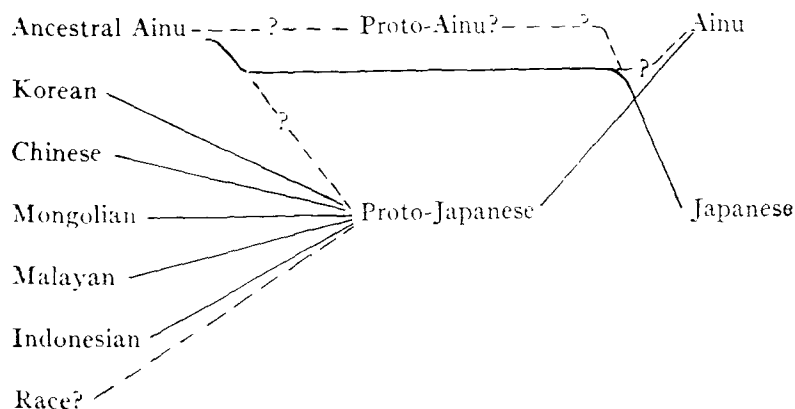
There has been more speculation concerning the origin of the Japanese people than as to the identity of the prehistoric people of Japan. Here again, the earlier theories were founded on mythology and legend or were deduced from the Japanese or Chinese classics. These are sufficiently indicated in the summary. A couple of the more far-fetched modern theories are also suggested (38, 39).

The efforts of the men listed in the summary from Baelz on have been increasingly scientific. Literature gave way to ethnology as a basis for determination, and ethnology was supplemented by physical anthropology and archaeology. The result has been an increasing awareness that the

question "What is the origin of the Japanese people?" merges with the query "Who were the prehistoric people of Japan?" (This was no more obvious to the Japanese scholar of yesterday than a similar identity to his European brethren.) The prehistoric people were seen to be so much involved with the Japanese that a blood relationship must be accepted in accounting for the latter. At the same time it was agreed that the recognized differences between Yayoi and Jomon cultures could not be taken to indicate racial differences.

Another general effect of this advance was to make the question of prehistoric man himself more complex. Instead of the simple situation presupposed in the single-line Koropok-guru and Ainu theories, or the parallel ancestries of the Confronting theory, or the only mildly confused, if obscure, ones of the Eclectics, we find many elements now entering the situation (e.g. 35, 29). Perhaps this modern opinion has been best presented by Dr Kenji Kiyono.

According to this view, prehistoric Japan was inhabited by a race which may be called Proto-Japanese. It had Jomon culture and was similar in some respects to both Japanese and Ainu, differing from both in other respects. The physical difference between these people and the Ainu or between these people and the Japanese was greater than the physical difference between the Ainu and the Japanese; i.e. three races must be recognized, though with a blood relationship existing among them: Proto-Japanese, Ainu, Japanese. There are suggestions of a prehistoric people preceding the Proto-Japanese phase. The more recent the Proto-Japanese is, the more he resembles the Japanese; and the more remote he is, the more the race seems to divide into several elements. In both the Proto-Japanese and the prehistoric stages the position of the Ainu is uncertain. Diagrammatically, with broken lines even more uncertain than the others:



*Summary**Origins as deduced from the classics*

Norinaga Motoori	1720-1801 (36)	Descendants of the kami (gods) who lived in Takamagahara (heaven).
Masahide Tamaki	{	Make Takamagahara = Yamato = Nara province.
Yoshikane Atobe		
Sadatake Ise		
Hakuseki Arai	1657-1725	Makes Takamagahara = Hitachi = Ibaraki province
Razan Hayashi	1583-1657	Cradle of the Japanese race is southern China.
Teikan Fujii	1722-1789	Japanese and Koreans came from same stock.

Some "simple-minded" theories

E. Kaempeer		Originated in Babylonia.
Zenichiron Oyabe	1928 (38)	Originated in Judea.
Takataro Kimura	1925-1926 (39)	Originated in Greece.

*Theories of origin founded on ethnology**a. One main stem*

Shozaburo Kanazawa	1929 (37)	Japanese and Koreans from same stock: philological deduction.
William Griffiths	1907 (40)	Language and manners trace Japanese to Ezo.
Haeckel		Japanese ancestors = Dattan.
Teiken Taguchi		Japanese ancestors = Huns.
W. Doenitz	1887 (41)	Japanese ancestors = Malays.
Bunkichi Horioka		Japanese ancestors = Malays.

b. More than one main stem

Erwin Baelz	1906-1907 (42)	Origin in both Malayan and Mongolian races.
Yonekichi Miyoke		Ditto.
Raisuke Numata	1903 (35)	Ancestors = Malays, Mongolians, Ainus, and ancient Chinese and Korean immigrants.
Ryuzo Torii	1919-25 (20)	On basis of Yayoi pottery: Japanese proper were a northern race from maritime province of Korea with Chinese culture plus peoples from Indo-China and Indonesia.
Hikoichirō Matsumoto	1919 (28)	Ancestors = northern race that came through Korea and were affected by Chinese culture.
Teikichi Kida	1919 (29)	Ditto.

Theories based on physical anthropology and archaeology

Montaru Suzuki	1918 (43)	No relation between Ainu and prehistoric man.
Kosaku Hamada	1922 (21)	Ancestral Japanese existed in prehistoric time in Japan, making and using pottery and stone implements.
Kukutaro Ōgushi	1920 (44)	Bones from Tsugumo and Kofu = Ainu.
Hikoichirō Matsumoto	1919 (28)	Prehistoric man and present Ainu = one group = Pan-Ainu; Pan-Ainu plus Asian = present Japanese.
Hasebe	1919 (45)	Questionable whether prehistoric man was Japanese or Ainu.
	1920	Kofu bones similar to Ainu.
Kenji Kiyono	1935 (1)	See text above.

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Titles of works published in Japanese are given in Japanese. Details regarding publishers have not been inserted because they would have to be in Japanese characters to be accurate and practically useful. They could be gotten in that form from a college librarian in Tokyo by anyone specifically interested.

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TOKYO, JAPAN

HISTORICAL CHANGES IN THE CHOCTAW KINSHIP SYSTEM¹

By FRED EGGAN

I

THE Southeast furnishes an interesting field for studies in social organization. Here is to be found a bewildering array of unilateral institutions, territorial groupings, and social classes which cross-cut one another in various ways. Most historical and comparative studies have concerned themselves with these more formal groupings, largely ignoring the underlying kinship systems.

The importance of kinship systems, particularly in relation to other aspects of social organization, is beginning to be apparent. The present paper, while presenting an instance of historical change in the field of kinship which has important theoretical implications, also attempts to furnish the basis for a preliminary classification of Southeastern kinship systems.

One of the important problems in social organization is an adequate classification of kinship systems according to types. Such a classification seems essential for either historical or comparative studies of social organization. Dr Leslie Spier has worked out a preliminary classification of kinship systems for North America into eight types, largely on the basis of the terminology used for cross cousins. In regard to two of these types he finds the terminology indicative of a more comprehensive classification of relatives:

Cross cousin terminology also offers a clue for the discrimination of the Omaha and Crow types. The first class together the mother's brother and his descendants through males: their daughters are always called mothers. The paternal cross cousins are then conceptual equivalents. Similarly systems of the Crow type class the father's sister with her female descendants through females and their sons with the father. Again, equivalent forms are used for the maternal cross cousins. That is, both systems ignore differences of generation in one or the other type of unilateral descent.²

¹ This paper is a rather unexpected outgrowth of a research project concerned with North American social organization under the direction of Professor A. R. Radcliffe-Brown. The field work was made possible by the Department of Anthropology of the University of Chicago, for which grateful acknowledgment is made. I would also like to acknowledge my indebtedness to Dr John R. Swanton. Subtract his studies from the material published on the Southeastern tribes and there is an unbridgeable gap. In using his materials as the basis for this study I have been continually impressed by his accuracy and clarity. I suspect he has foreseen many of the conclusions which I have reached in this paper. The paper has benefited from the criticism of Professors Radcliffe-Brown, Robert Redfield, and R. H. Lowie.

² Leslie Spier, *The Distribution of Kinship Systems in North America* (University of Washington Publications in Anthropology, Vol. 1, No. 2, 1925), p. 72.

If we look at the distribution of the Crow and Omaha types we find it to be somewhat irregular, though within rather widely scattered areas the distribution tends to be continuous.³ Furthermore, if we examine various Crow and Omaha kinship systems we find a series of variations on each pattern, so that often there is some difficulty in deciding whether a given kinship system is a Crow type or something else.

II

A preliminary survey of Southeastern social organization indicated such a situation in regard to the various kinship systems studied. While Spier has classified the Choctaw, Chickasaw, Creek, Cherokee, Timucua, and (less probably) Yuchi as belonging to the Crow type,⁴ an inspection of the source materials led to some preliminary doubts as to the correctness of this classification.

The formal social organization of the Southeastern tribes seems to have been based on the matrilineal clan, but otherwise few generalizations may be made at present. The Choctaw,⁵ for example, had matrilineal exogamous moieties divided into non-totemic clans, a territorial division into three or four groups of "towns," and four social classes. The Chickasaw,⁶ close linguistic relatives of the Choctaw, had matrilineal totemic clans with a dual-division which was not exogamous, along with various local groupings. The Creek,⁷ just to the east, in addition to the "Upper" and "Lower" tribal divisions, had numerous matrilineal totemic clans grouped into phratries and moieties, with a further dual-division into "red" and "white" towns, associated with "war" and "peace," respectively. The Cherokee had a simpler organization: matrilineal totemic clans which were exogamous, and possibly grouped into seven phratries. The Yuchi⁸ had matrilineal totemic clans which were exogamous and which varied in rank; these

³ R. H. Lowie, *The Omaha and Crow Kinship Terminologies* (Proceedings, Twenty-fourth International Congress of Americanists, Hamburg, 1930), pp. 102, 105.

⁴ Spier, *op. cit.*, pp. 73-74.

⁵ J. R. Swanton, *Source Material for the Social and Ceremonial Life of the Choctaw Indians* (Bulletin, Bureau of American Ethnology, 103, 1931). The moieties went out of existence in historic times, the clans taking over the functions of exogamy, etc.

⁶ J. R. Swanton, *Social and Religious Beliefs and Usages of the Chickasaw Indians* (Forty-fourth Annual Report, Bureau of American Ethnology, 1928).

⁷ J. R. Swanton, *Social Organization and Social Usages of the Indians of the Creek Confederacy* (Forty-second Annual Report, Bureau of American Ethnology, 1928).

⁸ W. H. Gilbert, Jr., *Eastern Cherokee Social Organization* (Ph.D. thesis, ms., University of Chicago, 1934).

⁹ F. G. Speck, *Ethnology of the Yuchi Indians* (Anthropological Publications, University Museum, University of Pennsylvania, Vol. 1, No. 1, 1909).

were cut across by a patrilineal division of the men into "chiefs" and "warriors," associated with "peace" and "war," respectively. This dual patrilineal organization tended toward endogamy in that a "chief" preferred his daughter to marry other "chiefs." The Natchez¹⁰ apparently combined a matrilineal clan system with a system of social classes, the whole regulated by definite rules of marriage. In addition the Natchez had a dual organization of "red warriors" and "white warriors." The Chitimacha seem to have had totemic clans and endogamous classes which approached true castes.

The kinship systems of the Southeastern tribes are all "classificatory" in that the father's brother is classed with the father, and the mother's sister with the mother, as far as terminology is concerned. In the following chart (fig. 1) we have outlined the kinship structures of the Southeastern tribes for which there is adequate data, in terms of the pattern of descent from the father's sister. The Crow type (A) is well known. Both Spier¹¹ and Lowie¹² agree that its essential characteristic is the classification of the father's sister's female descendants through females with the father's sister, and their sons with the father, thus giving a definite descent pattern.¹³ Lowie considers this classification an "overriding of the generation principle in favor of the clan or lineage principle."¹⁴

If we now examine the kinship structures of the Southeastern tribes with special reference to the pattern of descent from the father's sister, we find some interesting variations. In the Choctaw kinship system¹⁵ (B), for example, the pattern seems to be "turned around." Here the father's sister's son and *his* descendants through *males* are classed as "fathers,"

¹⁰ J. R. Swanton, *Indian Tribes of the Lower Mississippi Valley* (Bulletin, Bureau of American Ethnology, 43, 1911). See also J. De Jong, *The Natchez Social System* (Proceedings, Twenty-third International Congress of Americanists, New York, 1930).

¹¹ Spier, *op. cit.*, pp. 73-74.

¹² Lowie, *op. cit.*, p. 105.

¹³ While we are not here concerned with the nature of the terminology itself, it may be pertinent to mention one or two points. The Chickasaw and Creek classified the father's sisters with the grandmother. The Choctaw had a separate term for father's sister, male speaking, the women used the term for grandmother. In the Sixtowns division of the Choctaw we find the term for grandmother used by both sexes for the father's sister.

¹⁴ Lowie, *op. cit.*, p. 103. While Lowie specifically refers to the Omaha type, the same principle obviously holds for the Crow. This seems to be a basic principle for the classification of kinship systems. If this be so the Omaha and Crow types might be considered sub-types of a more fundamental "Crow-Omaha" type. The extent to which this principle operates can best be illustrated by constructing "lineage diagrams."

¹⁵ Cf. L. H. Morgan, *Systems of Consanguinity and Affinity of the Human Family* (Smithsonian Contributions to Knowledge, Vol. 17, 1871), Table II, 28, 29; Swanton, *Source Material for the Choctaw Indians*, pp. 84-90.

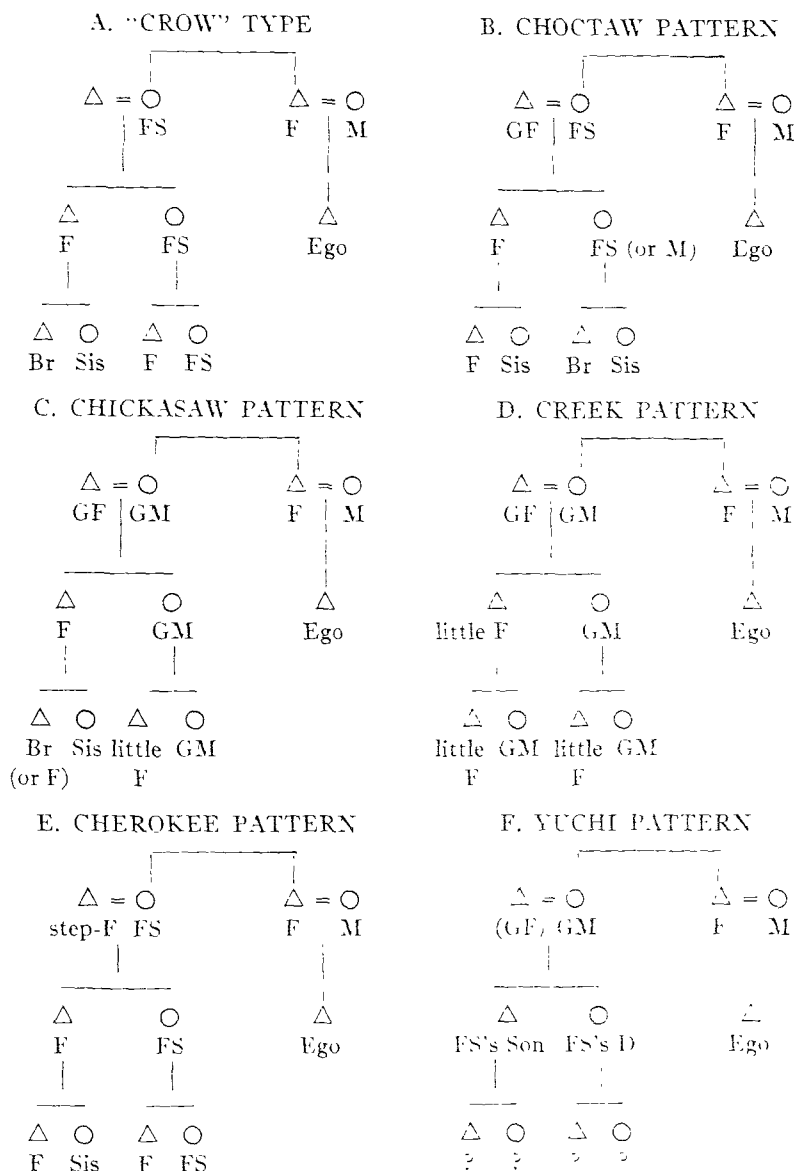


Fig 1 Kinship Structures in the Southeast Δ is male and ○ is female; Ego is male in each case. The equal sign indicates marriage. English equivalents are used for the native terms F, M, GF, GM, FS, Br, Sis, etc., standing for father, mother, grandfather, grandmother, father's sister, brother, and sister, respectively. For sources see the text.

whereas the children of the father's sister's daughter (who is classed with the father's sister) become "brothers" and "sisters." Morgan is quite explicit on this point:

My father's sister's son is my father, *Ah'-ki*, whether *Ego* be a male or female: his son is my father again, the son of the latter is also my father, and this relationship theoretically continues downward in the male line indefinitely. The analogue of this is to be found in the infinite series of uncles among the Missouri nations, applied to the lineal male descendants of my mother's brother.¹⁶

This is clearly something quite different from the typical Crow pattern of descent.

There are some interesting variations in the other tribes. The Chickasaw pattern of descent (C) as given by Morgan¹⁷ is identical with that of the Crow type, except for the minor variation of "little father" for the father's sister's daughter's son. Swanton,¹⁸ however, gives "father" as an alternative to "brother" for the father's sister's son's son. The Creek kinship structure¹⁹ (D) furnishes another pattern of descent. Here the descendants of the father's sister, in *both* the male and female lines, are classed as "father" or "grandmother." Hence the children of the father's sister's son are called "father" and "grandmother" rather than "brother" and "sister," as in the normal Crow pattern. The Cherokee kinship structure²⁰ (E) gives a pattern of descent from the father's sister much like that of the Chickasaw, except that the father's sister's son's son is regularly classed with the father, as are his male descendants through males. Information concerning the Yuchi kinship system (F) is difficult to interpret. We have no early accounts, but Speck²¹ indicates that the father's sister was classed

¹⁶ Morgan, *op. cit.*, p. 191. The last sentence is probably responsible for much of the confusion which exists in regard to the Choctaw kinship system. Structurally this pattern of descent is not *analogous* in the way that the Crow and Omaha are. J. Kohler (*Zur Urgeschichte der Ite*, Zeitschrift für vergleichende Rechtswissenschaft, Vol. 12, 1897, pp. 187-354) almost forty years ago considered the Choctaw system as analogous to the Omaha (tribal) system, and attempted to explain both on the basis of certain types of secondary marriages, types which had to be assumed for the Choctaw.

¹⁷ Morgan, *op. cit.*, Table II, 30.

¹⁸ Swanton, *Social and Religious Beliefs and Usages of the Chickasaw Indians*, pp. 185-86.

¹⁹ Morgan, *op. cit.*, Table II, 31; Swanton, *Social Organization of the Creek Confederacy*, p. 85-86.

²⁰ Morgan, *op. cit.*, Table II, 32, 33. Two schedules are given, gathered by missionaries in Oklahoma. The Cherokee (32) and Mountain Cherokee (33) divisions correspond with the "Western" and "Eastern" groups, according to Gilbert, *op. cit.*, pp. 50-56. Both were residing in Oklahoma but were differentiated in regard to time of removal and location on the reservation. (Cf. Grant Foreman, *The Five Civilized Tribes*, Norman, Okla., 1934, Book V.)

²¹ Speck, *op. cit.*, p. 69.

with the grandmother, while her children were called by descriptive terms. Recent information²² indicates an Omaha pattern of descent, the mother's brother's children being classed as "mother's brother" and "little mother," with the father's sister's children, reciprocally, being "nephew" and "niece," male speaking, or "little son" and "little daughter," female speaking. Information concerning the kinship systems of the Natchez and other Gulf Coast tribes is too fragmentary to be of much value. The Natchez, according to Swanton,²³ classified the father's sister with the grandmother. While no information is available concerning the terminology employed for her descendants, other features of the system, e.g., the classification of the brother's children as "children," male speaking, and "grandchildren," female speaking, and the sister's children as "nephew" and "niece," male speaking, and "children," female speaking, closely resemble those found in the tribes mentioned above.²⁴

III

On the basis of this preliminary survey it is possible to formulate some of the problems involved. One of the important problems is: do these patterns of descent represent new fundamental types of kinship systems or are they simply variations from a Crow type? If they are fundamental types they are unique; the writer is not aware of other kinship systems with these patterns of descent. If we examine the variants we find it possible to arrange them in a series between the Crow and Choctaw types: the Chickasaw have practically a pure Crow type, the Cherokee vary slightly, the Creek are intermediate, while the Choctaw have an almost completely reversed line of descent. All of these tribes had a similar culture, resided in the same area, were removed to Indian Territory under similar conditions, and were subjected to similar influences while there. This suggests that the variations found may represent historical changes. A further problem may be set up: if these are variants from a Crow type, do they represent a series brought about by the action of some common acculturation process, or are they merely "random," the separate results of unrelated circumstances?

These wider problems grew out of an interest in the Choctaw kinship system. On the basis of a preliminary study the writer had come to a tenta-

²² Communication from Dr Gunter Wagner.

²³ Swanton, *Social Organization of the Creek Confederacy*, pp. 94-95.

²⁴ A feature not noted in other systems reported for this region is the classification of the father's brother as "father" or "father's brother," and the mother's sister as "mother" or "mother's sister." Swanton inclines to the view that the use of a separate term for the father's brother is ancient, but this is possibly the result of contact. I found evidence of a similar change among the Mississippi Choctaw.

tive conclusion: the Choctaw system probably represented a pure Crow type which had altered under the stress of missionary and governmental activities. To *prove* this it would be necessary to find the older kinship system in operation in some relatively uninfluenced region, or to find an earlier account of the kinship system.²⁵ Morgan thought highly of his Choctaw schedules. The Reverends Byington and Edwards collaborated on one, Copeland furnished the other; these missionaries had long resided among the Choctaw and knew their language and customs. Furthermore the two independent schedules checked remarkably well. Hence they must be accepted as basic data—as representing the Choctaw kinship system as it was in 1860.²⁶

In 1933, on the advice of Dr John R. Swanton, the writer visited the old Choctaw country in an attempt to find some traces of the assumed older kinship system among the Bogue Chito and other groups which had remained in Mississippi following the general removal of the Choctaw to Indian Territory in the 1830's. This quest was completely unsuccessful, though a considerable body of contemporary material was gathered. The results, where they had any bearing, merely confirmed those presented by Morgan and Swanton.

Later in the summer, while studying Cheyenne and Arapaho social organization, the writer found in the "Chronicles of Oklahoma" the text of a speech which John Edwards made to the students of the University of California about 1880, outlining the *earlier* social organization of the Choctaw.²⁷ This is the same Edwards who collaborated with Byington in furnishing the basic schedule for Morgan; when the Civil War broke out in the next year he was forced to leave and went to reside in California. In this speech (edited by Dr J. R. Swanton) we find the following statements concerning the older Choctaw kinship system:

A third important principle was that kinship was not lost by remoteness. This involved a very peculiar system of nomenclature. For instance, with them, my father's brothers are all my fathers, and my mother's sisters are all my mothers, and their children are my brothers and sisters; but my mother's brother is my uncle, and his sons and daughters are mine and my father's sister is my aunt, her

²⁵ The comparative analysis outlined above might serve as partial proof. Actually, however, the writer did not see the implications of such an analysis for the Southeastern area until the Choctaw problem had been worked out.

²⁶ Morgan, *op. cit.*, p. 190. These schedules appear to represent the current kinship system at the time the schedules were gathered—1860. The differences between the schedules will be considered later.

²⁷ John Edwards, *The Choctaw Indians in the Middle of the Nineteenth Century* (Chronicles of Oklahoma, Vol. 10, Oklahoma City, 1932, pp. 392-425).

son is my father, her daughter is my aunt, and *her* daughter is my aunt, and *her* daughter is my aunt, and so on, as far as it is possible to go. This is what they call *aunts* in a row. The farthest removed of one's kindred by consanguinity are aunt, uncle, nephew, and niece. The line of relationship, after turning aside thus far, returns into the direct line, and becomes that of father to son, or grandfather to grandson. To us it seems a very complicated system.²⁸

Here is clear, unequivocal, documentary proof that the Choctaw formerly had a Crow type of kinship system.

There is good evidence, then, of a definite change in the Choctaw kinship system from the time of removal in the 1830's to the time when Morgan collected the schedules in 1860. Can the influences affecting the Choctaw from the time of removal to the Civil War be historically controlled? Fortunately this question may be answered in the affirmative; Grant Foreman²⁹ and Miss Debo³⁰ have both assembled and surveyed the available documentary evidence for precisely this period, 1830-1860.

The Choctaw were subjected to longer and more intensive acculturational influences than were the Chickasaw, Creek, or Cherokee. Missions were established among them as early as 1819. They were the first tribe from the Southeast to be removed to Indian Territory; they set up a new system of government on the model of our territorial governments; they early established a school system and encouraged education. Their leaders were more friendly to attempts to alter the old ways of life in favor of white ways.

There are many statements in Foreman³¹ indicating the efforts—and success—of missionaries, teachers, and government agents in changing the mode of life of the Choctaw. The fact that women worked in the fields and that a father (in accordance with the matrilineal system of inheritance) failed to provide for his own children, particularly worried the missionaries. There were introduced new regulations in regard to land which emphasized the position of the man as head of the family; by others leaders no longer represented the clan but the male membership of the district, being elected

²⁸ Edwards, *op cit*, pp. 400-401. The italics are Edwards'. This is a precise and excellent statement of the Choctaw kinship structure, and one which is consistent with what we know of other aspects of early Choctaw social organization.

²⁹ Grant Foreman, *op cit*. The following account is based primarily on Foreman.

³⁰ Angie Debo, *The Rise and Fall of the Choctaw Republic* (Norman, Okla., 1934). This volume was not available to the writer, but Swanton in reviewing this and Foreman's volume, considers that it supplements the latter, giving a much larger mass of material on the Choctaw in the form of a vertical monograph (John R. Swanton in *American Anthropologist*, Vol. 37, 1935, pp. 675-76).

³¹ Foreman, *op cit*, Book I, *Choctaw*.

by adult male voters. Marriage was regulated by law, and widows were entitled to a dower and children to inherit their father's estate.³²

For our purposes these statements indicate a change from a *matrilineal* emphasis to a *patrilineal* emphasis; though the missionaries and others concerned were not aware of the significance of the changes they were bringing about. The effect on the social organization of the Choctaw was to break down the clan structure and emphasize the territorial tie. In the later periods the clan structure became largely a memory, many individuals not knowing their own clans.³³ Specifically, this change seems to have affected the kinship structure by "turning around" the pattern of descent from the father's sister, making "fathers" descend in the male line, rather than "father's sisters" in the female line, as Edwards indicates for the old Choctaw system.

This conclusion, if it is of any value, should also "explain" the changes which have taken place in the kinship systems of the other tribes in the Southeast who were subjected to similar influences. Foreman presents evidence to indicate that the Chickasaw and Creek were considerably behind the Choctaw in "progress." The Chickasaw³⁴ in particular were backward. They were a smaller tribe, less sedentary, and more warlike. Their removal to Indian Territory took place later than that of the Choctaw and under less favorable circumstances. They were settled on the western portion of the Choctaw reservation, where they led a restless and unsettled existence, continually harassed by the unpacified Plains tribes. Attempts to merge them with the Choctaw were resented by both tribes, and delayed their advancement. Missionaries and schools were much later in influencing them; in 1847 there were no preachers or schools in their territory, schools not getting started until after 1850. Hence the Chickasaw retained their aboriginal customs to a greater degree, and for a longer time, than did the Choctaw. The Creek,³⁵ on the other hand, made more rapid "progress" than the Chickasaw. The first few years, after the removal of the majority of the Creek in 1836, were taken up with a continuation of the quarrel between the Upper and Lower Creek. They were suspicious and resentful of the efforts of missionaries to change their customs; in fact they expelled them from the Creek Nation in 1836 with the injunction, "Go teach

³² Foreman, *op. cit.*, pp. 84-85. Foreman is here referring to an account written for the "New York Evangelist" in 1852 by the Rev. Cyrus Byington.

The Mississippi Choctaw today, where they know of the old clan system, usually characterize it as patrilineal.

³³ Foreman, *op. cit.*, Book II, *Chickasaw*.

³⁵ Foreman, *op. cit.*, Book III, *Creek*.

your white men." The missionaries returned in 1842, however, and their influence gradually increased. After a measure of tribal unity was restored, some interest was taken in schools, there being a few schools by 1841. The Lower Creek settlements advanced faster than those of the Upper Creek, abandoning compact town settlements and communal cultivation at an earlier date. By 1844 chiefs were beginning to be elected, but in general they were prejudiced against the whites because they felt their authority to be lessened. By 1850-60 many changes were in progress: new laws were replacing old customs, property was being inherited according to legal provisions, schools were well established, men were doing the agricultural work, and missions were expanding.

On the basis of Foreman's material it is possible to arrange the Choctaw, Chickasaw, and Creek in a rough series, in so far as the *general* degree of acculturation is concerned. The Choctaw were subjected to the greatest influence, the Chickasaw the least, while the Creek were intermediate.

The Chickasaw had a Crow type in Morgan's time; the Choctaw earlier. There is good reason to believe the Creek formerly had a Crow type of kinship structure also. An early writer makes the following statement:

All the men of the father's clan or family are called their father, the women are generally called their grandmother, all the men of the mother's family older than themselves are their uncles, being their mother's brothers. All of their own age and under are called brothers, and all the old women of their mother's clan are called grandmother or aunt ³⁶

Swanton agrees to this but says:

In spite of the emphasis which Stiggins places on clans in determining the application of terms of relationship, an examination of the usages assigned to them shows that all the terms not individual cut across, or at least may cut across, the lines of the exogamous groups ³⁷

The writer would like to suggest, in view of the above analysis, that both Stiggins and Swanton were right; that the Creek have changed the applications of terms to the descendants of the father's sister. Their early institutions and behavior patterns, so far as we know them, seem to be consistent with such an assumption.

If we may tentatively assume that the Creek system represents a vari-

³⁶ Swanton, *Social Organization . . . of the Creek Confederacy*, p. 87, quoting from the Stiggins ms., no date.

³⁷ Swanton, *loc. cit.* He is concerned here primarily with the fact that the Creeks classify the descendants of the father's sister in *both* male and female lines as "father" and "grandmother." Hence it is not a clan (or lineage) classification exclusively.

ant from a Crow type, we thus have a series of variations which corresponds with the series worked out with reference to the general degree of acculturation. If this be so we have an "explanation" for certain discrepancies which exist in the source materials. In regard to the Choctaw, for example, Reverend Copeland's schedule gives "mother" as the term for the father's sister's daughter. If the Choctaw system were in a process of change, the term "mother" might be considered more suitable than "father's sister," since the children of the father's sister's daughter were called "brother" and "sister" in both schedules. This variation apparently was not popular for long, but the earlier process of shortening the line of aunts continued: thus Swanton found that the term for father's sister "sometimes extends to the father's sister's daughter."³⁸ The Chickasaw material furnishes us with an illustration of the first steps in this change. In Morgan's time the father's sister's son's son was classed as a "brother," but Swanton later reports both "brother" and "father" being used for this particular relative. The Creeks apparently have reached a relatively stable state in which the matrilineal and patrilineal emphases are more or less balanced.³⁹

The Cherokee furnish an additional group against which our conclusions may be tested. They belong to a different linguistic stock and have a different early history. On the other hand they were removed to Indian Territory and subjected to much the same acculturational influences that affected the other tribes, though if we may judge by Foreman's account,⁴⁰ these influences were less intensive than for either the Choctaw or Creek. During the first period after removal there was considerable trouble between the "Western" Cherokee, who had voluntarily migrated to Arkansas early in the 19th century and then had moved to Indian Territory in 1828, and the "Eastern" Cherokee, who were subjected to a forcible removal ten years later. Again white influence gradually brought about a change in the sentiments relating to females, a new division of labor, new laws and government, but these did not become well developed until after 1850. A national school system was established in 1841 and gradually grew as the leaders perceived the advantages of education.

A small group of Cherokee refused to be removed to Indian Territory

³⁸ Swanton, *Source Material for . . . the Choctaw Indians*, p. 87. In Mississippi the writer found only one or two elderly informants who extended the term for father's sister to the father's sister's daughter.

³⁹ Most of the material on kinship has been gathered with the view of illustrating the aboriginal systems. Hence changes may have gone on which have been largely ignored in the ethnological presentations.

⁴⁰ Foreman, *op cit*, Book V. *Cherokee*.

and remained in the hills of North Carolina, where they still reside on the Eastern Cherokee Reservation. This group, numbering at present less than 2,000 persons, has been studied recently by Dr W. H. Gilbert, Jr.⁴¹ He found them relatively uninfluenced in many respects, particularly in regard to social organization. He was fortunate enough to find the old kinship system in operation: his account of the terminological structure and the accompanying social behavior of relatives gives us our first relatively complete picture of the kinship system of a Southeastern tribe.

The system, as far as the pattern of descent is concerned, represents a pure Crow type. Relatives are recognized in four clans: one's own matrilineal clan, one's father's clan, one's mother's father's clan, and one's father's father's clan. In each of these, relatives are classified on a "lineage" principle. In the father's matrilineal lineage (and clan), for example, all men are "fathers," their wives "mothers" or "stepmothers," their children "brothers" and "sisters;" all women of the father's generation and below are "father's sisters," those above being "grandmothers" or "father's sisters," all husbands of these women are "grandfathers," all children are "father's sisters" and "fathers."⁴²

We have, then, existing among the Cherokee, the situation which the writer had hoped to find among the Choctaw. The evidence suggests that the Cherokee formerly had a Crow type of kinship structure, but that influences affecting the portion of the tribe in Oklahoma have modified the descent system in the same direction as in the other Southeastern tribes considered, so that the father's sister's male descendants through males are classed as "father."⁴³

These acculturational influences are of course not completely lacking in North Carolina, but seem to have been much less intensive. Gilbert mentions the loss of political power of clan heads, the gradual decline in family control, particularly in regard to marriage. Also:

The mother's brother is no longer a power in the family and the transmission of family names for the last three generations through the father's line has tended to shift the emphasis in lineality to the paternal ancestry.⁴⁴

⁴¹ Gilbert, *op cit.* The following account is summarized from this study.

⁴² This kinship structure is best seen when put in the form of "lineage diagrams." The Hopi kinship structure has an almost identical pattern, except that kinship is not extended to the father's father's clan. These two systems might well be considered as type structures for the Crow type, since the system of the Crow tribe is somewhat anomalous. If "lineage diagrams" are made for the Omaha (J. O. Dorsey, *Omaha Sociology*, Third Annual Report, Bureau of American Ethnology, 1884, pp. 205-370) the similarity of the Crow and Omaha types is apparent.

⁴³ It may be noted that this pattern (fig. 1, E) is very close to that for the Creek (D).

⁴⁴ Gilbert, *op cit.*, p. 278.

This shift in emphasis from the matrilineal to the patrilineal line among the Cherokee still residing in North Carolina should result eventually in similar changes in the pattern of descent, assuming other factors remain the same. Our hypothesis may therefore be verified, or modified, by future investigations of these groups.

The Yuchi furnish a separate and more complicated problem; they are considered partly to illustrate the value of our historical analysis, and partly to extend our survey of Southeastern tribes.⁴⁵ Speck studied the Yuchi in Oklahoma in 1904-5; they then resided in three scattered settlements in the northwestern corner of the Creek Nation. The Yuchi had belonged to the Creek confederation in the later period and were removed to Indian Territory with the Creek. In Speck's time they seldom mixed with the Creek, but were friendly with the Shawnee and Sauk and Fox.⁴⁶

We have outlined the kinship structure above, as far as it relates to the pattern of descent (fig. 1, F). In describing the kinship system Speck notes that

The family, in our sense of the word, as a group is of very little political importance in the tribe. The father has a certain individual social standing according to his clan and according to his society. The woman on the other hand carries the identity of the children, who may be said to belong to her. The bonds of closest kinship, however, being chiefly reckoned through the mother, it would appear that the closest degrees of consanguinity are counted in the clan.⁴⁷

It seems probable that by Speck's time (1904-5) the kinship system had already been considerably modified. The use of descriptive terms for the father's sister's children is unique in this area, and suggests a breakdown from some other pattern.⁴⁸ The classification of the father's sister as a "grandmother" is suggestive of a Crow type of system, especially when coupled with a matrilineal clan system.

This is mere conjecture, based on probabilities. We will never know precisely what the earlier system was unless new historical evidence is discovered. On the other hand we have evidence of important changes since Speck's visit. Dr Wagner has recently completed a linguistic study of the Yuchi and was kind enough to furnish me with a list of the current kinship

⁴⁵ My own point of view toward the Yuchi system has changed radically with the working out of this historical analysis. Without it Speck's and Wagner's kinship systems cannot be reconciled. (See below.)

⁴⁶ F. C. Speck, *op cit.*, p. 11.

⁴⁷ Speck, *op cit.*, pp. 68-69.

⁴⁸ The father's lineage might be expected to break down before one's own lineage in a matrilineal society.

terms. These, interestingly enough, seem to represent an Omaha type of structure. The father's sister is now called "little mother," her children are "nephew" and "niece," male speaking, or "little son" and "little daughter," female speaking. The children of the mother's brother are, reciprocally, "mother's brother" and "little mother." That the shift of the term for father's sister is recent is further indicated by the fact that the father's sister's husband is still called "grandfather."

In connection with this shift there are several factors which must be considered. Within the aboriginal Yuchi system there was a patrilineal emphasis through the "War" and "Peace" societies, which were confined to males and definitely patrilineal in membership.⁴⁹ Secondly, the same factors influencing the Creeks since their removal in 1836 have necessarily affected the Yuchi, though probably in a varying degree. Finally we have evidence of close contact with the Shawnee and the Sauk and Fox in recent years, both of whom have an Omaha type of kinship system. The Yuchi, then, possibly have gone through the whole sequence of changes from a Crow to an Omaha type of descent, though we have definite evidence for the last series of changes only.

IV

The immediate conclusions which may be drawn from this survey of historical changes in Southeastern kinship structures can be briefly summarized. (1) The evidence indicates that a Crow type of kinship structure was widespread over the Southeast.⁵⁰ The Choctaw, Chickasaw, Creek, and Cherokee all seem to have had such a system in historic times; the evidence for the Yuchi is inconclusive but favorable. Even the Natchez may have had a Crow type of kinship structure. (2) These kinship structures, originally Crow in type, were progressively modified by being subjected to varying degrees of the same acculturational process. For the Choctaw, Chickasaw, and Creek there seems to be a precise correlation between the degree of general acculturation and the degree of modification of the pattern of descent. The evidence available for the Cherokee and Yuchi confirms this correlation.

These conclusions have a firm foundation in documentary and other evidence. Further, they have a definite value; they make possible the recon-

⁴⁹ Speck, *op. cit.*, p. 68. Also we have the statement that both men and women labored together in the fields (p. 18).

⁵⁰ This conclusion, of course, is in keeping with previous classifications, though as I have attempted to show, not directly deducible from the available source materials. Spier relied on a communication from Swanton for his classification, it is probable that Swanton had arrived at conclusions similar to the ones here presented.

ciliation of inconsistencies between accounts for different periods, and thus afford a foundation for preliminary classifications and comparative studies, whether historical or generalizing.⁵¹

These conclusions also raise a whole series of problems which have implications for both acculturational studies and studies in kinship theory, though many of these problems require the analysis of much more material than is here presented. Since Hallowell,⁵² in a study of recent changes in the kinship terminology of the Abenaki, came to conclusions concerning certain of these problems which differ to some extent from those which the above material suggests, it seems worthwhile to examine some of these problems briefly.

The solid distribution of the Crow type in the Southeast is important. Lowie has pointed out that this is a characteristic feature of the two main regions in North America where the Omaha type occurs, and suggests that "there is only one conceivable explanation of the distributional data—historical connection within each of the two areas."⁵³ In the Southwest we also find a solid block of Crow types in the western Pueblos,⁵⁴ though the other occurrences of the Crow type in North America are rather isolated. In general the Crow and Omaha types occupy geographically separate areas; only in California and the Southeast do we find the two occurring side by side. For the Yuchi we know the change to the Omaha type to be relatively recent. While Lowie utilizes historical connection to explain the distribution within an area he finds no indication of any borrowing between areas.⁵⁵ Furthermore, borrowing as a *complete* explanation of the distribution within an area breaks down in the case of California where one of the Pomo subdivisions, as well as the Wappo, have a Crow type system in the midst of Omaha types.⁵⁶ The case of the Yuchi has some bearing on this problem. Superficially we might consider that the Yuchi have borrowed the Sauk and Fox kinship system through contact. But obviously a *pattern* of grouping relatives is not borrowed, particularly when the actual terms are not taken over, and the languages are not even mutually intelligible.

⁵¹ Unfortunately they also indicate that Morgan's schedules cannot always be accepted as representing the aboriginal kinship systems unaffected by white contact.

⁵² A. I. Hallowell, *Recent Changes in the Kinship Terminology of the St. Francis Abenaki* (Proceedings, Twenty-second International Congress of Americanists, Rome, 1928, Vol. 2, pp. 97-145), pp. 144-45.

⁵³ Lowie, *op. cit.*, p. 102.

⁵⁴ Fred Eggan, *The Social Organization of the Western Pueblos* (Ph.D. thesis, ms., University of Chicago, 1933).

⁵⁵ Lowie, *op. cit.*, pp. 102-103.

⁵⁶ Cf. Spier, *op. cit.*, pp. 73-74 and map.

I have attempted above to indicate some of the factors which have influenced this change among the Yuchi; borrowing is only one. If the situation were reversed in California, i.e., if there were one or two Omaha types among a large number of Crow types, we might suspect an acculturational factor such as we find in the Southeast.

In this study we have not been concerned primarily with kinship terminology, but rather with the patterns and principles which may be abstracted from native usage. In the systems of the tribes considered there have been few lexical changes; terms have changed primarily in regard to their applications.⁵⁷ Thus we find different patterns for grouping relatives at different periods. Kinship terminology and the kinship pattern may vary independently: the terms may change without affecting the pattern, as when a simple substitution occurs, or the pattern may change without affecting the terminology, or both may change. From this standpoint the traditional dispute over whether linguistic factors or social factors are involved in the kinship system has little point. One or the other may be dominant in different situations. Hallowell,⁵⁸ for example, found new terms replacing old ones among the Abenaki, as well as shifts in application: in the Southeastern tribes considered, the latter change seems more important.

From the standpoint of acculturation we have here an instance of culture change which is reflected to a certain extent by specific changes in the kinship pattern. While the acculturational process has not been adequately analyzed, its effects on the social organization seem to have been in the direction of emphasizing "patrilineal" tendencies at the expense of "matrilineal." The precise way in which the social factors affected the kinship pattern is an important problem. Even in this preliminary study a "causal" relationship of some sort is indicated, direct or indirect. It does not seem likely that these social factors have operated directly on the kinship pattern, which as we have indicated is an abstraction. Other aspects of the social organization have changed concurrently. Among the Choctaw, for example, the moiety and clan organizations gradually broke down under the impact of acculturational influences. The close correlation of the Crow type with matrilineal clans has been pointed out by Lowie;⁵⁹ in his earlier papers he attempted to explain kinship terminology as far as possible in terms of the clan. While it is possible that the acculturational process operated through the clan organization, the nature of the changes taking

⁵⁷ This, of course, changes the "meaning" of a term. Thus the term "father" has a different meaning among the Choctaw when the applications change.

⁵⁸ *Op. cit.*, p. 144-45.

⁵⁹ Lowie, *op. cit.*, p. 108.

place in the Choctaw system does not make this hypothesis very likely. The clan organization apparently gradually disintegrated; the kinship system, on the other hand, developed a new type of organization which did not directly reflect the clan system.⁶⁰

One important effect of the acculturational process was to modify the attitudes and behavior patterns which existed between various relatives. The patrilineal emphasis brought definite changes in the roles of males and females in the family and in the local group. The relation of a father to his children, in particular, was changed, largely at the expense of the relations between the child and the mother, and the child and the mother's brother. Specifically the relation of father and child was strengthened; the father gradually took over control of his children, became responsible for their education and training and for their behavior and marriage. Property came to be largely owned by the father and inherited by his children. Such changes must have influenced social attitudes toward relatives, as well as weakening matrilineal descent. It is this change in behavior patterns and attitudes which seems to be the medium through which the kinship patterns were modified. This is consonant with recent studies of kinship systems where a close correlation has been observed between the terminological structure and the social behavior of relatives. The changes in behavior patterns and attitudes seem to operate through affecting the choice of alternate principles of classifying relatives.

Hallowell, studying the historical changes in the Abenaki terminology, came to the conclusion that there was no precise correlation between the kinship nomenclature and social institutions.

The major lexical changes, as well as the readjustments in the usage of terms (pattern changes), were found to be most satisfactorily explained as "contact phenomena," resulting from the influence exerted upon Abenaki speakers by those of related Indian languages and Europeans.⁶¹

But Hallowell had no data indicating possible changes in social behavior except those inferable from the family hunting territory complex and the levirate.⁶² He partially agrees with Speck that the kinship terminology is in agreement with the social structure in earlier times, but fails to see how the specific changes in relationship terms can be directly connected with the gradual disintegration of the family hunting band.⁶³

⁶⁰ Compare Hallowell's conclusions, which are given below.

⁶¹ Hallowell, *op. cit.*, p. 145.

⁶² *Ibid.*, p. 138.

⁶³ Hallowell, *op. cit.*, p. 140. Compare this with the conclusion reached above in regard to the disintegration of clans in the Southeast.

These conclusions do not necessarily conflict with those reached in the present study. Hallowell was forced to infer the nature of the changes resulting from the contact phenomena: "local differences and custom must have been remoulded to some extent under these new conditions."⁶⁴ In the Southeast there is more evidence for these changes. On the basis of our analysis it seems likely that acculturational factors affect the kinship system through influencing social behavior and attitudes, rather than directly affecting the terminology.

Changes in social organization presumably may go on at different rates among different tribes. The rapidity of the changes reported for the Southeast is significant in indicating the sensitiveness of the kinship system to certain social influences, and raises some doubts as to "survivals" in kinship structures. A more important problem is concerned with the nature of the changes which have been described for the Southeastern tribes. Here a *similar* change seems to have taken place in most of the tribes considered. These changes vary in extent but may be arranged in a series (from a Crow type to a Choctaw type, and perhaps even to an Omaha type), and this series is correlated with the degree of acculturation. This suggests that we may have here a *general* type of change. If so we might expect other Crow systems, under similar acculturational influences, to undergo a similar series of changes.⁶⁵

The fundamental problem of the explanation of the kinship system in terms of correlated social institutions is too complex to be considered in any detail in this paper. In a future paper on the Southeast the writer proposes to bring together the relevant material and indicate its bearing on this problem. Lowie attempts to bolster the lack of preciseness in the correlations of matrilineal and patrilineal organizations with the Crow and Omaha systems by reference to special forms of marriage, though he points out that these are in many instances logical rather than empirical explanations.⁶⁶ More fruitful, in my opinion, is Lowie's insistence that "the more specific matrimonial arrangements are themselves a function of the rule of descent."⁶⁷ It should be possible to go even further and consider them both as functions of some factor or principle which they have in common. Incidentally, the levirate as a causal factor in kinship systems receives a set-

⁶⁴ *Ibid.*, p. 143.

⁶⁵ Such a hypothesis is not necessary for the conclusions reached for the Southeast, since the latter rest on empirical materials. If verified, however, it would have considerable bearing on kinship theory.

⁶⁶ Lowie, *op. cit.*, p. 100-107

⁶⁷ *Ibid.*, p. 108.

back in Hallowell's study. He finds, for example, an increase in the number of equations which might reflect the influence of the levirate during the period that the levirate is declining as an institution.⁶⁸

Finally we might point out that in the present study, at least, it seems possible to unite "functional" and "historical" points of view without doing too much violence to either. In studies of acculturation both would seem essential: we need to know something of the interrelations of social institutions before we can deal adequately with cultural change. Without the concept of a *kinship system*, for example, the changes recorded in terminology for the Southeastern tribes have very little meaning. On the other hand, without the historical analysis the kinship structure of the Southeast remains blurred. This analysis must be based on documentary evidence, however, since at present we have no satisfactory technique for reconstructing such changes as have been outlined. In terms of an ultimate interest in systematic general "laws" we have here an instance supporting the general hypothesis that "that any marked functional inconsistency in a social system tends to induce change."⁶⁹ The kinship systems of the Southeastern tribes seem to have partially recovered their internal consistency by means of a series of similar changes.

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⁶⁸ Hallowell, *op. cit.*, pp. 141-42.

⁶⁹ See A. R. Radcliffe-Brown, *Kinship Terminologies in California* (American Anthropologist, Vol. 37, 1935, pp. 530-35), pp. 533-34.

TATTOOING AMONG THE ARABS OF IRAQ¹

By WINIFRED SMEATON

DURING the period 1933-35 residence in Iraq gave me the opportunity of making certain observations on tattooing among Arabs and other peoples living in the country. Comparable material is to be found not only in Egypt and North Africa, but also on the other side of the Arabian Peninsula, in Iran and India. Although evidence for the long history of the practice is to be found in certain places, notably Egypt, no attempt is here made to go into the history or origins of tattooing, but discussion is confined to the custom as it exists at present among the Arabs of Iraq.

The information was obtained in two ways. During 1934 as a member of the Field Museum Anthropological Expedition to the Near East (Henry Field, leader), I secured part of the data on tattooing and branding.² This information was obtained from observations on all the individuals of the various groups measured, with questions as to the purpose of the tattooing and the names given to the various designs. Another source of information was from conversations with and demonstrations by professional tattooers in several places, as well as a number of women, mostly patients in the Baghdad hospital, who were elaborately tattooed. The two most important Arab groups observed during the anthropometric survey, including both male and female series, were the Shammar Beduins and the Albu Muhammad, a Marsh Arab tribe. Men from another marsh tribe, the Suwa'id, were also included, as well as from the Dulaim and Anaiza Beduins. The rest of the information on the Arabs was obtained in towns: Baghdad, Al-Kadhimain, 'Amara, An-Nasiriya, and Mosul.

Tattooing, which is a wide-spread practice in Iraq, is known colloquially as *daqq* or *dagg*, from a root meaning to strike or knock, and as the name implies, it is tattooing by puncture. Occasionally a man with a literary background will employ the classical word *washm*, but *daqq* is the generally accepted Arabic word. Tattooing is a custom which already shows signs of disappearing, especially in the cities. It is rarely observed among the upper classes, and is despised by city-dwellers of the lower classes as well. On the other hand, the tribespeople and *fellāhīn* still esteem it, particularly if the operation is performed in the town, and above all in Baghdad. Very often

¹ Read before the American Oriental Society, Middle West Branch, Chicago, March, 1936.

² A summary of tattooed designs among 398 Kish Arabs, 231 Iraq soldiers, and 38 Ba'ij Beduins has been published by Henry Field, *Arabs of Central Iraq, their History, Ethnology, and Physical Characters* (Anthropology Memoirs, Field Museum of Natural History, Vol. 4, 1935), pp. 455-56.

the tattooing is done by a townswoman, but in the towns themselves, according to an informant in An-Nasiriya, it is considered shameful to tattoo.

In Iraq it is found that tattooing is divided into two kinds, broadly speaking: ornamental or decorative tattooing, and tattooing applied for magic or therapeutic reasons. This statement is based simply on observation, and does not take into account the ultimate origins of the practice. Probably most tattooing has an ultimate magico-religious purpose, whatever may be its course of evolution. Magic and healing must of necessity be considered together, for the dividing line is arbitrary, especially among an unsophisticated people.

Generally the therapeutic and magic designs are simple and crude in form, with curative tattooing applied to the seat of pain or injury, whereas the tattooing done for the sake of beauty (*lil-hilā*) is more extensive and elaborate. But sometimes the divisions overlap, and a simple design will have no other reason than to be decorative, or an ornamental design will be employed for a therapeutic reason. Cauterization as well as tattooing is widespread among the people of Iraq as a cure for many ills, but this is a separate subject.

The most common kind of curative tattooing is for sprains. Another is tattooing against headache and eye disease. The tattooing is applied on the temple or forehead or near the eye. Tattooing is also used as a cure for local skin infection, and localized pain generally, and very often against rheumatism and cold.

All these, to our way of thinking, tend to be magical, but there is another type of tattooing which is avowedly magical, in which the tattooing is applied with the intention of helping to bring about some desired contingency. Magical tattooing is chiefly the concern of women, for here we enter the world of old wives' lore. Three recognizable varieties are found among the instances which came under my observation. The first is designed to induce pregnancy, a matter of great concern to Arab women; the second has the purpose of guarding children, especially boys, against death; and the third consists of charms for love or against other magic.

Tattooing to induce pregnancy was observed in only one case, but the practice was confirmed by statements from two other informants. One woman in the Baghdad hospital had three large dots irregularly placed on the lower abdomen, as well as a design around the navel. The dots in particular were to insure her having children, but she said she had already borne one child when the tattooing was applied. This was done on the third day of menstruation. A midwife from Al-Kadhmain, one of the best infor-

ments on the magic aspects of tattooing, also mentioned the practice of tattooing to insure child-bearing. According to her, the tattooing may be a single dot or a small design consisting of three or five dots, applied below the navel, or on the back just above the buttocks. It must be done on the second or third day of menstruation. A single dot in the center of the navel was specified by Kulthumah, the tattooer in An-Nasiriya.

A dot on the end of a child's nose is the most general form of magic tattooing encountered. In a country where the infant mortality is high, magic practices to preserve a child's life will be highly in favor. If a woman has lost several children, she will have the successive ones tattooed with a single dot, either on the end of the nose or on the lower abdomen. Some informants said that the magic effect was extended to later-born children, but others said that it was not effective for more than one child, and later-born children would have to be tattooed likewise. The tattooer in An-Nasiriya said that all the men in the village of Samawa are tattooed with a dot on the end of the nose, and one above the mouth on either side. This is done when they are children to make them look like girls so they will not die. A variation was observed in the case of a policeman in Baghdad who came of the Uzairij tribe. Instead of a dot on the nose he had on each temple a cross with a dot on each angle. His mother's previous children had all died, he said, so she had had him tattooed in this way to preserve him. He added that the design was also good for the head.

The efficacy of the third type of magical tattooing, which is a form of sympathetic magic, is aided by having someone read the Qur'an while the tattooing is being applied. This is practiced secretly by women, and I came across only one or two instances. In Baghdad I saw a woman with three dots tattooed in a triangle on the palm of her right hand to insure her keeping her husband's love. A similar design on the left hand would mean that the woman no longer wanted her husband's devotion. The midwife in Al-Kadhimain had a circle of five dots on the palm of her right hand. She said that she was her husband's second wife, and when he took a third, she decided that something must be done to ward off any possible conjuring on the part of the new wife. So one Friday noon, the most effective time, she had her right palm tattooed while a woman mullah read Qur'an. The potency of the tattooing could not be doubted, for the result was that her husband divorced both his other wives and kept her!

Besides the magic and therapeutic varieties, there is a vast amount of tattooing whose ostensible purpose is to beautify the wearer. Most Arab women, at least outside the cities, are so tattooed. Not only the face and hands are decorated, but arms and feet, back, thighs, chest, and abdomen.

Among the Albu Muhammad definite observations were made on only a few women, but from superficial observation it seemed that nearly all of them were tattooed. The husband of one Albu Muhammad woman stated that his tribeswomen tattoo extensively because the men like it, and refuse to marry a girl who is not tattooed. Among the total of one hundred and twenty-nine Shammar women observed, only three were not tattooed, and they were young girls.

A very pretty and elaborately tattooed girl from the neighborhood of Hilla, who was twenty years old and had been married seven years, was one of my richest finds, especially for the actual designs. Her tattooing had been applied not all at one time, but during the course of three years. During her tenth year, her face, forearms, hands, and chest were tattooed; during the following year, her thighs and back; and in her twelfth year, her feet and upper arms were tattooed. All this was simply ornamental, but she had also a little curative tattooing, namely, a single dot in the inner corner of the right eye because of pain in the eye, and three marks on the right thigh, done by herself, and a linear mark on the right foot, tattooed to cure pain in the leg, which occurred after childbirth.

Another informant at An-Nasiriya said that her face, hands, arms, and feet had been tattooed some time before marriage, and her thighs, back, and abdomen had been done at the time of her marriage—all in one operation, which took seven hours, and must have been exceedingly painful.

Although the idea was never suggested by any of my informants that tattooing is a puberty rite for girls, the fact is that for the most part, girls are tattooed about the time of reaching puberty, or at least before marriage, which is apt to occur not long after puberty. There seems, however, to be no sort of tabu attached to the operation, either for the person tattooed or for the operator.

Tattooing among the Arabs is not confined to the women, as one is sometimes led to believe, but is practiced to a wide extent by the men as well, although the latter for the most part confine theirs to the hand and forearm and the face. But tattooing of the face is not as common among men as among women, and where it is found among men, it generally has a definite purpose, magic or curative, while designs on the hands and arms may have such a purpose, or may be simply decorative. Sometimes it is admitted that such tattooing is for beauty, and sometimes, if the man is rather ashamed of what he considers a feminine method of adornment, he says it is *hīch*, nothing. It may be suspected that the typical wrist design displayed by men, which outlines the wrist and back of the hand, may have the fundamental purpose of strengthening the wrist, and in fact, this reason

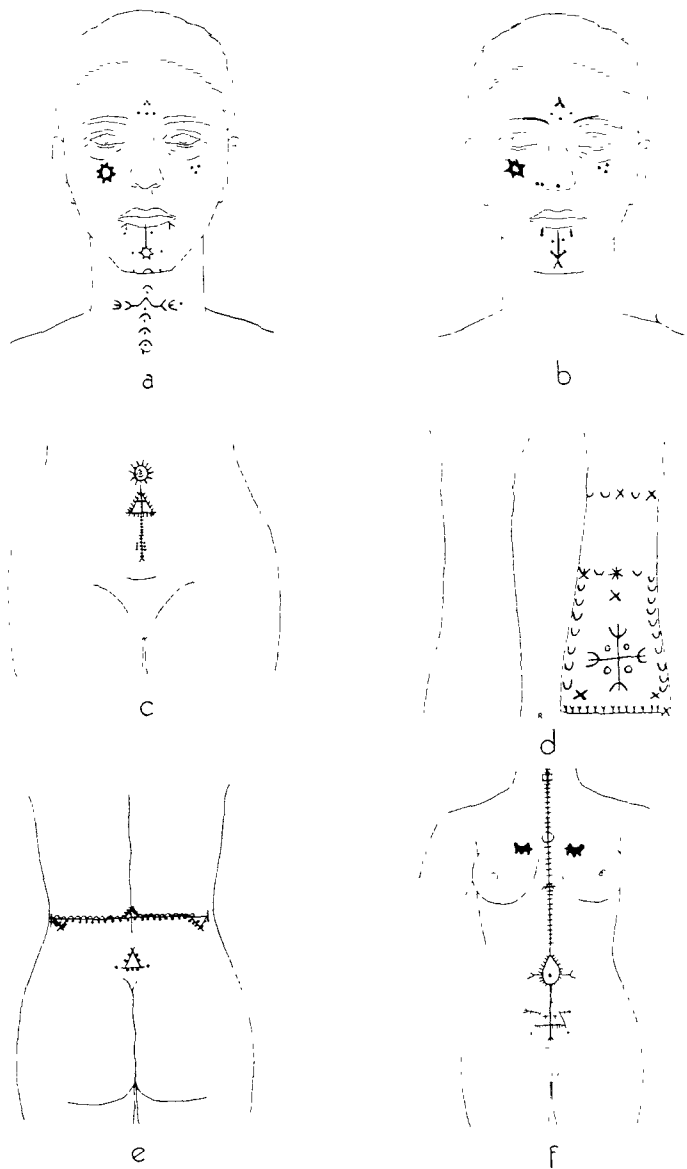


FIG. 1 Facial, body, and arm tattooing of Arab women a-d, Women of Shammar; e, Non-tribal woman of Baghdad; f, Gypsy woman (Kaulia).

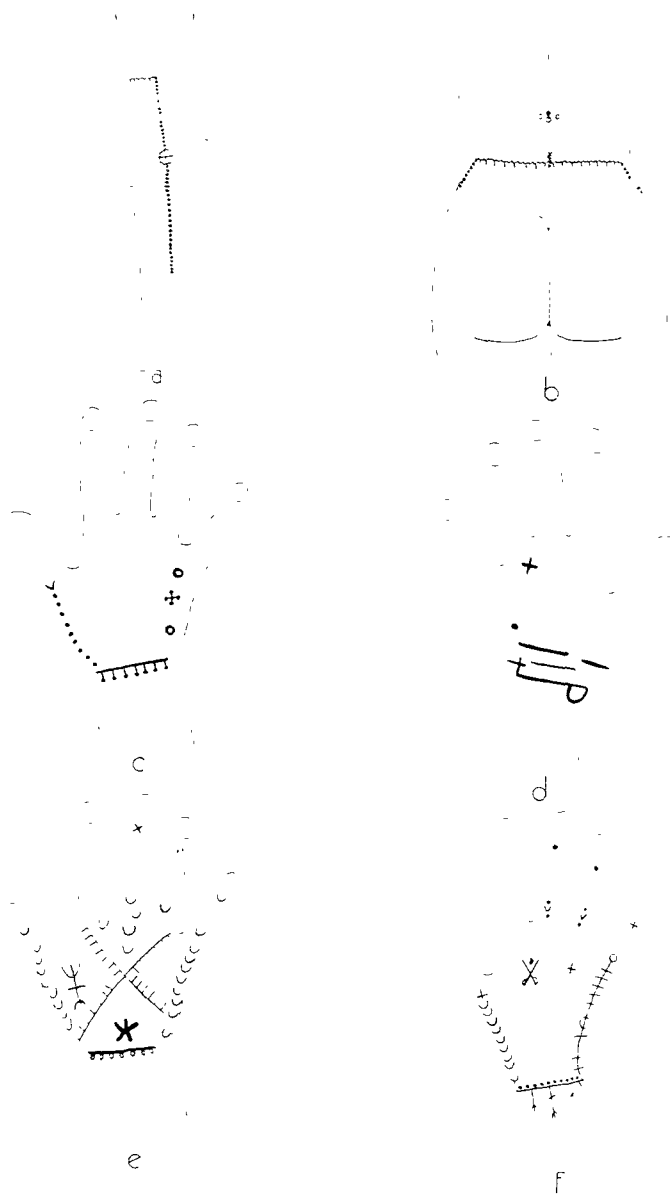


FIG. 2 Body and hand tattooing of Arab men and women a, b, Woman of Abu Muhammad, c, d, Men of Abu Muhammad, e, Shammar woman, f, Dulaim man Curative tattooing is illustrated in d.

was given by a professional tattooer in 'Amara. Many men were observed with lines tattooed across their wrists as a cure for sprains. Sprained wrists and thumbs seemed to be quite common, according to the number of cases in which tattooing had been resorted to as a cure.

Tattooing seems to be more common among men in the south of Iraq, that is, the Marsh Arabs near 'Amara, and the settled tribes of the district around An-Nasiriya, than among the Beduins. Among the latter, not more than one-third were tattooed, while among the settled tribes, at least three-quarters of the men were tattooed.

Nearly all tattooing among the Arabs of Iraq is done by women, mostly professionals. It is not a hereditary profession, but any woman who has the skill and inclination can become a *daggāḡah* or tattooer. No evidence was produced to show that the tattooer must come from any specific group, except that in a few instances the tattooing was said to have been done by gypsies, or that she must undergo any preliminary ceremonies or observe certain tabus at any time. Much of the simpler sort of tattooing is done by mothers upon their children, sometimes when only three days old.

Arab tattooing is always blue in color, and the designs are geometrical, or sometimes extremely stylized representations of natural objects. There are various methods of making the pigment for tattooing, which is known as *kohl* or *baṣmah*, but the principle is the same, for the chief ingredient is always carbon in the form of lamp-black. The word *kohl* usually refers to the powdered antimony which is put around the eyes, but it is also used to mean lamp-black, which is used by the poor in the same way as the antimony. The carbon is precipitated by burning either the ordinary kerosene of lamps, or tallow, or a piece of cloth dipped in *dihn*, the mutton fat used for cooking. Sometimes indigo is added, or bile from the gall-bladder of an ox, which sets the dye, but the commonest method is to gather the soot precipitated on the bottom of a dish held over the lamp, and make a paste. Many people hold that the soot must be moistened with *ḥalīb umm al-bint*, the milk of a woman nursing a daughter, which has magic properties, but others say that it is not good, and use water or kerosene. The use of human milk was noted in several places, always the milk of a woman nursing a girl, as the milk for a girl is supposed to be specially soothing and cooling. On the other hand, the chief tattooer in An-Nasiriya said that it was not good to use milk because it attracted flies, and then the tattooing spoiled. She herself used the simplest of ingredients, the soot of kerosene moistened with water, and samples of her work were both clean-cut and of good color.

In all cases the instruments used are ordinary sewing needles of a varying number according to their size and the technique of the operator. Usu-

ally they are of a good size, but smaller than a darning needle, from two to four bound together for at least half their length. First the design, which in most cases depends on the taste and skill of the operator, is drawn on the skin with the needles dipped in the dye, and then pricked through. The tattooed surface may or may not bleed; whether it does or not is not important, except that some women in An-Nasiriya said that it is better to perform the operation in the morning because it bleeds a lot if it is done at noon. The tattooed area may be swollen and matter for three days. A scab forms, which comes off after three to seven days, leaving the design well fixed under the skin.

As for the designs employed, a great deal could be written on the subject, especially on the history of the names, and the comparison of the designs themselves with those found in ancient and modern times on pottery and textiles. The designs are geometrical or stylized. Generally they consist of combinations of dots and lines, especially zigzag and cross-hatched lines, circles, crescents, chevrons, triangles, stars, and crosses, and elaborations of these. The elements everywhere are the same, but of course in some districts certain patterns are used, which in other places are not known, or at least not held in favor.

The patterns also depend on the part of the body tattooed, especially in the case of ornamental tattooing among women, for curative tattooing is usually simple in form. Both men and women have the back of the hand and wrist tattooed, the whole design often being known as the "glove." Lines with some sort of cross-hatching or other decorations form the most important parts of patterns on the legs and arms, and down the chest. The latter type of design is found everywhere among the women, and consists essentially of a line which begins at the lower lip and runs down the chin, neck, and chest nearly to the waistline, and sometimes extends to the navel. Other designs on both forearm and upper arm circle the arm like bracelets, and similar designs are found on the ankles, although foot and ankle patterns do not necessarily go all the way around.

Women's eyebrows are frequently tattooed, and most women have some tattooing on the face, especially on the chin, and dots between the eyes and above the upper lip. Sometimes vertical lines on the chin are extended through the lower lip, and I have seen a few women with all of the lower lip tattooed.

The elements of the patterns are given their proper titles: star, crescent, zigzag, double zigzag, and so on, while the whole designs are generally named for the part of the body adorned: chin, chest, back, side, foot, or wrist. A design on the side of the cheek is known as "the shadow of the side-lock," and dots on the upper lip may be called "mustache" or "shadow of the nose-ring." A single dot on the face, and especially between the eyes, is

called "dimple." Rayed figures are known as "sun," "star," or "flower," while circles may be called "disc," "ring," or "moon." It is interesting to note that one or two of the terms used to denote small designs on the face refer originally to marks or blazes on horses. A design consisting of a line with single cross-hatching, particularly on the wrist, is often called "comb," but I am inclined to think that the design is derived from the figure of an animal, presumably a gazelle.

Conventionalized gazelles are perhaps the most interesting of the designs noted. Men particularly are fond of having a gazelle tattooed on the inner forearm. Women also have gazelles, on the forearm or hand, and sometimes in pairs on the breast, on each side of the line running down the chest from the face. Since representations of living beings are forbidden to Moslems, one is tempted to think that the tattooed gazelles may be survivals of an ancient totemism. Some of the so-called "combs" look very much like elaborated gazelles, while on the other hand, a simplified form something like a broad letter H is also called gazelle.

Another interesting and primitive design, which was found in all groups, is the cross. It is always found with arms of equal length, and frequently there is a dot in each angle. From ancient times, and in many places, the cross has had a certain magic function attached to it, and the idea is borne out by one of my informants, the midwife from Al-Kadhmain, who said that the cross, or as she called it, the four-sided, is the best, that is, the strongest design. The design of the dotted cross is by no means modern, for it is noted among those of tiles from Samarra,³ dating from the middle of the ninth century, and on fourteenth century potsherds found by Dr N. Debevoise⁴ in the neighborhood of Tell Dahab, near Tell Asmar. Another sherd bearing the same design was found at Tābūs on the Euphrates.⁵ Debevoise suggests that these stamped designs may be potters' marks.

This is but a brief summary of such notes and observations as I was able to make on tattooing among the Arabs of Iraq. It is hoped to publish a fuller account later, with special attention to the designs and names thereof. The present discussion, while by no means exhaustive or conclusive, will, it is hoped, contribute something to our knowledge of tattooing in southwestern Asia.

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³ Ernst Herzfeld, *Die Malereien von Samarra* (Die Ausgrabungen von Samarra, Forschungen zur islamischen Kunst, Ser. 2, Vol. 3, Berlin, 1927), pp. 75-78.

⁴ Staff member of the Oriental Institute Expedition of the University of Chicago, excavating at Tell Asmar during the season 1931-32.

⁵ Friedrich Sarre and Ernst Herzfeld, *Archäologische Reise im Euphrat- und Tigris-Gebiet* (Forschungen zur islamischen Kunst, Ser. 1, Vol. 4, Berlin, 1920), Vol. 4, p. 11.

MY interest in the study of Zuñi was twofold: to get a cultural perspective by acquainting myself with a different culture, and to learn the field technique of American anthropology. In other words, I would rather take the study of Zuñi as an illustration of larger issues involved than as a limited piece of ethnography. Thus in the present paper if I raise many questions, my effort is to be taken as trying to seek light and not as trying to make criticisms. In fact, my choosing Zuñi particularly was because of its rich literature by outstanding authorities.¹

I arrived at Zuñi, in western New Mexico, on June 15, 1935, and except two weeks' excursions, I spent all the time in a native family at Zuñi until September 16th when I left for the east. My role was more of a participant observer than of an active inquirer, except for some census-taking, after I was taken in by the community quite as a matter of course. I explained myself as one from China who was anxious to learn the wisdom of other peoples in order to teach my own people better. I repeatedly told them that I was not interested in getting secrets of any sort, and that they could tell me anything they cared, but that, should I happen to ask something which they would not like to tell, I would appreciate their telling me so. About the end of my stay, they seemed to be not very conscious of me, especially the family in which I lived, which assumed joking relationship with me and would not break their conversation on my entering their living room. Once in a while they would volunteer some information, and a few days before I left they were ready to discuss group activities at large, either public or esoteric. I participated in the family planting of feathers in the field before the rain dances, of which I observed six, being all that took place that summer.

The present paper is concerned only with some aspects of interpretation of Zuñi life. Due to lack of space, factual information and the problem of adaptation are reserved for other occasions.

* Owing to Mr Li's absence from the country, he has had no opportunity to see this article in its final revised form. Responsibility for the revision is ours —Editor.

¹ The writer wants to take this opportunity of acknowledging his grateful indebtedness to the Department of Anthropology of the University of California and its friends for the preparation of his field work at Zuñi, to the U.S. Indian Service for the connection which made the study possible, to all the friends at Zuñi who were very kind and helpful, and to the Department of Anthropology of Yale University for the stimulating manner in which culture was treated while the material on Zuñi was being written up. It is a great pleasure to acknowledge also that personal conferences with Dr Benedict and Dr Bunzel have helped a great deal to distinguish between the official view, native or otherwise, and the realities of a culture, which the writer did not have the privilege of studying more than a single summer season.

When going over earlier publications on Zuñi, one cannot help having all sorts of curiosity and bewilderment, yet field experience with enough reflection inevitably corrects both second-hand impressions and immediate irrelevant comparisons. True perspective and objectivity can be gained by distinguishing between judgments based on isolated cultural traits and those based on contextual relations, between absolute schemata of one's own culture and relative significance of another cultural pattern, between the selective nature of old mechanisms and the penetrating power of an intrusive system.

RELIGION

The prevailing impression among ethnologists in America is that Zuñi religion is a purely formalistic thing without much bearing on personal feelings. Perhaps Benedict and Bunzel are responsible for this. In her book on the *Patterns of Culture*, Dr Benedict spoke of "the acts and motivations of the individual" in Zuñi religion as "singularly without personal reference."² As her intention was to establish clear-cut types of culture by contrasting Zuñi life with that of the Kwakiutl and that in Dobu, it is understandable that the pictures are probably over-simplified. But once the pictures are so painted with all the process of elimination and selection, they tend to appear as independent entities, as complete in themselves, and thus very misleading.

Dr Bunzel's presentation on the other hand, in spite of strong emphasis on the formalistic side, is more balanced because of its intensive concentration on Zuñi alone. Yet one's discerning power in reading between the lines is easily confused once her characterization has taken hold of the mind, particularly when the emphasis is reinforced by Dr Benedict. A few quotations from Dr Bunzel's report will serve as concrete examples. In one place she says:³

In Zuñi, as all the pueblos, religion spreads wide. It pervades all activities, and its very pervasiveness and the rich and harmonious forms in which it is externalized compensate the student of religion for the lack of intensity of that feeling. For although the Zuñi may be called one of the most thoroughly religious peoples of the world, in all the enormous mass of rituals there is no single bit of religious feeling equal in intensity and exaltation to the usual vision quest of the North American Indian.

² Ruth Benedict, *Patterns of Culture* (Boston, 1934), p. 105.

³ Ruth L. Bunzel, *Introduction to Zuñi Ceremonialism: Zuñi Origin Myths, Zuñi Ritual Poetry; Zuñi Kachinas* (Forty-seventh Annual Report, Bureau of American Ethnology, 1932, pp 467-1086), p. 480

Elsewhere Zuñi prayer is said to be

not a spontaneous outpouring of the heart. It is rather the repetition of a fixed formula. . . . Practically all the techniques employed by primitive or civilized man to influence the supernatural are known at Zuñi—fetishism, imitative magic, incantation, and formulae figure largely in ritual while the more personal approaches of prayer (which in Zuñi, however, is largely formulistic), purification, abstinence, and sacrifice are also conspicuous. The weighting is on the side of the mechanistic techniques which are highly developed.⁴

One naturally has this question in mind: Why should "the spontaneous outpouring of the heart" be antithetical to "the repetition of a fixed formula?" In consideration of the social content of an individual mind, one's aspirations need not necessarily be in conflict with the cultural framework in which one finds oneself, and indeed the most developed forms of self-sacrifice are the result of an intensified super-ego. If one's interest is in the contrast between a self-possessed mental attitude and that of a vision quest, it is legitimate to make the contrast. But one form of mental process needs no greater amount of personal feeling than the other, so far as religion is concerned. Both excited and contemplative types of religion may be following respective cultural patterns to the same degree, and both may be as personal below the surface of social conformity. Upon entering a different culture, one is likely to forget all the intricacies of one's own cultural forms. Should an American student of ethnology be asked whether the Holy Rollers or a church group, with its methodically conducted ceremony, have a greater degree of personal feelings in their religion, he is more apt to make a refined judgment. And the analogy of the Christian church serves to show that the participants in a well conducted service, following stereotyped prayers and songs, may differ immensely in their levels of participation. One may respond to the outside collective behavior with all individual fantasies quite irrelevant to the service. Another may be identifying himself so well with the surrounding fellowship that collective behavior is an embodiment of his personality as a whole for the time being. No doubt some of the Holy Rollers, as some of those who indulge in vision quest, are entirely mechanical in their yelling and frenetic manifestations. After all, there is a tremendous difference between the official appearance and the inner reality.

After all the emphasis on the fixed nature of individual prayer, in form and content, Dr Bunzel herself has given some hint to the contrary. For she speaks of its being "individually varied in degree of elaboration" and

⁴ *Ibid.*, pp. 493, 499.

quotes her informants as saying: "Some men who are smart talk a long time, but some are just like babies." And on the same page she goes on to mention the occasions "on which men display their skill in handling the poetic medium." She also speaks of "the compulsive force" of Zuñi dance.⁵

According to the testimony of Mrs Stevenson, all the prayers "are repeated in low and impressive tones."⁶ The mode of behavior characterized by low and impressive tones is always observed by any student of Zuñi culture, and is easily forgotten as characteristic of personal feelings when one jumps to contrasting Zuñi with something else, as if personal feelings were strangely absent among these Pueblos. In speaking of a myth to persuade the Corn Maidens to go to the people, she had some more symbolic statement to make: "All spoke with their hearts; hearts spoke to hearts, and lips did not move."⁷ If the symbolic significance is to have hearts speak to hearts without outward manifestations, is it not just the opposite of mechanical prayers? Personally I was very much impressed with the deeply religious atmosphere in the truest sense of personal communion, when I intruded unwittingly one early morning on an old man in all solitude in the open field, praying to the rising sun. I retreated instinctively even before I realized that I was in the "Holy of Holies." Such a living picture was much more impressive than that reproduced by Stevenson in Figure 1, facing page 15 of her above-mentioned work. Having been made aware of this event, I had more occasions to watch such scenes from afar.

Having established the place of personal elements in Zuñi religion, let us pay some attention to another aspect of the problem of religion. In consideration of the all-powerful religious activities at Zuñi, one often wonders how the people find time and interest for the activities in the domain of the profane world. Here again, the exclusive preoccupation of almost all the students of Zuñi culture in the esoteric and the abstract has left some impressions of its lack of balance. But a moment's reflection will show that, whatever the impression, a society cannot function forever if actually unbalanced. Moreover, what seems strange to an outsider, may not be strange at all in the inner mechanism of the culture itself because of its well rounded checks. One trait transplanted in another culture without its previous background and context will be strange indeed. Thus all the unbalanced picture of a treacherous Dobuan or of a vision-seeking Plains Indian will lose its

⁵ *Ibid*, pp. 615, 899.

⁶ Matilda Coxe Stevenson, *The Zuñi Indians: their Mythology, Esoteric Fraternities and Ceremonies* (Twenty-third Annual Report, Bureau of American Ethnology, 1904, pp. 3-608), p. 171.

⁷ *Ibid*, p. 527.

significance in its proper context. American college games might be taken by an outsider to mean that American students must have no time for studies. The plain fact in Zuñi seems to indicate that the esoteric interest is simply an aid to the ordinary struggle for existence. Its activities are not only meant to cope with the unknown, but also to orient the activities of the profane world. For instance, the religious dramatic dances are not only so much prayer for rain or snow, which is the most desirable thing for arid Zuñiland, but they serve both as calendar and as harmonizing force for the community. That other groups, the fraternities, are largely curing societies is too obvious to need comment. Most significant of all is this fact, that although theoretically all the male members are supposed to belong to the dancing groups, they participate only in turn: the service of those who do participate is called on at regular intervals, because dances come regularly; and apart from those who are performing the sacred duties, the people in general do not attend as bystanders all the time on such occasions. As a rule they attend their farming or other business in spite of the dances taking place in the community. It is toward evening or on days that they are free from such duties that a large audience surrounds the performers. Even the high priests take care of their own farms. But whenever the dances are crowded with large audiences, both the latter and the dancers seem to co-operate unconsciously to produce a harmonious atmosphere. In other words, the audience is also performing the sacred duty as well. Such a phenomenon does not seem to have drawn much attention from the students of Zuñi, but it is worth while to point it out, particularly because such performances take place in the open air, attended by all sorts of people, without any effort on anybody's part to give directions to the audience. In view of the regular noise in any indoor gathering, be it a church assembly or a dinner party, it is remarkable to find responsive quietness prevailing on the plazas of Zuñi in the public dances. Dr Bunzel, in speaking of the pleasurable activity of group dancing, has this happy remark: "Joy is pleasing to the gods and sadness is a sin against them."

The participants in dramatic dances are affiliated in six groups, with headquarters in six kivas corresponding to the six directions. Although the whole male population is supposed to belong to these groups, not every man is active. The active membership of these groups is given in Table 1.⁹

But even consciously there are six categories of dances out of the great mass of those performed by any one of these groups which are considered

⁹ 1925-1926-1927

¹⁰ 1931 population 1935-1936: 11,580 females and 11,566 males, of the latter, 768 are not yet of marriageable age.

TABLE 1. MEMBERSHIP IN KIVA GROUPS

<i>Name of group*</i>	<i>Kiva number</i>	<i>Cardinal point and color</i>	<i>Number of members</i>	<i>Members</i>
he'ikwe	1	north, yellow	4	15
tcupa'kwe	2	south, red	3	38
'ohekwe	3	east, white	5	40
muhekwe	4	west, blue	2	86
'upsanakwe	5	above, many-colored	2	19
hekiapakwe	6	below, black	4	54

* Following Bunzel's orthography (*Zuni*, pp. 148-50).

basic to genuine sacred drama. These are *teakwena* (with drum), *teakwena* (without drum), *wotemla* (mixed dance with drum), *wotemla* (mixed dance without drum), *mubiktakla*, and *he'ikwe*.

Apart from these basic forms, there are imitations of the dances of other tribes, which in turn are classified into masked and unmasked dances, the latter being just for fun. Of the former I noticed the Corn, Mountain, Sheep, and Butterfly dances of the Hopi, the Kumatso, the Ancestral, composed at Zuñi, and the Nahabico (a ceremonial song of the Hopi). These are more or less serious, comparable to the dances of the Pueblo Indians without masks, which are just for fun. Included in the latter are the Navaho Squaw dance, a made-up Kumatso dance, and the Navaho Squaw dance. I observed one of the masked dances, the *teakwena*.

TABLE 2. MEMBERSHIP IN FRATERNITIES

<i>Names*</i>	<i>Members</i>
ne'wekwe (Galaxy fraternity)	42
shi'wannakwe (including three orders: Medicine, Magic, and Fire)	41
ha'lo'kwe (Ant fraternity)	41
ma'tke 'san'nakwe (Little Fire fraternity)	50
ma'tke 'hlan'nakwe (Great Fire fraternity)	20
pe'shatsilo kwe (Cimex fraternity)	26
'san'iaikiakwe (Hunter's fraternity)	15
'hle'wekwe (Wood fraternity)	25
chi'kialikwe (Rattlesnake fraternity)	17
'ko'shi kwe (Cactus fraternity)	12
shu'maakwe (from the spiral shell)	18
u'huhukwe (Eagle-down fraternity)	38

* Following Stevenson's orthography (*Zuni*, pp. 148-50).

of the Navaho. Boys and girls were dressed like Navaho men and women, sang Navaho songs, and the artificial mustaches were exceptionally ludicrous.

Besides the six groups of katchina dancers associated with the kivas, there are twelve active medicinal fraternities, some of whom also participate in the above dances. They are shown in Table 2.

LEADERSHIP

After this digression for some factual information, we find another one-sided statement on returning to the problem of interpretation of Zuñi life. Avoidance of leadership in social life is a corollary of the lack of personal feelings in religion. If one is not interested in vision quest and if "Man is not lord of the universe," so that "The forests and fields have not been given him to spoil. He is equal in the world with the rabbit and the deer and the young corn plant"¹⁰—if all this is true, what is more natural than the supposition that leadership among men is not desired. But here is just a case in which the premise is correct enough while the conclusion does not necessarily follow.

Dr Benedict reports that a Zuñi is afraid of becoming "a leader of his people" lest he should "likely be persecuted for sorcery," and that he would be only "interested in a game that a number can play with even chances," for "an outstanding runner spoils the game."¹¹ The basic fallacy seems to lie in the tendency to reason with the logical implications of one's own culture. In the competitive Western world where one is brought up to assume that the world is made for his exploitation and where, if one does not push ahead, one is surely pushed behind, it is certainly logical that lack of personal acquisitiveness implies the denial of leadership. But in another society where a mutual give-and-take is more harmoniously assumed among all the beings of the world, one might be as humble as may be and yet exercise high power of discrimination among differences and values. Thus leadership is naturally assumed by those who are recognized by their associates, as well as followed by others who do not see in the act of following any degree of humiliation. The problem is not the contrast between leadership and its denial, but the valuation of the ways and means of achieving it. In any face-to-face community it is safe to assume that no individual with common sense will try to make himself ridiculous by seeking what is obviously beyond his reach, and that even the most eager and legitimate aspirants to high position will make the ordinary official declination

¹⁰ Bunzel, *op. cit.*, p. 488.

¹¹ Benedict, *op. cit.*, p. 99.

of an offer. Modern societies have asylums to take care of the insane, but a primitive community would have to charge the mentally dangerous with sorcery in order to follow the policy of "safety first" for the communal welfare.

That there are so many public functionaries with well-guarded prerogatives in Zuñi is a proof of the existence of social ambitions. Dr Bunzel reports a case in which an old priest refused to give information as to the order of events in a rarely performed ceremony, although he was persuaded to reveal a particular prayer for a consideration. "In Zuñi a 'poor man' is one who has no special knowledge or position in the ceremonial system. A 'valuable' man has knowledge and prestige."¹²

A healthy amount of ambition is in existence in any living society. Only the means of acquiring prestige and realizing ambitions are different, being culturally conditioned. Once agree to play the game, it must be played according to the rules of the game. The rules are different in different societies, but their existence is universal. With reference to Zuñi in particular, not only do ordinary forms of struggle for individual supremacy exist, but violent forms also occur once in a while. When Dr Hodge was excavating the ruins near Zuñi, a Protestant Zuñi got the information from him that a sacred object valued by the people was but a small figure of St. Francis, inherited from the early Franciscan padres, and he used this information to discredit his Catholic opponents to his own advantage. Previous to this the priests of Zuñi valued the object as indigenous, and with this discovery a strife of immense magnitude took place between the Catholic and the Protestant elements. Backed by the victorious party, the particular Protestant assumed the governorship of the reservation. More recently a high priest put himself above the other priests who were much more respected, and his means were pure politics which would have been beneath contempt in earlier days. His underhand campaign was linked with that of a new governorship whose incumbent was to be his right-hand man. Both were successful, and the general public was sharply divided between the advocates of the old order and those of the new politicians.

DISCIPLINE

Another case of oversimplified interpretation of Zuñi is in connection with child behavior. The universal idea of the students of the area is that the child is not chastised at all and behaves well automatically. This sounds strange to those who have had much experience with children in any other culture, but it is more or less accepted by all on the authority of universal

¹² Bunzel, *op cit*, p. 617.

agreement. Here is a typical passage from Mrs Stevenson, which is by no means an isolated example. One can read almost the same remark in any writings on Southwestern ethnography whenever such a topic is commented on.

The Zuñi child is rarely disobedient, and the writer has known but one parent to strike a child or to use harsh words with it. The children play through the livelong day without a quarrel. The youngest children never disturb or touch anything belonging to others. In years of experience with the Zuñis and other Indians the writer has never lost an article through them, either of food or otherwise.¹³

Because of the universal acceptance of this sort of judgment, those who are sympathetic with the Indians tend to idealize Indian life in this particular, and those who are hostile, especially the White traders, say that the Indians have no discipline from the time they are babies so that even the grown-ups are just spoiled children. Here again, what seems unjustified is not so much the observation as an interpretation based on an incomplete recognition of the factors involved. The observers are easily led astray by their own background in supplying the missing logic with their own.

The working mechanism seems to be something like this in Zuñi. First, the responsibility of supervision does not fall too heavily upon any one individual. All the members of the family besides the parents coöperate to see that the child behaves well. In fact, any member of the community who happens to pass by will say something to correct some misbehavior of a child. Confronted with this united front of the adults, so to speak, the child does not have much chance in trying to play one against the other. And if he is not unduly constrained, why should he make it unpleasant both for himself and for others? It is often observed that a very obstreperous child is easily hushed by a slight sound of any adult, in fact, by any facial expression which is seen by the child. It would be a wonder indeed that such things happened were there not sufficient conditioning beforehand. Second, in spite of all signs of parental love, the children are allowed a much greater independence in the free world of their own. The parents do not fondle them beyond the necessary physical care. To get bodily enjoyment by caressing the baby as a plaything and calling this love is not the pattern in Zuñi. Early in life the baby is put in a cradle for most of the time. He is often nursed in the cradle. As soon as he can play by himself, he joins the other little fellows and is out of sight of the threshold of the family. He comes back for food or for attention whenever he feels the need for it. He enjoys the world of his associates most of the time, and when he is near his parents he does

¹³ Stevenson, *The Zuñi Indians*, p. 293.

not feel the need to show off to please them or to demand too much from them. They are taken for granted as the source of his wellbeing. Third, chastisement does occur once in a while, as acknowledged by Mrs Stevenson. But it is done deliberately and effectively. There is no fussing around on the part of the mother, nor is there endless talk among the adults so that the child is encouraged to be mischievous by giving him so much publicity and attention. It is with the introduction of a new educational philosophy into Zuñi that some of the parents are beginning to feel uncertain of their children. Formerly the children had one well-recognized authority, but lately they are made to understand that missionaries and school teachers are trying their best to discredit their parents. And the new educational philosophy referred to is the fear on the part of the educators of the Fear lest it be instilled in the tender minds of children. The teachers are afraid that the children be made afraid. In other words, the family is losing control of the child, and the school is not adequate yet to handle the situation. This is no place to deal with this fundamental problem with any degree of fairness, but the point is that here is a problem due to transition.

To instill in the child all the education for daily life in a sense more pervasive than formal school education, there is the religious system of the people as a fourth factor. As early as the child is able to recognize anything he is subject to the impressive dramatic dances coming regularly the year round. To him, of course, the performance is undertaken by supernaturals. The unmistakable moral derived from all these is that they will be angry over any misbehavior. When he is initiated into one of the dancing societies, there is more formal lecturing on the moral philosophy of life, and he is whipped as a sign of purification. And above all, there are special functionaries among the supernatural beings, whose duty it is to punish the misbehaving. Apart from those who are purely religious guardians, each kiva has an atole, either man or woman, to take over the unpleasant job of frightening any notorious child on any necessary occasion. Sometimes it is done in connection with occasions of dance ceremony. I happened to see one of the katchina gods in full regalia go to a family in late evening after the public service was over. He had long hair covering his face and a large sword in his hand. A child, supported by two elderly women, presumably his mother and maternal grandmother, stood before this frightful figure, who made a long speech and all sorts of gestures with his hand. Finally he seemed to be satisfied with the intent look of the child and the assurances of the women behind him, and got himself out after receiving the blessing of the family in terms of prayer, prayer meal, and material gifts. The child must have been notorious somehow, and that impressive scene, I believe,

must be a lasting memory. Again, it must be pointed out that the present interest is not in the wisdom of such a procedure, but in the actual mechanism used to mold the child's behavior. It seemed a sound policy, though, to have the child supported by the helping hands of two elderly women, while he himself was standing on his own feet.

MAN AND WIFE

The next point of my interest in the interpretation of Zuñi life is stimulated by Dr Kroeber. In speaking of woman's title to the house, he has the following remark:

When a building is pulled down, it is the men who do all the heavy work. When it is re-erected, or an entirely new house built outside the old town, the men quarry and lay the stone, cut and lay the roof logs, and carpenter the doors and windows; the woman's part is auxiliary throughout, except for the light labor of plastering, in which she holds sway. Yet when a man has built such a house, and he and his wife quarrel and separate, even though for no other reason than her flagrant infidelity, he walks out and leaves the edifice to her and his successor without the least thought of being deprived of anything that is his. Men have shown me the houses that they have put up for a wife who subsequently installed another man as her husband, and have pointed out the glass windows, which they had purchased from the store-keeper with their own earnings, still in place, but the information was given casually, and without implication of injustice being involved.¹⁴

From the standpoint of Western culture, this is extraordinary indeed. And Kroeber goes on to say that "the Zuñi does not have an inkling of having been chivalrous in such an abandonment" and that "his conduct is as much a matter of course as resigning oneself to anything inevitable, like a cloudburst washing out one's cornfield." But a more intimate knowledge proves that there is no reason whatsoever for "an inkling of having been chivalrous in such an abandonment." The truth is this. A man has no worries about a house. If he is married, he lives with his wife. If divorced, he either goes back to his parents (his mother's house) as before his marriage, or to his sister's home in case of the lack of the former, or he is married into another woman's family and lives with them. A widower without any near relatives, such as parents, sisters, or daughters, is taken care of by any maternal relative. And further, there is the question of responsibility, over and above that of a property claim. Bunzel has this apt statement: "With his departure obligations cease, and his successor fathers his children."¹⁵

¹⁴ A. L. Kroeber, *Zuñi Kin and Clan* (Anthropological Papers, American Museum of Natural History, Vol. 18, Part 2, 1917), p. 89.

¹⁵ Bunzel, *op. cit.*, p. 477.

From this angle, he has nothing to lose, but everything to gain. However, he is not unduly favored by his culture requirements. If he leaves behind all his children to be fathered by his successor, he is likely to be supplied with other men's children to be fathered by him when he joins a new wife. Individual cases may be fortunate or otherwise, but in the mill of the community mechanism no one set of people can afford to be so obviously favored or disfavored, if the community is to persist.

Although there is no intention of presenting factual information in this paper, it may be of some interest to note that, of 1420 case records of the Zuñi (the total population is 2036) divided among 219 households, there are 14 houses owned by men, all the rest being owned by women. Men own houses when they have absolutely no other relatives to live with, when their wives come from other tribes than Zuñi, or when they want to follow the White pattern of establishing families of their own, not to be bothered by the wives' relatives. Of the same number of case records, it is found that there are 22 cases of patrilocal residence and 7 cases of clan endogamy. Although inter-clan marriage is the rule, these cases of marrying within a clan do not seem to draw much attention from the public, when they have decided to "marry anyway."

In order to view the family situation in a more complete context, let us examine sex relations more closely. Both Cushing and Mrs Stevenson give detailed account of the ways in which marriage was arranged. But "to marry Zuñi-fashion" today is synonymous with any physical cohabitation without public sanction. Although marriage certificates issued by state or reservation officials do exist, they represent cultural distinction rather than sanction of marriage as such. Marriages of this sort are not many. Fetching water from the well or cisterns is, as of old, a good opportunity of arranging for a liaison. The difference lies only in the fact that water vessels of pottery were once on the heads of the maidens and now water buckets are in their hands. Young men scatter around, either singly or by twos and threes, either behind street corners or against fences, with large sombreros to shade themselves when they do not want to be recognized. Such a scene begins in the early evening and lasts until everything is perfectly dark. Then they go back home for supper. After that the unmarried ones make night calls according to arrangements made earlier in the evening. This accounts for a late supper as a rule. One minds one's own business, so far as the boy is concerned. His favor is determined by the girl, whose ex-lovers may be quite friendly to one another. But during the evening manœuvres between the sexes in general the maidens are not simply interested in love-making, but each girl is equally interested in spying on the affairs of her lover. No

one hastens in the evening, but walks leisurely and with a great air of casualness. A visitor cannot be aware of this until with sufficient time it inevitably dawns on him that open-air evenings at Zuñi are magically charged, that everybody seems to be sneaking around in a sneaking atmosphere, with occasional inquisitive eyes cast upon the stranger who is not in harmony with the general setting.

Night calls are made by men with or without intention of marriage. One Zuñi youth, who has been a widower for some time, says that had he sufficient money he could arrange to sleep with any young woman whose husband happened to be away or who was not married. "This is not boasting," he says, and seems to corroborate the impression of White residents in general. The parents of the girl give informal consent by not objecting to the youth's presence. Scandals arise only when the parents are not in favor of him. They may set to work in favor of one man at the expense of another. The parents of the youth do not care where their son finds living quarters, until they are informed that a relationship of some permanency is established. Lest some substitute should take his place during his absence, a husband will try to stay home as much as possible. Sheepherding requires camp life, but as it is not yet the fashion to take one's wife to the camp, one would try to remain away from the sheep. A boy as a rule is found to attend them. A group of sheep with a burro (which is displaced by the horse for any other purpose and is not seen in the pueblos), herded by a young boy or boys in cooperation, and visited once in a while by the owner to take back one or two sheep for food, is the usual arrangement.

Infidelity seems to be one of the major causes of the rapid dissolution of marriage at Zuñi, and certainly is the major cause of all the petty troubles among the young people—troubles which exhaust their interests beyond the immediate horizon of rivalries. These difficulties are not between the girl's lovers as such, but appear as competitive efforts of each sex to outwit the other and as friction indirectly produced among men who are otherwise related. The local government can settle any trouble of daily life except that connected with sex relations, to which it refuses to give a hearing. Thus below the calm surface of Zuñi life, most of the individuals have some marital trouble or other, and it is a rule rather than an exception to find that each has more than one matrimonial history. Two cases of divorce came to my immediate knowledge. They occurred in one family; one man returning to it after he had left his wife in another household, and another leaving his wife left behind while he found temporary lodging with his maternal relatives. The first was angry with his wife because she was jealous of his "alleged" intimacy with an unmarried girl. The one who

walked out was accused of spending money on somebody else, while he himself was dissatisfied with the "meanness" of the family, which depended on his money for securing credit from a White store to get daily supplies. I mention these two cases, because they illustrate the general situation—particularly the latter case.

In this connection, it is curious to note that characteristics of a joint household based on the kinship principle stand out very strikingly. This in spite of the fact that "the Zuñis do not have large families;"¹⁶ the average size of a Zuñi household being still about seven and one half persons as found by Dr Kroeber.¹⁷ (Yet an unusually large household of over twenty members is not at all conspicuous in the minds of the Zuñi.) What we find as an attitude typical of the wives of brothers in a Chinese family is surprisingly comparable to that of the husbands of sisters in a Zuñi family. I cannot resist the temptation of making a comparison, in spite of my conscious effort to keep away from any irrelevant associations. While Chinese wives are married into the husbands' family, or rather the husbands' parents' family, the Zuñi husbands are married into their wives' parents' family. It is true, as Dr Kroeber has pointed out, that the Zuñi "are not woman-ruled people" and that "the position of woman is not materially different from that which she occupies in nations of non-matriarchal institutions,"¹⁸ but to a Chinese the role of Zuñi women seems much more important. It is not correct to say that woman rules man in Zuñi, but what is true and important is the fact that woman is not ruled by man at all. To have an abstract statement of this sort does not mean anything, but the realization of the carefree atmosphere surrounding Zuñi womanhood carries significance. She is the naturally protected person in her own home. It is the husband who must make the necessary adjustment. And this makes all the difference in the world. She and her unmarried brothers and her sisters, either married or unmarried, have only themselves to care about as far as external behavior is concerned, while her husband has to be considerate and calculating. If the others are slow in coming to the table, for instance, he must not show signs of eagerness and impatience. In case he has something in mind to do and his father-in-law has something else to let him do, he will inevitably conceal his own intentions and comply with the desire of his father-in-law. If he is not quite satisfied with his wife's sister's husband, he has to be tactful and not too frank. All these circumstances do not imply that the other people need be malicious. A one-sided

¹⁶ Stevenson, *op cit.*, p. 293

¹⁷ Kroeber, *op cit.*, pp. 123-24.

¹⁸ *Ibid.*, p. 90

adjustment is enough to make his situation not as comfortable as that of others. Thus he is quite comparable to a Chinese woman who is married into the man's family. She may be well treated, yet the very fact of *being treated* is the core of her difficulties. She cannot take anything for granted, however well she is treated. It is she who makes the adjustment to her husband, her parents-in-law, her husband's sisters and brothers, and his brother's wife. We are likely to believe in China that the petty troubles among the wives of the brothers are the result of definitely womanish qualities. It is a revelation to find the husbands of Zuñi sisters in similar difficulties, and what is more, such difficulties are due to similar adjustments irrespective of sex. An American woman may find it strange that co-wives could manage to live together at all, and it is equally strange for a Chinese to see the friendly relations between the ex-husbands of a particular Zuñi woman. America seems to lie in between in making emotional judgments, but a Chinese must actually see the matrilineal community at Zuñi in order to realize with any degree of vividness that a woman can be the carrier of a clan, which would become extinct were there no longer women members. So long as community life is a kind of symbiosis and is human, individuals must observe the rules of the game, once the rules are set.

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COOPERATIVE RICEFIELD CULTIVATION
AMONG THE SIANG DYAKS OF CENTRAL
BORNEO¹

By JOHN H. PROVINSE

I

THE ethnological reports on the primitive peoples of the world are regrettably lacking in the amount of information they contain pertaining to primitive labor. Nowhere is the literature more scant in this regard than in Indonesia, particularly with respect to the cultivation of the rice fields. In the reports on these peoples one seldom finds more than the merest mention of the actual labor of clearing, planting, harvesting. Full and elaborate information is infrequently encountered as to the purely technical processes involved, the tools used, the technique of felling, or planting, or harvesting, with usually quite careful descriptions of the ceremonies and rituals which appear to be such a necessary adjunct of all tillage work and without which one gathers that the economic organization would collapse. But when one endeavors to find out who works, when they work, how, for how much or for how long, one finds either no mention at all of the actual conditions, or has to be content with a statement or implication that the working of the fields either is a very simple, communistic endeavor, in which all work and share alike, or that it is done with the help of relatives, friends, and neighbors.

Ricefield cultivation in Indonesia is of two types: irrigated and non-irrigated. The former type, which may or may not be terraced cultivation, is, with few exceptions, employed by the more advanced peoples of the region who have had contact with Islamic, Indian, or Western civilization; the non-irrigated type, which with considerable certainty can be considered the earlier and more primitive, is practiced for the most part by the more backward and peripheral groups, inhabiting the less accessible, mountainous, and heavily wooded regions.

Though one may find throughout Indonesia many minor variations in the non-irrigated cultivation, in general this "dry" culture, known also as "hill" or "slash and dibble" culture, and locally as *ku*, or *kaingin*, or *jhum*, or tavy culture, presents very much the same features wherever it is found. It consists in making, during the dry season of the year, a small clearing in the jungle, and planting, at the beginning of the wet season, a few grains of rice into holes made with a sharp-pointed planting stick or dibble. With few exceptions the population of these isolated regions is exceedingly sparse

¹ Rewritten portion of a Master's thesis accepted by the Department of Anthropology, University of Chicago, 1931.

and the amount of land available is practically unlimited. A man is therefore under little compulsion to use the same land over again, year after year, or ever again, unless he so desires.

The clearing of the brush and trees is done not only to provide ground space in which to plant, but also to remove the large trees and branches which shut off the sunlight. The same sunlight, however, which nurtures the rice crop, also brings to life innumerable weeds and grasses, against which the less hardy rice cannot maintain itself for more than one or two years, after which the field is abandoned and a new clearing made. After a few years, by which time the new jungle growth has attained such size and strength as again to crowd out the weeds and grass, the old field may be recleared and again planted. Often as many as fifteen or twenty years will elapse before the old field is again used.

In selecting a site the side of a hill or a gently sloping piece of ground is usually, though not necessarily, chosen. Ordinarily the clearing is begun by cutting away all the underbrush, small trees, grass, and creepers. Then, beginning at the bottom of the slope or at one edge of the area to be cleared, the remaining trees are notched rather deeply, sometimes on but one side, sometimes on both uphill and downhill faces, but are not completely severed. Certain large key trees near the top of the slope or at one side of the slashing are then felled in such a way as to fall upon and carry with them those trees already partly cut through. In this way a comparatively large plot, sometimes as much as an acre, can be crudely cleared in a short time.² The larger branches of the felled trees are then lopped off and the entire area left to dry out, the length of drying time depending upon the size of the trees felled and the weather. A week or ten days of exceedingly dry weather is often enough to prepare a field for burning, more frequently a month is required.

One burning, especially in plots being used for the first time and where large trees have been felled, is often not sufficient to reduce the large trees and branches, in which case the charred debris, or such of it as can be moved, is collected into piles and burned again. Even such a second burning will seldom clear the field of a great number of half-burned tree trunks and enormous roots, but it increases the amount of ash available for, and knowingly so used for, fertilizing the soil.

As soon as the burning is finished, which, unless a second burning is

² Here are some notions on Indian soil, for example among the Sama Nagas, where the trees are not felled. Here the branches are trimmed off except for a small tuft left at the top of the standing old tree trunk. Within two or three years such trees will branch out again. It is this, or that, that the farmer cut off the tree completely.

necessary, can usually be accomplished in one day, the field is ready for planting. The object is, of course, to get the seed into the ground immediately before the rains come. Small holes an inch or so in depth are made with a sharp-pointed stick and three or four kernels of rice (most commonly called padi) dropped into them.³ Sometimes the holes are covered, sometimes not; in either case a certain loss in the seed planted is always expected and experienced from the forays of the birds. If the seed has been planted too long a time before the rains set in, the seeds may not sprout.⁴ Or, again, it may turn very warm after the first rains and so bake the soil that the sprouts cannot come through the hardened crust. If such happens more padi must be sown.

Once the fields are planted and the rains have begun, little more is done to them except to guard against the birds and jungle animals, monkeys, wild pig, deer, rats, etc., which come in, particularly during the ripening season, in great numbers. Rude bamboo fences are often built around the plots, scarecrows are employed, and dogs frequently assist the watchers—usually the women and children—in keeping the wild animals out of the fields. Weeding is done occasionally among some of the peoples, but it is a very disheartening task and no great effort is expended on it.

When the grain is ripe, some five or six months after planting, it is harvested laboriously with a small crescent-shaped reaping blade, the seed padi for the following year being selected from the first rice harvested. After threshing, the unhusked rice is usually stored in specially constructed granaries in the fields or in the nearby village, in large bin-like baskets underneath the village house, or in smaller baskets in the small ricefield house; this small house being a characteristic feature throughout Indonesia and often being occupied for several months of the year by the owner of the field.

When, from this generalized picture of the agricultural pattern, which with minor variations will apply with considerable accuracy to the tillage activities of all those Indonesian people engaged in the "hill" cultivation of their ricefields, we turn to the labor involved, its cooperative nature, its amount and kind and organization, the present literature affords very little. A review of the published material discloses that among three of the groups, the Bontoc, Bagobo, and Achinese, an exchange of labor is referred to, but one reads in vain to see how this exchange is organized.

³ Among some of the peoples the seed is sown broadcast, the seedlings being raked in, sometimes not. The planting stick method is used by the Dyaks.

⁴ Occasionally water is poured into the hole at the time the seed is put in, to insure sprouting.

Wages are intimated for two of these same groups, the Bontoc and Achinese, as well as for the Tinguian. Among the Subanun, Muruts, Ibans, Duhoi, Angami Nagas, Ao Nagas, and the Tanala of Madagascar, we find that cultivation is carried on "with the assistance of neighbors," "by friends and kindred," or "by groups of relatives." Assistance procured by providing a feast is reported for the Achinese, the Angami Nagas, and is intimated for the Bontoc Igorot. Two groups, the Sakai and the Moi, are reported to be communistic in the development of their fields, and such is also stated for the Muruts. Only one report can be said to be anything more than suggestive—that of Mr Hutton on the Sema Nagas, where one obtains a rather good picture of the "gangs" upon which the cultivation of the fields largely depends.⁵

The following section attempts to supply some of the details of the general tillage pattern and the labor organization involved, based upon information secured by the writer in 1929 during a three months visit⁶ to the Siang Dyaks of central Borneo, during July, August, and September, the time of year devoted to the clearing of the jungle for ricefield plots.

II

The Siang people of central Borneo occupy a fan-shaped sector spreading northward from Poeroek Tjahoe on the upper Barito River in longitude $114^{\circ}20'$ – $114^{\circ}40'$ East, all within less than one degree of latitude south of the equator. There are approximately 2500 adults scattered in some fifty small villages over an area not exceeding four or five hundred square miles. The country is quite hilly or mountainous, covered with thick jungle growth, and traversed by numerous small mountain streams which flow either north or east to the Laoeng, or south to the larger Barito. It is along the smaller streams, many of them not navigable even in the small prahus of the Dyaks, that most of the Siang villages are found.

Culturally these people resemble the other Indonesian, or non-Malay, non-Mohammedan peoples of Borneo. Each village consists usually of one

⁵ It is at once apparent that but a few of the many so-called Indonesian peoples—slightly more than a dozen—are mentioned in the preceding summary. This does not mean that the writer has neglected the reports on other groups, but that these reports fail to contain even a reference to the labor employed in the fields, and the mere enumeration of them is pointless.

⁶ The writer was attached at the time to the All-American Mohawk Malaysian Expedition, under the leadership of Theodore Seelmann, and financed by the All-American Mohawk Radio Corporation of Chicago. To this company and to Mr Seelmann the writer is indebted for the opportunity afforded him to gather the information here presented. Most of the observations were made at the village of Nono Kliwon on the Toepoch river, an average Siang village of eighteen families two days by trail from the Dutch outpost at Poeroek Tjahoe.

long house, raised ten to twenty feet off the ground on hardwood piles and divided lengthwise through the center, the rear half being again divided into the family rooms and the front half left undivided to form a long hall or balcony. Each village has a head man, or pumbakal, who is chosen by the adult men of the village with Dutch approval and whose official acts usually reflect the sentiment of the men gathered in informal council. There is no intervillage organization, though chiefs of certain villages are recognized as having more power in Siangland than others.

The family is the basic unit of the social organization, kinship is the most important social and economic tie, but a clan or gens organization is unknown. Marriage is customarily, though not necessarily, extra-village, is matrilocal and usually monogamous. Sickness and disease are caused by the presence in the body of the sick person of certain malignant spirits, in the casting out of which the services of medicine men, or blians, are employed. After death a soul does not return to its accustomed resting place until after the death feast, or tiwah, is given by the surviving members of the family. This may occur a year or more after the death, and is a time of drinking and promiscuity into which nearly everyone enters. The rice wine is also drunk to excess on other festive occasions. Head-hunting, though once important, is no longer indulged in.

Rice and wild pig are the main articles of the diet, supplemented from time to time by fish, jungle fruit, roots, honey. The wild pig is hunted with dogs and spears, and though the blowpipe and poison dart are utilized to some extent, the spear is the main hunting weapon. Large knives, known as mandaus or parangs, are used for cutting underbrush and cleaning game, as well as for much household work. Small axes and adzes of iron, hafted to a springy root or piece of rattan, are used for cutting the big trees. Ironworking, using the double-cylinder forge, is well developed.

The above will suffice to place the Siangs in the general Indonesian picture. Let us turn now more directly to the subsistence life. Almost the entire life of these people is concerned with wresting a living from the jungle, hunting the birds and animals, collecting fruits and vegetables, or clearing the land to provide space and sunshine for the cultivation of their main crop, rice. The hardest and most important of these activities from the point of view of the effort expended, though not from the amount of time each year devoted to it, is the clearing of the jungle land for cultivation.

The amount of land available for cultivation is practically unlimited, though the people of each village rather definitely regard the land surrounding it as belonging to the men of that village for purposes of cultivation. Individually, however, they do not regard themselves as having anything

more than a temporary claim to use the land. As long as a man wishes to use land which is being cleared or has been cleared by him, no one can take it from him. If a man abandons a used plot after its first year, and moves to another without manifesting in some way⁷ his intention to retain his use of the first plot, someone else may come in and cultivate the old field. There is a well recognized feeling, however, that if an abandoned field is wanted for use by another within one or two or even three years, permission must first be obtained from the user. After two or three years usually anyone may clear and cultivate it.

The first problem that the Dyak faces in his cultivation is the choosing of a site, usually done at the end of harvesting in April or May, or before the beginning of the dry season in June or July.⁸ The site of the previous year is considered: if the soil appears not to have been exhausted, if a good crop has been realized the year before, and if the grass and weeds have not overrun the place, this site may be used again.⁹ If last year's plot seems undesirable, those previously used but which have lain idle for three, five, ten or more years, are considered. The roots and stumps and partially burned timber from the previous clearing may have rotted and fertilized the soil; many of the rocks may also have been cleared away previously. Such a plot also has the advantage that the new growth of trees will not have attained such size as that in the undisturbed jungle.¹⁰

If no old site is considered suitable, a new piece of jungle must be cleared. In choosing this site attention is paid to the size of the trees to be felled, to the quality of the soil, the scarcity of rocks and roots, its distance from the village, the availability of water for domestic purposes. If the roots are many and very large, rice planted among them is likely to be stunted. If the trees are very large ones, after felling they will require con-

⁷ By planting javau, vegetables, or perhaps rubber, or by simple announcement of his retention of an interest in the plot.

⁸ The dry season in the interior of Borneo is not as distinctly marked as in many other parts of the archipelago. From late June until October there was some rain on every day except eight which the writer spent in the region. The rainy season proper, however, usually begins in October, becomes heaviest during the months of December, January, and February, and tapers off again into comparative dryness in April or May.

⁹ Such is infrequent, however, and only two last year's sites were being used again at the village of Nono Kliwon during the writer's visit. Two other last year's fields were being devoted to secondary crops.

¹⁰ At Nono Kliwon the chief's present ricefield site was last used six years before, when he himself had a crop there. The trees were about eight to ten inches in diameter and there was not much grass or underbrush to be cleared. Of eighteen fields being cultivated around Nono Kliwon, two were fields used the previous year, five were fields that had been used five to ten years before, and ten were in previously uncleared jungle.

siderable time to dry out sufficiently to burn. A slightly sloping or level piece of ground is more desirable than a steep one; it is essential also to stay away from too high or too greatly exposed ground, for the winds in January or February are often strong enough to knock down the rice plants if they are in an unprotected location.

The site having been chosen, the first work is the clearing of the underbrush, creepers, grass, and small trees. After these have been cut, the debris is allowed to dry out thoroughly—a few days being sufficient—before the larger trees are felled upon it. Then the larger trees are attacked with the small axes, partially cut through until the top or the edge of the clearing is reached, when the peripheral trees are completely felled as previously described (see page 78). Of course, such method of felling does not perfectly nor completely clear a plot, and a good bit of hard work still must be done felling those trees which have withstood the avalanche of the first felling.

After the trees are down many of the larger branches still extend high into the air, and these must be cut off in order to allow for more complete burning. This leveling process, known as *mehera*, is almost as strenuous work as felling the trees originally. After the leveling the trees are allowed to dry out and the plot is burned, on a windy day if possible. A second burning may or may not be necessary. The fields are usually ready for planting by the middle of October.

Each family, that is, a man, his wife, and their unmarried children, has its own ricefield. The fields are not necessarily located in the same general region, nor in the same direction from the village, though in former years when head-hunting and raiding parties had constantly to be taken into consideration, it was usual for all the fields to be very close together if not actually adjoining. At present, two or more families, sometimes as many as ten or twelve, often go together in the preparation of the same area, cooperating through all the different stages of felling the trees, burning, planting, watching, and harvesting.¹¹ If sufficient good land is available in one location, it is desirable to join together in cultivating it; for though head-hunting and raids no longer give the people much concern, joint cultivation supplies a companionship and economic advantage that is very desirable. During the growing season, when the animal pests are bad, watching of the fields can be done turn and turn about if several families are involved. Further, as one man at Nono Kliwon expressed it, if there are

¹¹ At Nono Kliwon, during the year in which these observations were made, six families worked together in one large plot; two other families were joined in another. The rest of the families in the village—ten in number—worked separately, their fields being widely separated and in different directions from the village.

several fields together, it is not likely that the animal pests will ruin any particular crop completely, but rather will injure all partially, whereas if a man is alone the pests may clear out his entire field.

But it is not always possible to join together. There may not be sufficient good ground in one place for all to get a good crop, or there may be different opinions as to the best locations for the year, or there may be individuals, as there were at Nono Kliwon, who prefer to work their plots by themselves. When, however, several do join together in the selection of a site, each family has a particular portion which is its own, the boundaries of the plot being approximately determined at the time of selection of the site and more definitely located during the time of felling. It is usual to leave certain small trees standing as markers to define the boundaries of plots.

Occasionally there is a dispute over boundaries, due to failure to mark the trees which define the intended plot, or in straightening out the edge of a plot, after felling, one man's markers may encroach upon ground already included within another man's line of trees. Every case that was brought to my attention at Nono Kliwon was said to be unintentional and was easily adjusted between the parties.¹² Infrequently the dispute cannot be settled by the parties to it, in which case it is carried to the headman of the village whose decision in the matter, sometimes with, sometimes without the aid of the older men, is abided by.

The amount of time necessary for clearing the jungle varies considerably from one plot to another, dependent upon whether an old plot is being used or new ground has to be cleared. If a last year's plot is made use of, there is usually nothing but a small growth of grass and trees to be cleared, which can be accomplished in a few days' time without a great expenditure of effort. A field with somewhat larger growth may require three weeks or a month for cutting, while a plot in previously unused jungle, with big trees to fell, requires six, seven, or eight weeks to prepare for burning.¹³

¹² However, one morning at Marowei, a Siang village to the north, one of the grown daughters of the family in which the writer was living came in sobbing, complaining that the man felling trees on the plot adjoining hers had put his mark on a tree on her piece of ground and claimed a small piece of land which she herself had cleared. Though the contested piece was but ten yards square, there was haggling for three days before it was settled, the man finally taking the disputed sector and agreeing to help the girl fell more trees on her plot, which he did.

¹³ Aboen, who was working among the big trees, spent seven weeks on his field when his foot was badly injured by a falling tree and he was forced to leave off work. He was then within a week of completion. Others who had already finished their felling took it upon themselves to finish Aboen's field for him, expecting no remuneration therefor.

When one visits a ricefield in the making, it is not unusual to find five or ten or even twenty or more people working on the same plot. This may be accounted for by either one or another of two methods commonly employed by the Siangs in working their fields. More frequently it is due to a well recognized system of labor exchange, known as *hando*. Usually by previous arrangement, but often not until the clearing is underway, several of the heads of families will club together for the purposes of joining their combined efforts in developing their fields. If ten men thus agree, they will all work one day in one field, the next day in another, and so on around until in an ideal case at the end of ten working days a man will have worked nine days on others' fields but will have the services of ten men for one day on his plot. Usually one day only is spent on any one field at one time, thus insuring that all fields will near completion at approximately the same time and that everyone in the *hando* arrangement will have approximately the same amount of cleared land for planting when the rains come. If the *hando* agreement includes only those families which have their plots adjoining, the same group may be seen working in the same area day after day, but *hando* may be entered into with others whose fields are not adjoining, and the people actually working in any particular plot do not furnish any conclusive evidence as to the ownership of the area. After a man has returned service for all services rendered him, if such has not finished his clearing, he may proceed alone, or may enter into *hando* with others. Nor need a man necessarily *hando*: he may prefer, and often such is the case,¹⁴ to work his field by himself, assisted only by the members of his family.

In this system of labor exchange, no clear-cut evaluation of a particular individual's services is made. Work performed by a man may be returned in services of one of the older boys of the family or even the man's wife, and though this may result in some inequality in the amount of assistance secured, it is "against Dyak custom to complain." If not satisfactory a man can decline to *hando* another year or can enter into combinations with other families.

In *hando* each man is supposed to provide his own food, but in practice this most frequently works out that the ones who are assisting on any particular day take their morning and evening meals in their own homes, but the owner of the ricefield where the work is being done furnishes at noon a

¹⁴ Aboen was working his field by himself, assisted only by his wife and sister-in-law. His objection to *hando* was that he always got very poor assistance in return for his own hard work on someone's else field. Pladong and Odoh also worked their fields without *hando*, saying they could do much better alone, both have grown children, unmarried, who assist them.

meal consisting of boiled rice and perhaps a little dry or salt fish or wild pig. Occasionally the workers leave for the field as soon as it is light in the morning, when breakfast is served to them in the field house by the owner of the field, the women usually accompanying the men to the fields to prepare the meal or meals.

Besides this exchange of a day's labor for a day's labor, a second means exists for obtaining help with ricefield cultivation. This other practice is known as haweh and is carefully distinguished from the exchange system of hando. Here a man who does not wish to return services for help rendered informally announces that on such a day he will have haweh at his ricefield and invites all those who are so disposed to come and assist him. Such services as are rendered him on that day need not be returned by him, but it is expected of him that he give a feast, or at least something more than the customary boiled rice and fish, during the day. It is a festive work-gathering, not unlike a Western corn-husking or barn-raising, and the owner of the field provides not only breakfast for all those who come, but at noon, in addition to bare necessities, provides pig or chicken, and usually tuak, the native rice wine. If the tuak is plentiful the working party may turn into a drunken rout before the day is ended, and probably very little work will be accomplished after the midday meal.¹⁵ The amount of work performed on such days, at least after the midday feast, usually is in rough inverse proportion to the amount of tuak available; but since a man's standing in the community and the success of his festive services are somewhat dependent upon his liberality at haweh times, it is incumbent upon him to provide rather generously for all who come to work. As the owner of one field at which haweh was being made said, "If I do not furnish plenty to eat and drink, I will become known as stingy, and people will not come to help at another time."

Haweh, of course, is not as reliable a method of procuring services as the direct exchange of labor, but on the whole considerable work can be accomplished in this manner, and with much less effort on the part of the owner than in hando. It is, of course, a practice which can be resorted to more frequently by the well-to-do—those who have a supply of rice, pigs,

¹⁵ At one haweh which the writer attended, all the men worked hard until noon felling trees, but at the midday meal nearly all drank too much and no more work was accomplished. The owner of the field did not complain. It was at this drunken party that the writer witnessed one of the very few quarrels which came to his notice during the visit. Pladong, one of the older men of the village, smeared pig oil into Koembit's hair during a bit of horseplay. Koembit, well past fifty himself, and regarded as the richest man in the village, resented it. He spent most of the evening in the writer's quarters telling what "bad manners" Pladong had.

chickens, and tuak—and it is a difficult thing for a poor man to secure assistance in this way. But if a poor man is able, by loan or otherwise, to secure the necessary food and drink for a feast, he is as much entitled to haweh as the richest man in the village, and usually secures as much labor return, oftentimes more, than the well-to-do owner. Haweh is utilized particularly by those who, for some such reason as sickness or enforced absence from the village, have been delayed in the clearing of their plots; then feast labor may accomplish much in a short time. Or it may be that a man will let his clearing drag along until all the others have finished their work; then there being no work to perform for the others on an exchange basis resort can be made to haweh if the man cannot accomplish the work by himself.

The two methods of procuring assistance described in the preceding paragraphs, and which we may well designate for convenience “exchange” labor and “feast” labor, are not restricted to the clearing of the fields but are utilized as well during burning, planting, watching, and harvesting. Both methods are also resorted to for other purposes than cultivation. When a man needs help in bringing in a prahu (dug-out canoe) or a torah (large memorial pillar) from the jungle, or in building a house, or in shaping a coffin, or preparing a grave, he may seek assistance from his friends and relatives, repaying the services in kind when the necessity for them arises, or with a feast which may be held at the time the services are rendered or at some future date.

The employment of others on a payment basis is not unknown among the Siangs, and although money or any other standardized medium is not found, payment in commodities or by “shares” is occasional. If during the preparation of a joint field, a man becomes ill, he may employ some one else not in the working partnership to work for him, paying him in rice or other commodity. And if he cannot employ others to work for him, his co-workers in the field must help finish his plot as well as their own, the man who is so helped being expected to repay later either a part of his harvested crop or by extra labor when he is able. If a man has an individual plot and becomes ill, he may make an arrangement for some one to work his field for him, sharing one-third or two-thirds, depending upon the amount of work necessary to be done for him. Or it may be an agreement whereby at the time of harvest the man who has assisted will harvest one day for himself, the next day for his friend, or, after all the rice is in, the harvest may be divided into two piles, one for each. But these methods of assistance are exceptional to the ordinary methods of exchange and feast labor. Occasionally help will be given unfortunate individuals by the other members of the village for which help no repayment of any kind is expected by the donors;

but such help is only tendered those whose misfortune has come through no misconduct on their own part and who are deserving of it.

With this rather simple picture of the organization of labor before us, let us look more closely at its background. There are many things extraneous to the cultivation of the fields which must be taken into account in trying to understand or to describe the organization of effort which these people have achieved. The circumstances of life of any two individuals in the society are never identical: some men have large families to provide for; some have no family at all, or only a wife. The children of some are small and helpless and a constant drain upon the family provider; some have grown children who assist materially in the economic struggle; some who have been expecting to count upon the help of their children lose them through death or early marriage. Through marriage or through inheritance a man or woman may come into such estate that the economic pressures, though never entirely removed, may be greatly reduced. Family factors alone, not to mention individual variations of ability, ambition, energy, health, preclude a perfect reciprocity in the Siang system.

The man is considered as the head of the household, the room in the long house where he and his family reside is referred to as his, and it is expected of him that he will provide to the best of his ability the rice, wild game, and jungle produce, while the women will take care of the children, husk the rice, make mats, and do the cooking. It is unseemly for a woman to do the hardest kinds of work, to fell large trees, to hunt wild pig, or even to bring in a load of firewood; and a man who spends his entire time around the house to the neglect of his jungle work is guilty of a breach of responsibility.

Boys and girls, until they have reached the age of six or seven, are not required to work in the fields. Most of their time is spent in play, swimming, trivial household duties, meanwhile absorbing from their elders the ways of life and the traditions of their people. Quite early, however, they become accustomed to performing such services as can be entrusted to them: simple house work, minding the younger children, guarding the drying unhusked rice from the pigs and chickens, occasionally bringing water from the river or wood from the woodpile. When they are able to make their way along the jungle paths and to appreciate the jungle dangers, they are allowed to accompany their parents to the ricefields where they do such tasks around the ricefield house as they are able, gradually attaining such familiarity with the surrounding forest and the use of the knife that they are allowed to search for rattan or bamboo or other jungle products that may be useful in native crafts. The amount of direct labor, however, per-

formed by the children up to the age of nine or ten in the cultivation of the fields is relatively insignificant in the general labor problem.

After nine or ten, however, many of the children of both sexes have sufficient strength of body and facility with the native tools to assist materially in the clearing of the jungle underbrush and creepers, preliminary to the felling of the big trees. Their services can be utilized in collecting debris for burning, to some extent in planting, to a greater extent in watching and weeding, and at harvest time. A noticeable division of labor occurs very soon, and the boys are seen to devote more of their time to the men's work, the clearing of the fields, hunting, fishing, and the girls to the women's work of cooking, tending the children, pounding rice, or mat-making.

Not until the boys are fourteen or fifteen years of age are they considered able to take their places with the older men in felling the big trees, difficult as well as dangerous work. Until such time as they marry, usually three or four years later, their services are rendered to their father's field or other work in his family. Upon marriage the man leaves his father's house and takes up his residence in the village to which the girl belongs, or if the marriage has been in the same village, in the long house to which the girl belongs. A father, therefore, loses his son's help upon the son's marriage, and though the son then becomes a member of his wife's family, his residence with the father-in-law is not of great duration—seldom as long as a year—and the father-in-law does not benefit appreciably in the service rendered him.

It is expected of the newly married couple that they shall set up their own establishment, in the wife's village, as soon as they are able. When this is done, usually with the assistance of the two families concerned, the girl devotes her whole time to the new home, and if children do not come immediately, she assists her man in the ricefields as much as her household duties will permit. Though the division of labor in the fields is not so rigidly drawn as to preclude a woman from helping in felling the big trees, or to preclude the man from assisting in preparing a meal, it is a serious reflection upon a man whose wife must work too hard in the fields.¹⁶

A man's grown daughters, like the sons, while still unmarried, devote a considerable part of their time to the work in the fields and assist ma-

¹⁶ Aboen's wife and sister-in-law, who helped Aboen in felling big trees, were very much ashamed to be forced to do such work. The wife also complained rather bitterly that she should have to carry in a load of firewood from the jungle. Aboen was by no means a lazy person, as reference to his record in the Appendix will show. He appeared to be one of those individuals who, despite the best intentions and tremendous effort, was frustrated by a lack of sound judgment and an undue share of misfortune.

terially in the family welfare and the economic status of the father. The services of these grown daughters can be and sometimes are exchanged, but the usual thing is for them to work only on their father's field.

Women who have lost their husbands through death, desertion, or other cause, and who have not been successful in procuring another are oftentimes forced to cultivate their own plots by themselves. Of course, some of them are fortunate enough to have parents or brothers or other relatives to whom they can look for help, but when such is not the case they must engage in the hard work of clearing their own ground, usually confining themselves, however, to those areas which have been previously used and on which the growth has not yet attained any great size. On the whole, the women can swing an axe as effectively as the men. When they do cultivate their own plots,¹⁷ they may enter into hando arrangements with their friends and neighbors, and may, if not too poor, which they usually are, secure feast labor. If a woman is left with several small children and has no relatives upon whom she can call, she is usually assisted by the others in the village, through gifts of rice and wild pig or by help in the clearing of her field; at least until such time as the children have become old enough to help her.

With the exception of the children, who until they are married are cared for in their father's house and the old people who no longer are able to do hard work in the field, everyone must cultivate a field of some sort. The old men and women are cared for by their relatives, or these lacking, by friends and neighbors, and though no definite reciprocal obligations arise from the help extended to them, the old men ordinarily contribute a helpful share to the family existence by gathering and stripping rattan, sharpening tools, carving out boards and troughs, tending the children; the old women by weaving, cooking, tending the children, or such other small duties as devolve upon the stay-at-homes. With these exceptions no one is exempt from the necessity of making a ricefield, not even the medicine man or the chief.

Among the Siangs as among other peoples there are certain individuals who are shiftless, certain other ones who do not regard with any great circumspection their obligations either to their family or to their fellow-workers. These people here, as elsewhere, somehow get along. No one of them, however, or at least no one of them who has not learned to emulate the shrewd Malays and Chinese can be said to live entirely by his wits, and without occasionally devoting himself to real labor in the fields or in

¹⁷ Three such instances came to the writer's attention during his visit. As many more instances were related by various informants.

the jungle. The Siangs, as other Dyaks, are notorious for their hospitality and visitors from other villages are readily accepted and provided for, sometimes for considerable periods of time. But if a man after considerable time makes no attempt or offer to reciprocate in some way for the hospitality afforded him, he is asked to move on. The Siang territory is a small one, communication between the villages is frequent, so that a man's shiftless ways quite soon spread to all the long houses and that man's reception becomes gradually less and less hearty. No really deserving person who through sickness or other misfortune has come to difficulty will be permitted to suffer or starve among the Siangs, but an undeserving person is seldom tolerated longer than is necessary to find out what he is.

III

Of recent years economists and other social scientists have justly complained that the material so far gathered from primitive peoples does not tell very much about what they wish to know. Descriptions of foodstuffs, primitive tools, weapons, or traps, the various techniques of cultivation, hunting, fishing, harvesting, etc., no matter how detailed or accurate such descriptions may be, do not furnish much aid in the solution of those problems which are nearest the hearts of the economists. How much of the life of primitive man is spent in work, in play, in idleness; how much in the discharge of obligations to his fellowmen? What is the motivation behind work and the business of making a living? What are the factors, environmental, social, religious, or selfish, in this economic drive? How do production, distribution, exchange, function? These are but a few of the questions to which answers are sought.

Fifteen years have now elapsed since Dr Malinowski, striving to correct the barrenness of ethnologic endeavor, ploughed new furrows in the sterile landscape of primitive economics, deposited and fertilized a few promising seeds, and went on to other fields.¹⁸ Inspired by him Raymond Firth has subsequently contributed an interesting investigation of the Maori in which he exhausts the extant literature on the New Zealand people to give us a correlated picture of Maori life from the economic standpoint.¹⁹

Dr Malinowski has effectively brought into further disrepute the already disreputable concept of a primitive economic man; has admirably hammered home the motivating importance of the semi-commercial ceremonial Kula in the Trobriand economic life; has disclosed the influence of magic in the economic activity. For all of these services his work has received the

¹⁸ B. Malinowski, *Argonauts of the Western Pacific* (London, 1922).

¹⁹ *Primitive Economics of the New Zealand Maori* (New York, 1929).

deserved plaudits of anthropologists and economists alike. But one wonders, after reading *The Argonauts of the Western Pacific* how these Trobriand people actually live. That they are impelled to work and that they are kept at it is diligently demonstrated to us, but when and how do they do it and by whom is it done? After all, arm shells and bracelets do not satisfy at least one very important vital need. The unique Kula exchange, like the excrescent potlatch of the Northwest Coast, provides an incentive for work that is paralleled in our own society by many vain drives to industry and accumulation, but after all, these socio-psychological drives are not the description of the economic structure that arises from them. There is no reason to believe but what in every society that is a going concern there will be found some socially determined drives that keep people at work and that these urges in any particular culture will take on different forms. To point out the variety of these motivations is an interesting, romantic, perhaps even a useful procedure: but if ethnology is going to realize its full responsibility to economic theory it must present for the economists' use objective material that goes beyond these individual drives and reveals social and economic intercourse as a thing in its own right, capable of being studied and understood in terms of societal balance, stress, and integration.

The purpose of the present paper is not the refutation of Dr Malinowski's socialized economic man, not an attempt to dispute the influence of magic and ritual in Melanesia and New Zealand, not a stricture upon the economic importance of ceremonial motivation. It has a different problem; that of describing the cooperative effort of a primitive group in the cultivation of its fields. It aims at presenting an apologetically meagre but what the writer believes a significant amount of data that will help to clothe the concept of social reciprocity in objective fact rather than in subjective impressionism. And incidental to that problem it will, the writer believes, appear that the organization of labor and production, despite its cooperative character, is not always or necessarily the complex and highly socialized phenomenon, shot through and through with magic and sustained by psychological drives of non-utilitarian nature which the two studies of Malinowski and Firth might lead us to think is the rule in primitive society.

It is easy enough to agree with the emphatic statements that self interest is not the only motivating force in native industry, and the rather self evident proposition that many other social, magical, and religious factors modify and condition this self interest. It is less easy, however, to agree when the influence of these other factors is so stressed as to give one the impression that self interest as a motivating factor in economic life plays

but a secondary role. The writer's experience in the field failed to disclose the overshadowing importance which has come to be attributed to these factors. Not, however, because they were neglected in the observation of the agricultural activity: in fact, an effort was made to see them and to note their possible conditioning effect. That they were not observed may be due to the short period of time covered by the observation, the unfamiliarity with the culture, or to the intellectual shortcomings of the observer, but the fact remains that the cooperative effort in cultivation herein described appeared to function under its own motive power to a much greater extent than it was motivated by external social and religious forces. In this fact resides the writer's justification for his separate treatment of the data.

The chief, the smith, the medicine man, though their respective offices or callings gave them certain prestige in the community life, and somewhat reduced the effort in cultivation which they must put forth, were not relieved from work. They labored in the fields along with the others, their work dictated by their individual physical needs or the wants of their families. The wealthy, though their additional resources made it easier for them to live than was the case for some of the less well-to-do, were no more exempt from work in replenishing their rice bins than was the poorest individual in the village. Idlers idled at their own risk.

Restrictions and taboos, the paralyzing effect of which on the economic existence of primitive people has so often been noted, are not denied for the Dyaks. Many of them were told the writer and many pages of notebooks were covered with them. To cite but a few: at marriage, the couple is restricted from work for three days; on the death of a close relative no work may be performed for seven days; if the relative is a baby or small child the restriction is reduced to three days. But it was further noted that an occasional taboo on activity for three or four or seven days is not at all a disastrous restriction in a society which does not regard leisure as an evil nor measure time as money; further it was noted that though these restrictions applied to "work," they did not preclude such male activities as hunting in the jungle, or collecting jungle produce, or doing odd jobs around the house, or such female activities as cooking, caring for the children, carrying water, all activities which have considerable importance in the round of economic life. And at the one death which occurred in the village during the writer's visit—that of a baby for which the traditional restriction from work is three days—the father repaired to his field the next day after the death, the breach of observance exciting no comment whatever in the village, causing no punishment to be meted out to him, and being explained

by the chief with the statement that "just now that family is very busy." Numerous as these taboos and restrictions may be, powerful as they may appear on paper, the facts of their existence seem to have been overworked and their effects on the economic life to have been overstated.

Likewise omens. The flight and calls of numerous birds, the actions and cries of certain animals, the occurrence of certain natural phenomena, are all regarded as prophetic and ominous by these jungle people. But it is a fact that during the three months in which the writer lived the everyday life of these people, no journey was ever postponed, no ricefield was ever abandoned, no work was ever laid aside for any other reasons, either observed by him or given as an excuse by the natives themselves, than reasons of utility, personal disinclination, or inclement weather. Not only was no ricefield abandoned after being selected, but no native to whom the question was put could refer the writer to anyone who had ever abandoned his field on account of inauspicious omens.

This is not to deny that these people do not consider themselves to be surrounded by a great spirit world, a world which for them contains many non-understandable agencies which can do many strange things. But this spirit world is of much greater importance to them in those phases of life which their limited knowledge precludes them from accounting for rationally—such phenomena as disease, sickness, birth, death, propagation—than it is concerned with the facts of their economic existence. True it is that when a man selects his field, omens are frequently consulted, that at planting time offerings are made to the spirits, and that at harvest a thanksgiving feast is held. But true it is also that a man selects his field not because the omens are right but because he sees in the plot chosen a suitable soil, an absence of stones and weeds and grass, the possibility of a fair crop, a possibility based not upon what the omens have said but upon his own past experiences with soil, weeds, and ricefields in general. Whatever help he can obtain from the unseen world of spirits in securing a good harvest by means of ceremonies and offerings he will gladly, hopefully, even fearfully put forth the effort to obtain, but he is neither so ignorant of the facts of life nor so oblivious of their practical operation as to believe that these spirits will fell the big trees in the jungle, or burn them, or plant his rice, protect it from the animals, weed it, and harvest it. Anyone who has watched a loin-clothed Dyak, armed with a small axe, attack a tree six feet in diameter and after an hour of perspiring labor, see him scurry for safety out of the way of the falling trunk, cannot come away from the scene with the feeling that here is a man whose every movement is regulated by some magical influence. Anyone who has seen one of them come home in the eve-

ning from his field, plunge into the river for a refreshing bath, consume great quantities of boiled rice, and stretch out on the floor of the long house balcony for a contented chew of betel-nut and lime, will not attribute to this man any inability to recognize and face the cold hard facts of existence, nor deny that he knows what work is and what it will bring him in the way of bodily sustenance at some future time, spirits or no spirits.

For two fortnightly periods the observer kept a day-by-day schedule of the activities of several individuals in the village where he lived, the better to secure an objective record of the everyday life. These accounts are embodied in the Appendix.²⁰ Though the investigation was abruptly terminated and the records are regrettably inadequate in many respects, such as they are they fail to indicate any great number of external pressures which drive the individual to his work. And for those who maintain that such bare records can never reproduce the subtle influence of these extra-economic pressures the writer can only say that the records were kept as fully as his observation of the facts of every-day life seemed to warrant.

APPENDIX

The following pages are the records of activity of six men and one woman of the Siang village of Nono Kliwon for two fortnightly periods during the season devoted to the preparation of the jungle ricefields. The continuity of the records is broken by an unavoidable absence of a week which detracts somewhat from their already meager value as an account of primitive occupation, but the records are appended as a sample of what may be done with such data kept over a yearly or longer period for a large number of individuals. Besides the concrete information which such records furnish, the data lends itself readily to statistical manipulation and analysis.

It is, of course, often difficult to record with exactness in such large units as a day every activity an individual engages in every twenty-four hours. An hourly unit would furnish a much more exact picture, but the returns from such minute tabulation are probably incommensurate with the added effort required. This difficulty is not a very real one in a record of Dyak activity except with regard to those days on which the individual remains at home. It must be pointed out, there-

²⁰ Attention is invited to the records of the Appendix as a method of securing objective information on primitive economic activity. Such a record maintained over a long period of time for a large number of individuals, men, women, and children, would provide an excellent framework for the consideration of the life of a primitive community. Firth has called attention to the need of such records in the following words: "But as a note for the field-worker, it may be here remarked that a most valuable ethnographic document would be a diary of native work from day to day, extending over a long period of time—say a complete year. This would provide most useful data in regard to the organization of activity and the seasonal distribution of occupation" (*Primitive Economics of the New Zealand Maori*, p. 56).

fore, that in the following records the designation "at home, resting," does not mean necessarily that the man on that day did not devote any of his time to any gainful occupation, but merely that the major portion of the day was spent in leisure; likewise, a man "at home, working," was not necessarily employed during all the hours he remained in the village.

The records are roughly accurate of the individual activity and are based not only upon the recorder's observation of what the man was doing but also upon what the man considered himself to be doing on the particular day. No attempt was made to begin the records until the observer had become well enough acquainted with the customary activities of the village and with the individuals selected to be able to keep the record by personal observation as well as on the testimony of the people themselves.

Several interesting things suggesting further investigation are brought out in the recapitulations. The chief of the village spent ten of the twenty-eight days hunting wild pig in the jungle, while Tatak, whose diet consisted in pork as much as did the chief's, did not hunt a single day, but spent double the amount of time the chief did in the ricefields. Almost a third of the time of the medicine man Seetak during the four-weeks period was devoted to ceremonials for the cure of the sick, yet for this he received no remuneration except one small gift of rice and on two occasions, when he performed at other villages, his meals. Medan spent eleven and one-half days at home resting but only two in the ricefields. This reflects not only the fact that Medan had his field on a plot that was used the previous year and hence the work was not so pressing, but also a certain streak of indolence which the writer observed in him.

Unfortunately, due to the abrupt termination of the investigation, the recapitulations were not made up until after the writer's departure from the field. Hence the many problems suggested by them could not be pursued further at the time. It would be especially interesting to have the records complete enough to demonstrate the actual working out of the labor exchange system among these people.

The record for Liwoei, the only female record obtained, is but a poor sample of the work performed by the women. Most of the women who, like Liwoei were without small children, spent much more time in the ricefields than did Liwoei, whose man was a very good worker. Those with small children were at home practically all the time, as were the old women.

RECORDS OF ACTIVITY

TATAK (male): age about 50, married; wife and grown son.

<i>Date</i>	<i>How Occupied</i>
Aug. 24	Working in his own ricefield
25	Ditto
26	Ditto
27	Working in Oeke's ricefield (hand)
28	Working in his own ricefield

	29	Ditto
	30	Home, resting (made strap for knife)
	31	Working in his own ricefield
Sept	1	Ditto
	2	Home, resting, wife's uncle visiting from another village
	3	Home in a m., resting, his own field in p m.
	4	Home, resting, half day, his own field half day
	5	His own ricefield
	6	Sahadan's ricefield (haweh)
	7-12	No record (observer absent from village)
	13	Home, resting
	14	Home; helped Kenting make coffin for dead baby
	15	Home, assisting Kenting (other work taboo)
	16	His own ricefield
	17	Home, resting
	18	Ditto
	19	Sahadan's ricefield (haweh)
	20	His own ricefield
	21	Ditto
	22	Half day his own ricefield; home resting half day
	23	Half day his own ricefield; home half day—sick
	24	Home—sick with dysentery
	25	Ditto
	26	Ditto (on his mat in the long house all day)*

Recapitulation

Number of days working in his own ricefield	12½
Number of days working in others' ricefields	3
Number of days hunting in jungle	0
Number of days home, resting	0
Number of days home, but working	3½
Number of days home, sick	3
	—
Total	28 days

* Tatak died a week later.

DAK (male): age about 40, married; wife and grown son, chief of village

<i>Date</i>	<i>How Occupied</i>
Aug. 24	Hunting wild pig (alone); got three
25	Working in his own ricefield
26	Working in Oeke's ricefield (hanelo)
27	Ditto
28	Hunting wild pig (with Medan)
29	His own ricefield; home 4 p m., went fishing
30	Hunting wild pig (alone), got one
31	His own ricefield

Sept. 1	Tatak's ricefield (haweh)
2	Home, resting, half day; own ricefield half day
3	Hunting wild pig (alone), got none
4	Home half day, half day in own ricefield, which is finished for burning
5	Home, resting
6	Hunting wild pig (alone)
7-12	No record (observer absent from village)
13	Hunting wild pig (with Tolong and Medan), got one
14	Home, resting; helped repair observer's quarters
15	Home, resting; Kenting's baby's funeral (work taboo)
16	Hunting wild pig (alone); got one
17	Home, resting
18	Ditto
19	Ditto
20	Working in Aboen's ricefield (haweh)
21	Hunting wild pig (with Odoh), got one
22	Hunting wild pig (alone), got none
23	Fishing; brought prahu from down the river
24	Mended fish net; cooked out jungle oil for preserving fish-nets
25	Hunting wild pig (with Tolong and Medan); got one
26	Repairing prahu; home all day.

Recapitulation

Number of days working in his own ricefield	4
Number of days working in others' ricefields	4
Number of days hunting wild pig in jungle	10
Number of days at home, resting	4
Number of days at home, working	6
Total	—
	28 days

MEDAN (male). age about 35, unmarried; lives with unmarried sister; assistant chief of village.

Date

How Occupied

Aug. 25	Working Daka's ricefield (hando)
26	To Moeara Toepoeh to see Malay trader about debt
27	Home, resting
28	Hunting wild pig (with Daka)
29	Home, resting
30	Hunting wild pig (alone), got one
31	Home, resting
Sept. 1	Home, resting
2	Home, helped in cleaning up village for inspection
3	Home half day, resting, half day in jungle
4	Home, resting
5	Ditto
6	Half day in ricefield, half day at home, resting

- 7-12 No record (observer absent from village)
- 13 Hunting wild pig (with Daka), got one
- 14 Home, resting; helped repair observer's quarters
- 15 Home, resting, Kenting's baby's funeral (work taboo)
- 16 Ricefield half day; home, resting, half day
- 17 Home, resting
- 18 Ditto
- 19 Went to Poeroek Tjahoe as coolie for writer
- 20 Ditto
- 21 Ditto
- 22 Ditto; returned from Poeroek Tjahoe afternoon
- 23 Home, resting
- 24 Ditto
- 25 Hunting wild pig (with Daka and Tolong); got one
- 26 Hunting wild pig (alone); got none

Recapitulation

Number of days working in his own ricefield . . .	1
Number of days working in others' ricefields	1
Number of days hunting wild pig in jungle	5
Number of days traveling in other parts of Siangland	5
Number of days home, resting	11½
Number of days home, working	3½
	<hr/>
Total	27 days

SEETAK (male): age about 45, married, blian (medicine man)

*Date**How Occupied*

Aug. 24	Home, resting
25	To Changkang to perform as blian for sick person
26	At Changkang
27	Ditto
28	Ditto; home 5 p m.
29	Working in his own ricefield
30	Working in another's ricefield (hawe)
31	Ditto
Sept. 1	Tatak's ricefield (hawe)
2	His own ricefield
3	Home, resting
4	Ditto
5	His own ricefield
6	Sahadan's ricefield (hawe)
7-13	No record (observer absent from village)
14	Home, resting; last night performed for Kenting's dying baby
15	Home, resting; Kenting's baby's funeral (work taboo)
16	Home, resting

17	Home, working on blian's paraphernalia
18	Home, resting
19	Home, resting, half day; half day performed as blian at Mapit's ricefield house
20	Abocn's ricefield (haweh)
21	To Tombang Bana for blian's performance
22	At Tombang Bana
23	At Tombang Bana; home in evening
24	In jungle gathering firewood
25	Home, resting
26	Ditto

Recapitulation

Number of days working in his own ricefield	3
Number of days working in others' ricefields	5
Number of days working in jungle	1
Number of days (or nights) performing as blian	8½
Number of days home, resting	7
Number of days home, working	2½
	—
Total	27 days

TORONG (male) age 23, married three years; no children

<i>Date</i>	<i>How Occupied</i>
Aug. 25	Working in Daka's ricefield (hando)
26	Tatak's ricefield (haweh)
27	Oeke's ricefield (hando)
28	Pladong's ricefield (hando)
29	Djawa's ricefield (hando)
30	Home, resting
31	Working in his own ricefield
Sept. 1	Tatak's ricefield (haweh)
2	Ditto
3	Mapit's ricefield (haweh)
4	Hunting wild pig (with Oedoet and Nyaring); got two
5	Working in his own ricefield
6	Sahadan's ricefield (haweh)
7-12	No record (observer absent from village)
13	Hunting wild pig (with Daka and Medan), got one
14	Home, resting, half day, ricefield half day
15	Home, resting, Kenting's baby's funeral (work taboo)
16	His own ricefield
17	Home, resting
18	His own ricefield
19	Hunting wild pig (with Kenting), got one
20	Home nearly all day, two hours in ricefield

21	Working as coolie for writer
22	Ditto
23	Ditto; home in evening
24	Home, resting
25	Hunting in jungle (with Daka and Medan)
26	His own ricefield; clearing finished

Recapitulation

Number of days working in his own ricefield	5
Number of days working in others' ricefields	9
Number of days hunting wild pig in jungle.	4
Number of days traveling in Siangland	3
Number of days home, resting	3½
Number of days home, working	2½
	—
Total	27 days

ABOEN (male). age past 50, married; wife, mother-in-law and two sisters-in-law in his house.
(Aboen an Ot-Danum Dyak from the Sampit River country to the west)

Date	How Occupied
Aug. 25	Working in his own ricefield (slept at field)
26	Ditto
27	Ditto
28	Ditto
29	Ditto (home in evening)
30	Ditto (slept at field)
31	Ditto
Sept. 1	Tatak's ricefield (haweh)
2	His own ricefield
3	Ditto
4	Ditto
5	Ditto
6	Sahadan's ricefield (haweh)

Note The observer was absent from the village September 7-13. During his absence a falling tree so injured Aboen's foot that Aboen was not able to work again during the writer's visit. Though this record, hence, is not as adequate as the others, it is presented as evidence of Aboen's industry during the thirteen days he was observed.

Recapitulation

Number of days working in his own ricefield	11
Number of days working in others' ricefields	2
	—
Total	13 days

LIWOEI (female). age about 50, wife of Tatak; one grown son; sister of the chief

<i>Date</i>	<i>How Occupied</i>
Aug. 24	At home, pounding rice, cooking
25	Home in morning, house work; afternoon in ricefield
26	Tatak's field until 5 p m : pounded rice two hours in evening
27	Home, house work, pounding rice, etc.
28	Ditto
29	Ditto
30	Ditto, two hours in ricefield gathering javau
31	Ditto
Sept. 1	With Tatak in ricefield (cooking for haweh group)
2	Three hours in ricefield, home remainder of day
3	Home, house work, pounding rice, etc
4	Ditto
5	Ditto
6	Ditto
7-13	No record (observer absent from village)
14	Home, house work, etc
15	Home, helping with feast after burial of Kenting's baby
16	Home, house work, etc
17-26	Home, house work, pounding rice, stripping rattan, etc.

Recapitulation

Number of days at home with house work, cleaning rice, weaving, stripping rattan, etc.	24
Number of days helping in ricefields	3
	—
Total	27 days

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TODA MARRIAGE REGULATIONS AND TABOOS

By M. B. EMENEAU

IT IS many years now that the Todas of the Nilgiris in southern India have been known as a people of peculiar customs and it is thirty years since these peculiar customs were described with a wealth of detail by Rivers in his monumental volume.¹ Even now, however, much remains to be gleaned, and this paper attempts to supplement and correct Rivers' account of one important department of the life of the Todas, viz. the regulations concerning marriage and the taboos associated with these regulations.

A comprehensive account of the general framework of Toda organization is necessary, though it will be in part repetition of what is already well-known. The tribe has, first, two main divisions: *toirδas* and *toūviŋj*.² For marriage purposes these divisions are endogamous. Each is subdivided into a number of divisions called *mod* (the familiar *mund* is the Badaga form of the word). This word is used also to denote any of the small villages of a few huts in which the Todas live, any of the dairies which stand isolated from a village, including the most sacred of these dairies, the *ti:mod*, any of the funeral places, which usually are free of buildings, in fact any spot which at any time is the scene of Toda activities. But *modo:!*, "man (or, people) of the *mod*" is used only of the members of a *mod* in its widest sense, that of a subdivision of the people, and in this paper *mod* will be used only in this sense. The *toirδas* are reckoned to have ten *mod*, the *toūviŋj* five. These divisions are exogamous. Membership in a *mod* is determined by legal paternity; a man belongs to the *mod* of his "father," as does a woman until her marriage. The "father" is the man who has last given the bow to the mother of the child in the ceremony called *puwstutt*, "act of giving bow."³ Physical paternity does not count at all for legal purposes, as Rivers has clearly stated. The legal father then is the legal husband of the child's mother, and the husband must be of a different *mod* from that to which his wife belonged by birth.

Further, a man cannot marry a woman if the two are *poŋjo:!*. Membership in a group of people known as *poŋjo:!* is determined by descent in the female line. The female members of the *poŋjo:!* to whom a man belongs are in his own generation his sisters, the daughters of his mother's sisters, the

¹ W. H. R. Rivers, *The Todas* (London, 1906)

² For an explanation of the phonetic symbols employed see the final paragraph of the paper.

³ See Rivers, pp. 319-23.

daughters of his maternal grandmother's sisters' daughters, and so on; in the preceding generation his mother, his mother's sisters, his maternal grandmother's mother's daughters' daughters, and so on. To the generation younger than him belong his sisters' daughters, his mother's sisters' daughters' daughters, and so on. A man will also designate as his *połjo:!* his mother's sons, his maternal grandmother's sons, his sisters' sons, and so on for any son of a woman who is related to him as *połjo:!*, but not the descendants of these males. This network of relations in the female line cuts across *mod* divisions, and divides the *to:rðas* at present into five exogamous sets of *połjo:!*, the *töuvi/j* into six. Rivers⁴ has given a concrete example of how *mod*-relationships are handed down from one generation to the next; the relationships of *połjo:!* are preserved in the same way even when links in the relationship are no longer remembered.

A man, then, while marrying within his own half of the whole tribe, must marry outside of his own *mod* and outside of his *połjo:!*. As it is put by the Todas themselves, he must not marry a woman who is *ũudmort-kvæ:ðvoj*, "one who is born to one and the same *mod*" or *połjo:!* to him. The other peculiarities of Toda marriage and sexual relationships, viz. polyandry, the institution whereby a man may take another's wife upon the payment of compensation, that by which a man may take a concubine from the other half of the tribe, and the general license permitting a man to have sexual intercourse with a woman who takes his fancy without any permanent tie being formed, seem to have blinded observers to these fundamental rules, which govern not only marriages, but also all sexual relationships. No man may marry or have intercourse with any woman who is related to him through a wholly male line or through a wholly female line. This is the system which is generally followed by the castes and communities of Hindu south India (with some exceptions which follow the rules prevalent in north India or some peculiar system of their own). The corollary following from it, that a man may marry his cross-cousin, obtains of course among the Todas as it does among the other south Indian communities following these rules. Among the Todas it is optional, though preferable, and is applied in the symmetrical fashion.⁵ It seems to be the rule among many of the south Indian communities that the mother's brother's daughter is preferred, though without excluding the possibility of marriage to the father's sister's daughter. there is no trace of this preference for asymmetry among the Todas.

⁴ Page 490.

⁵ Rivers, p. 514, and my informants generally

Rivers' account of the general rule⁶ is incomplete and in part incorrect. How this came about is of course not certain, but a guess may be hazarded on the cause. There is even now, when information is apparently more easily obtained on most subjects than it was thirty years ago, some reticence and embarrassment about the subject of *po!jo:!*. It seems to be connected in part at least with the taboos that surround the relationship, perhaps more with the general air of indecency and ridicule with which the Todas approach the subject. This will be made clear when an account is given of the names of the divisions and the stories behind them. At the moment it is necessary to criticize Rivers' account. He interprets the term *po!jo:!* (his *püliol*) as including both sets of prohibited affinities. This, being quite contrary to Toda usage, explains the impression he records that "it came almost as a new idea to some of the Todas that his *püliol* included all the people of his own clan" (i.e. *mod*). It was a new and wholly erroneous idea. The list of *püliol* which he gives is in part a mixture of two sets of relationships, in part incorrect, and as a whole incomplete. A man may not marry the daughters of his father's brothers (Rivers' heading i) or the sisters of his father (heading iii), since they are *üüd mortkvæ:ðvoj*. The daughters of his mother's sisters (heading ii) and the daughters of his sisters (heading iii) are forbidden, since they are *po!jo:!*. His fourth head, the daughters of the sisters of his father's father, gave him trouble because of the two cases of marriages to such persons found in the genealogies.⁷ The truth is that such marriages are not forbidden. The sisters of a man's paternal grandfather are not *po!jo:!* to him, but *üüd mortkvæ:ðvoj*; they cannot have married his *üüd mortkvæ:ðvoj*, and their daughters will not be related to him in any prohibited way. The rule given⁸ that a person must not marry the child of his *mot šinj* (*matchuni*, i.e. cross-cousin) is incorrect. His cross-cousin may have married a man of his own *mod*, in which case the daughter will be forbidden to him. If she has married outside his *mod*, there are no objections. Again, if a man's male *mot šinj* has married a woman who is *po!jo:!* to the man's mother, the *mot šinj*'s daughter will be forbidden to the man; otherwise, not. The case in the last paragraph on p. 511 of Rivers' work, that of a man marrying a daughter of his sister's daughter, is impossible, since she would be his *po!jo:!*. The final case in this paragraph, the marriage of two brothers to the daughter of their sister's son, violated no rule; the disapproval that attended the marriage was undoubtedly due to the disproportion in age, not as such, but as depriving

⁶ Rivers, pp. 509-11.

⁷ Rivers, p. 511.

⁸ Rivers, p. 511 *med.* and again p. 513.

some young man of an opportunity to marry, a consideration which must always have some weight in a community as small as that of the Todas. It should be remarked further that relations of *po/ʃo:/* are extremely hard to trace in Rivers' genealogical tables. This is partly due to the intrinsic difficulty of using for this purpose a set of tables constructed for another purpose, partly to the fact that there are many inaccuracies of detail in the assignment of children both to fathers and to mothers. To set all right two sets of tables should be constructed, one for each kind of relationship: whether any useful purpose would be served by the task may be considered doubtful.

A note on the semantics of the word *po/ʃo:/* may be given here. It means "man (or person, or people) of the *po/ʃ*." The most usual meaning of *po/ʃ* is "sacred dairy," a meaning which will not explain our word. Another meaning is found, viz. "Badaga house" (the Toda house is called *a:s*). A sentence such as the following no doubt gives the clue: *emelom ūwɔd-vo/ʃo:/o:vemi*, "we all are persons of the same (*ūwɔd*) *po/ʃ* (i.e. *po/ʃo:/*)." It is quite obvious that our word is based on an obsolete meaning "house," the members of which, a man, his mother, his sisters, form the nucleus of the whole *po/ʃo:/* institution. The semantic development of *po/ʃ* should be stated then in the following order: first, "house," second, "dairy." The second meaning then drove out, for reasons at present unexplainable, the first except in the application to the houses of the Badagas and in the compound *po/ʃo:/* and sentences like the one given above, which are rather based on the old first meaning of the word then reformed on the basis of the word *po/ʃo:/*. That this interpretation of the Toda words is correct is seen from the Tamil cognate *pa/ʃi*, "building, temple."

As was said above, there are at present five sets of *po/ʃo:/* among the *to:ɾōas*. Another has in recent years become extinct by the death of its last female member, *sinul* of the *keruwo:/* (found in Rivers' Table 28 as *Sinul*). Of the remaining ones the names and stories are as follows. The *kūatēmbə/ʃo:/* or *o:rvuɪɾʔa:ikvo/ʃo:/* is said to be descended from *kūatēn* (Rivers' *Kwoten*). Of him it is told that his spiritual power was so great that he lit fire on the surface of a stream; his *po/ʃo:/* make fire by friction with great ease. The other name is based on a story that one man of the line bought for six rupees six (*o:ɾ*) cakes made with chillies (the cake is called *puɪ*). A crow (*ka:ɪ*) carried one off, and the man, leaving the others behind, chased the crow, whereupon crows carried off all the other cakes. This story like most of the others to follow ridicules the stupidity of the members of the line. Another is called *moɾʔa:ɪkəntʃəvo/ʃo:/*, "people of the house of one who heated the child at the fire." The story goes that in

order to strengthen a new-born child, they smeared it all over with butter and put it in front of the fire; as a result the child died. Another is called *mukorðurkto:rjvm/vojvo/ʃo:/*, "people of the house of those who carried large baskets to the mukor swamp" or *mo:vγo:ηoðmölγũartvojvo/ʃo:/*, "people of the house of those who ground pepper without seeing the deer." It is told that seeing carrion-eating birds circling over a swamp they assumed that there was a dead deer there and went off with large baskets to bring back the flesh, telling the women meanwhile to grind a large heap of pepper for the curry. Of the other two, more indelicate stories are told. One is called *pikukvenvotsvojvo/ʃo:/*, "people of the house of one who put butter on the navel." A woman covered her navel with butter and made a dog lick it and have intercourse with her. The other is called *pultʃvo/ʃo:/*. It is said that formerly a man and woman of this *po/ʃo:/* had sexual intercourse with one another. The women of this line are said to be particularly strong in their sexual passions. the verb with this meaning *artsti*, "she is passionate," and the noun *arikn*, "passionate woman" are tabooed under the conditions described below, and it is said that the element *pultʃ-*, not otherwise found, has the meaning of these tabooed words.

Six *po/ʃo:/* of the *tõuvi/ʃ* survive: one other is known to have become extinct some years ago with the death of *kũu:rjũm:r* of the *ko/lemo:/* (*Kurikudr* in Rivers' Table 72). The others are as follows. The *ũatvaγð-wurspodjvæ:tõ:djvojvo/ʃo:/*, "people of the house of one who hunted the pig in the *ũatvax* swamp" are so named because one of the line hunted a pig which he had seen for a whole day and night and yet let it escape him. Another is called *nõ:ηgo:t̃kvar/γartjvojvo/ʃo:/*, "people of the house of those who sent fuel to *nõ:ηgo:r*." Men of this line saw a hare, and intending to hunt it for food, set the family to work to collect fuel to roast it for three days. A third is called *o:rvm:t̃γe:rjvojvo/ʃo:/*. The story is the same as for the *o:rvm:t̃γa:kvõ/ʃo:/* of the other division, except that the cakes were in a basket *muke:rj*, to which the element *ke:rj* in the name is equivalent. Another, the *pevur:t̃γõ:l̃jvojvo/ʃo:/*, "people of the house of the stealer of a large piece of meat," are so named because one of the line stole a large piece of meat at the sacrifice of a calf (*æ:rγumtt*, "act of killing male buffalo").⁹ A fifth is called *kũðurγo:ilvat̃j̃t̃setape/kõθkjvojvo/ʃo:/*, "people of the house of one who seizing a horse's leg pole-vaulted." It is told that a man, finding a dead horse, cut off the leg and vaulted with it until he reached the door of a house where he was beaten by the occupants. The last is called *õ:ñõũ-kõrkõjvũnæt̃pũ:t̃vũrkutvojvo/ʃo:/*, "people of the house of one who taking the milking-pot in his left hand at the *tĩ* dairy called *õ:ñõũ* broke

⁹ See Rivers, p. 274.

wind loudly eighteen times." As a result of this exploit, the men of this *połjo:!* are forbidden to become *poło:!* to this *ti:*. Another story is told of these people, similar to that told of the *moγδurtko:it švojvołjo:!* of the other division. It is said that a child was heated at the fire (it seems with the intention of making ghee from it) until it died. Then it was hung up on a tree and the people wept, closing one eye. All these stories were checked by getting them from as many different informants as possible, and agreement on them is very close, though details differ in the different accounts. The large element of the ridiculous in them puts them in line with the *kũwdzwl*, the Toda's favorite sport when sitting about in a company. Stories are told of members of the company in which the wildest and most improbable actions are attributed to them. Why the *połjo:!* should have become the object of such sport it is hard to say, but under the circumstances one cannot seek for much historical truth in the stories.

A number of taboos on actions and language operate in the intercourse between *ũwdmortkvæ:dvoj* and *połjo:!* of opposite sex. The primary one is the taboo on sexual intercourse, and most of the others are obviously to be considered as avoidances of incitement to this. The taboos begin to operate at puberty. They apply also to relations between parents and adult children, especially between father and daughter (it is to be remembered that a daughter when married no longer belongs strictly to the father's *mod*), and to relations between father-in-law and daughter-in-law (she is strictly not related to him as *ũwdmortkvæ:dvoj*) and between mother-in-law and son-in-law (she is not in any sense his *połjo:!* or his *ũwdmortkvæ:dvoj*). It is said that the taboos have now slackened somewhat except between father and daughter, and the impression given is that observance of them was never very rigid except when a fairly large company was present. For example, it is clear from Rivers' account and from my informants' accounts that irregular sexual intercourse was and is common. When it is found out and becomes known to the people of the tribe, the *no:jm* or council takes cognizance of it and imposes on the man a fine of at least one buffalo.

Most of the taboos on action, other than that on sexual intercourse, seem designed to prevent physical contact or the unchaperoned presence in one another's company of a man and a woman of the prohibited degrees of affinity. In the latter class must be put the prohibition against two such persons traveling together unaccompanied from one place to another. An exception is made for a man and his mother. Taboos against physical contact are as follows. They may not sit on the same *tuṇ* or raised earth seat; they may not touch one another, and so may not assist one another in dress-

ing the hair or in bathing. The latter is so extended that one may not even be present while the other is bathing, though the preparing and bringing of bath-water is allowed and is as a matter of course done for a father by his daughter. Another curious taboo may be placed here, though it is limited in its operation in such a way that its allocation is somewhat doubtful. The water with which a man washes his right hand after eating (it should be remembered that in India the food is handled with the right hand only) must not be thrown away by his female *po!jo:!* or *ũũdmortkvæ:dvoj* who is older than himself. His mother may do so and normally does until he marries, when his wife normally performs the office. His daughter or daughter-in-law may also dispose of the water. There is also a prohibition against breaking wind in the presence of a *ũũdmortkvæ:dvoj* or *po!jo:!* of the opposite sex.

Men and women seldom sing together, though it sometimes happens, and more especially in the case of husband and wife. But *ũũdmortkvæ:dvoj* and *po!jo:!* of opposite sex are forbidden not only to sing together but even to sing at the same time in one another's hearing. If a man is singing out of sight but within hearing of a woman of these categories and she wishes to burst into song, she will first ask who is singing and then regulate her actions accordingly; so also if a man hears a woman singing. One can only suppose that if they sang at the same time, they would be considered to be acting as if they were marriageable.

References to all subjects, whether regarded by our communities as decent or indecent, are very freely made in Toda conversations, as I have had occasion to observe when getting information from parties in which both men and women were present. Very frank speech is usual with women as well as men. But between *po!jo:!* or *ũũdmortkvæ:dvoj* of opposite sex, or even when one of these is present in the company but not actually addressed, this freedom is taboo. Reference, whether direct or by circumlocution, is forbidden to sexual intercourse, menstruation, the private parts, including by extension the nipple, the navel, the armpit, and pubic hair, to the secreta, or breaking wind. Biting the skin of the neck while making love seems to be common, and the red mark which lasts for some time is a subject for the jest: "You have been bitten by a tiger." Use of the compound verb stems which denote the act of biting in this way (*mũrɣũart -*, "neck-cut," and *kũartɣũũrt -*, "cutting-give") or any indirect reference or jesting is forbidden between people between whom the taboos operate. So also is the use of the word *kũũrdjvõm*, "fowl-lice," which also denotes the insects found in the pubic hair or the hair of the armpits, reference to these insects is also forbidden. Other words are forbidden in specific senses.

Thus *adê-*, "dig," is allowed when a word for "pit" is used with it, but forbidden when used with a word meaning "woman," in which case it means "rape," so also *kwî-t-*, "pluck up by the roots," which with a word for woman means "rape." The word *nartj-*, "make walk," is permitted when *ur*, "buffalo," is compounded with it in the sense "drive buffaloes on migration" or "pay a buffalo as a fine for pollution of the dairy,"¹⁰ or when in combination with a word for "bead" it means "string (beads)," or in other uses not involving indecency; when a word for "woman" is used with it, it means "have sexual intercourse with" and is forbidden. So also the very common verb *patj-*, "seize, take," which with a word for woman means "have sexual intercourse with." The stem *o:j-*, "become," is very frequent, usually in compounds; when used by itself, it means "menstruate" and is forbidden. The word *po:tj-*, "shout" is forbidden in the sense "(woman) cries out during sexual intercourse." Again *atvat* is an imperative with literal meaning "in that direction-seize." It usually is found in contexts where it means "take this and hold it stretched out that way," it is forbidden in this sense since the same utterance can also mean as applied to a woman "have sexual intercourse in animal position." It can also be used as an exclamation "nonsense!" and this use is not forbidden. In the same way *pwn*, "gold bangle in a dairy," is forbidden when used in the sense "privates of a young girl." Similarly *po:θj*, "moss," may not be mentioned since a variety of moss is used during menstruation as a sanitary napkin. Women are tattooed and any reference to the process of tattooing or to the markings is forbidden. The word for "tabooed words," *kipout*, lit. "inferior-words," is itself tabooed.

These language taboos produce an interesting result in the field of social habits. Wordy quarrels about marriage arrangements, the financing and details of ceremonies, and other affairs are very common among Toda men. These quarrels most frequently take place at the large gatherings at ceremonies of all kinds. Since on most such occasions, especially at funerals, many women are present, the probable presence of *ũwmortkvæ:dvoj* or *połjo!* within hearing prevents the use of indecent language by the quarrelers. My informants tell me that this is the rule also in quarrels at purely male gatherings: evidently indecency has not become an element in abuse under any circumstances, since its prohibition in some circumstances has prevented the growth of a habit. I am also told that quite recently, with the relaxing of all taboos and the example of Tamil speakers, a certain amount of indecency has crept into the language of abuse.

¹⁰ See Rivers, pp. 293-305

Enquiry into the details of the $\text{mo}\gamma\delta\text{o}\iota\text{tj}$ institution (called by Rivers *mokhthoditi*)¹¹ supplements Rivers' account in some important details. For all legal purposes, i.e. membership of *mod* and the consequent relations to the tribe as a whole, Rivers is correct. Children of these unions are legally children of the man who has given the bow to the mother. In investigating restrictions on the freedom to form these unions it was found that there are two classes of union. In one the man who takes the woman gives only a cloak ($\text{pu}\iota\gamma\upsilon\iota\text{j}$). Children of this type of union and the man's legal children may form $\text{mo}\gamma\delta\text{o}\iota\text{tj}$ unions without any restrictions, except that such a union may not be formed while the parents are still living together in a union of this kind. When the union in the earlier generation ceases, a union in the next generation may be formed.

In the other class of union the man gives to the woman jewelry and makes to her male relatives the $\text{ko}\iota\text{milv}\omega\delta\theta\text{t}$ salutation ("act of falling on leg," i.e. kneeling and putting to one's head the other's feet in turn; of the one who raises his feet it is said, $\text{ko}\iota\gamma\text{o}\iota\text{tj}$ —, "leg-show"). Children of the union are then in the same relation to the man's legal children as if they were $\text{ũ}\omega\delta\text{mortkv}\epsilon\iota\text{dvoj}$, and the same rules hold for unions between them and between their descendants as for unions between $\text{ũ}\omega\delta\text{mortkv}\epsilon\iota\text{dvoj}$ and their descendants. For example, a half-brother and a half-sister may not form the union: their children may form the union, for they are *pseudomotšinj*. Children of half-brothers may not form the union, but their children may. Rivers¹² mentions the union of $\text{kũ}\omega\iota\text{sia}\iota\text{ff}$ of the $\text{tũ}\upsilon\text{vi}\iota\text{j}$ with $\text{pi}\iota\text{j}$ - mury of the $\text{to}\iota\text{r}\delta\text{as}$. He insisted on giving *ter* for her¹³ as if she could be his legal wife, and lived with her for many years (which is unusual in these unions), having by her four children, who were regarded by everyone as his own except that they belong legally to the $\text{to}\iota\text{r}\delta\text{as}$. This seems to have been the first case in which *ter* was given in one of these unions, and there have been some later imitations of the precedent. This type of union is merely an extension of the type in which the $\text{ko}\iota\text{milv}\omega\delta\theta\text{t}$ salutation is performed, and the regulations governing unions of the descendants are the same.

The taboos described above operate also between all those who are in a pseudo-relation of $\text{ũ}\omega\delta\text{mortkv}\epsilon\iota\text{dvoj}$ because of a $\text{mo}\gamma\delta\text{o}\iota\text{tj}$ union. In the first type of union they cease to operate when the union is dissolved, in the second they are permanent in their operation, just as is the prohibition against further unions.

¹¹ Described in Rivers, pp. 526–29.

¹² Page 534 *med*.

¹³ See Rivers, pp. 523–25.

The Toda words given in this paper are written in the alphabet of the International Phonetic Association with some changes. The language has three t-phonemes: t a tongue-tip dental, *t* a tongue-tip alveolar, and ɽ a retroflex: there are three d-phonemes with the same positions, d, *d*, ɖ; only two n-phonemes are found, n alveolar and ɳ retroflex. The language has a peculiar sibilant phoneme, which is homorganic with the alveolar stops and has the body of the tongue well flattened and lowered from the roof of the mouth, thus differentiating it from ʃ in the same position, while its position differentiates it from s. For this sibilant I write *s*. Three r-phonemes are to be distinguished: *r* a one-flap sound at the roots of the upper teeth (the IPA symbol for this sound is not available), r a trill in alveolar position, and ɽ a trill well behind the alveolar ridge, i.e. in retroflex position. The symbol j represents the English y-sound except when between voiceless consonants or final after a voiceless consonant, when it also is voiceless. Of the vowel symbols, *u* is high, back, unrounded, *o* mid, mixed, rounded. Four l-phonemes have to be distinguished, and the limitations of the printer's stock have forced me to write them as follows: l alveolo-dental voiced, *l* alveolo-dental voiceless, ɭ retroflex voiced, ɭ retroflex voiceless.

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THE BEAR FESTIVAL OF THE OLCHA

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DESCRIPTION OF THE FESTIVAL

THE Olcha are a small tribe dwelling on the lower part of the Amur River between Sophiysk and Bogorodsk. Their entire number does not exceed seven hundred men. Their language stands in close relationship with those of the Goldi and Orochi, and their culture with that of the Gilyak and Goldi. The Olcha are divided into a number of patrilineal exogamic clans representing social as well as religious units.

Like the Gilyak, Orochi, Ainu, and Negidals,¹ the Olcha arrange a bear festival following the rearing of the bear. Like the Gilyak, the Olcha connect the bear festival with commemoration of their dead kinsmen. After the death of a relative a dog was selected to be his panjau, that is, the embodiment of the deceased soul.² During the funeral the dog was led close to the coffin and its halter laid in the hands of the dead. Afterwards the halter was forcibly "torn out of the hands of the dead," the dog led into the house and tied to the plank-bed just at the place where the deceased used to lie. It is supposed that the soul of the dead person passed into the dog. If it is possible, a bear is captured or bought. He becomes a panjau, and the dog is given to the gusisel, that is, the men of the wife's clan (the clan from which spring the sisters' husbands).

For regularizing the passing of the panja the dog is untied from the plank-bed and the bear is tied for some time in its stead. This is why the bear festival is considered to be a commemorative act, notwithstanding that the Olcha arrange a real commemoration (kasa) with the assistance of a shaman. The existence of these two types of commemorative ceremony can evidently be explained as a mixture of the Gilyak ceremony (bear festival) with that of the Goldi (kasa).

Suppose a bear has been bought and has attained the requisite size. The time of the festival is near. In the autumn, just when it begins to freeze, the clansmen assemble, decide the question of the festival organization in winter, and arrange meanwhile "the small festival." They lead the bear out of the cage into the house, feed him there, and regale casual guests. Then the bear is led back into the cage. This is the end of "the small festival." At the beginning of January the old clansmen meet to confer where and when the bear festival is to take place. They usually decide to begin

¹ The Goldi do not arrange a festival with rearing of the bear.

² Panja, soul, is equivalent to fanja of the Goldi. The Gilyak call the selected dog presku. See Krcinowitch, *The Dog-Breeding of the Gilyak*.

at the end of January with the intention of finishing in the first fortnight of February. The duration of the bear festival varies greatly. It depends on the wealth of the host and the abundance of the provision supply. The shortest festival is seven days, the average nine, and the longest fifteen to seventeen days.

The festival organization of every clan has certain peculiarities, but its general outlines are the same in all Olcha clans. We shall describe the festi-

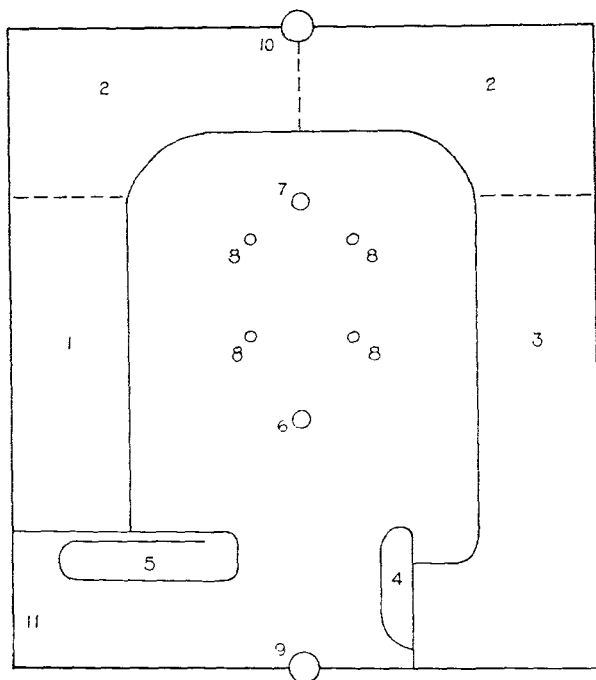


FIG. 1. Plan of an old-style Olcha house 1, Pusku, 2, Malu, 3, Goči, respectively the left, middle, and right parts of the plank-beds; 4, Duantetawa, "the hearth of the forest"; 5, Temutawa, "the hearth of the water"; 6, Bahsaturani, 7, Gočiturani, 9, Čonkoditurani, 10, Maluditurani, posts supporting the roof; 8, Kada, small posts to which the bear is tied; 11, Uca, the door.

val of the Valzu clan, indicating in some places the different order of various parts of the ceremony in other clans.

First day. On the first day people are called to drag the bear out. They come to the cage, open it, and drag the bear out with nooses previously prepared. After binding the bear, they lead him to the house of the host and tie him to the post (see the plan of the house, Figure 1). When this is

done some one of the bystanders jumps from behind on the shoulders of the bear, catches his ears, and presses his head to the ground. Instantly some of the men fall on the bear, grasp him, and tie his paws. When the tying is finished, all the men are ordered to jump off. After that the bear is fed with mosi (a preparation of boiled fish), red bilberries, and some roots. When the bear has eaten, all the men begin to pull the cords tied to his paws till the animal lies with his belly on the earth and with his paws widepread. Then water is thrown over him, a collar with chains put on his neck, and all the nooses taken off. Then the bear is turned in such a way that the halter that was at the malu turns towards the entrance and the one that was at the goci comes to the malu. When this circuit is accomplished the bear is dragged out into the street, beaten with sticks, and teased. The bear is thrice dragged out into the street and back into the house, and the whole ceremony repeated thrice. After that the bear is tied to the post and left so during the night. The same evening nine men make a road for dog racing. This is the end of the first day.

Second to seventh days. Relatives and friends arrive in the morning to take part in the dog racing. The dogs are fetched, and all go to the house where the bear has passed the night. There the dogs are counted and the number of dogs to a sledge settled. This depends upon the entire number of dogs and the number of days during which it is proposed to continue the races. The harness is prepared and adjusted, and the dogs divided into teams. The dogs are then fed, smoked with the smoke of fir tree branches, and put to the sledges. Those men who are not taking part in the race run to the finish. When they have attained it, the race is started. The races continue during the second to sixth days. At first nineteen or twenty dogs are harnessed and a man drives the sledge. Later the poorer dogs are taken out of the harness and the burden in the sledge is made accordingly lighter. A boy of ten years replaces the man in the sledge; after that the dogs run with an empty sledge, and on the last day the dogs run only with the harness without any sledge at all. Their number is usually not more than one or two. The dog that has won the race is rated very highly and its fame spreads far through the neighboring villages. The guests are not yet numerous. On the fifth day messengers are sent up and down the river to call for the guests "to come all on the eighth day."

During the racing days the relatives in the host's village regale the bear. One of the clansmen says to the host, "Let the bear be our guest." The bear is led to his home and sleeps there one night. The bear and the guests that have arrived for the festival are also regaled in this house. Often the bear is driven to the agnates (doha) in a neighboring village in a sledge

drawn by twenty dogs. When the races are finished, all present assemble in the house of the bear owner, where entertainment is waiting them. This is the end of the first part of the festival, which is attended only by inhabitants of the host's village, the nearest doha, and a few guests.

Seventh day. The mosi is made on the seventh day. For that purpose skins of beluga (*Acipenser huso*), trout, and Siberian salmon are soaked in a wooden trough and scaled. The skins are then boiled and rubbed with berries in a small trough. When all this is cooled it forms a jelly-like substance, the most delicious part of the Olcha meal. The preparation of mosi requires a whole day's work of twenty persons under the supervision of one of the old men. Others go to the forest to bring branches and young trees for decoration and preparation of ritual shavings. The house and playground (araču), where the bear is to be shot afterwards, are decorated with fir trees, covered with shavings, and fenced with bushy willow sticks thrust into the earth. This is the day of the sengisel arrival. Sengisel are the relatives on the mother's and wife's sides. They are considered honored guests and partners of the festival. In the evening the owner of the bear arranges an entertainment and regales the sengisel with mosi.

Eighth day. The host, together, with his guests, makes shavings of the bushy willow sticks cut in the forest. Such a stick is pressed with an end against the abdomen and shaved with a thin curved knife. A thin shaving curls from under the knife. The shavings are ready early in the afternoon. By that time all the guests have arrived. The bear is then dragged out "to look at the shavings." This is the time for all the gamasun (father's sister's sons, mother's brother's sons) "to take hold" of the bear. Running from behind, they jump on the bear's back. The animal cowers in confusion under its burden and instantly several arms pull the daredevil from its back. All whom the host's clan call gamasun and gusi (mother's brother) must perform this leap to prove their courage and dexterity. After the "taking hold" of the bear is finished, he is led round the village. Meanwhile a feast takes place in the house of the host, continuing until dawn. This feast (toje) plays a great role, being the central event of the second part of the festival.

The amount of food consumed during a bear festival is immense; of fish alone the supply is about a ton. The toje used to begin at daybreak and continue through the night until dawn of the next day. Here the guests had also to keep up to the mark. The guest who fell asleep early, saying, "My belly is full," was considered a weak creature. It was necessary to sit up the whole time, and to eat, eat without end. A host who gave a poor repast was considered a bad host. It was a matter of honor to hear the guest say, "Oh, there was much food."

Ninth day. In the morning dogs are led into the house of the bear's owner. One or two of the best are selected to be tied to the plank-bed. The bear is then dragged out of the house, taken round the village, and led back to the house of the owner. Here the bear is fed with different kinds of fish. Then the implements of the bear festival are brought in from the sildi where they are always kept.³ The clan flints, two knives for cutting the meat, an axe for hacking the bones, a vessel for water, a kettle for grease, are all brought in and hung up on the malu wall. Then the bear is turned round in such a way that the halter on the malu side faces the door and vice versa.

While some are turning the bear, others bring in water and give the bear to drink. After that an old man beats the bear with a stick with shavings on the end, repeating the words: "I clean the dirt from You-man (husugdeni); walk along our way; walk there. You shall come near a lake—wash there. Man, wash the dirt away. Wash your muzzle according to our rules. Pass over a hundred places, jump over three hundred feet, turn around fifty times, pass ten mounds, cross forty mountain ranges, roll down our slope: there will be men of the tayga (duanteni)." This beating is called a "cleansing" of the bear. The significance of this procedure is to compel the bear to go from the festival to the men of the tayga. When the "cleansing" is finished, the bear is drawn out into the street and led towards a hole in the ice. If the bear is male he is led thrice around the hole; if female, twice.⁴ After that the bear is led again to the house of his owner, the door of which is opened beforehand. When his forepaws have passed the doorway he is pulled back in such a way as to prevent his hindpaws passing it. Then he is again drawn in and pulled back again just as he passes the doorway with his front paws. This ceremony is called docpočambau. With a male bear it is repeated thrice, with a female, twice. When the bear is drawn to the doorway an old man beats him with a fair-sized tree. The bear is not led into the house at all, but at the end of the docpočambau he is tied near it with a chain. By this time the women, dressed in their richest garments, gay with rich patterns, complicated ornaments, and various furs, come out of the houses. Three of the women beat with sticks against a dry trunk about nine feet long and six inches thick, hung up horizontally on two ropes. A bear head is roughly hewn with an axe on one end of the trunk. Skillful strokes make the trunk sound like a drum.

Meantime all the contents of the sildi are loaded on a special sledge. The victuals are laid on it and the bear tied to its rear edge. As several men

³ The sildi is a small barn usually situated near the outskirts of a forest. The implements of the bear festival and the skins from the heads and paws of the slain bears are kept there.

⁴ The Gilyak also lead the bear around a hole in the ice. Not all the Olcha clans follow this custom.

pull the sledge, the whole procession takes its course to the araču. The araču is usually situated not farther than two to three hundred paces from the village, but it is supposed that the way to it is very long. On the way a stop of some minutes is made to symbolize the passing of a night in the forest. When the araču is reached, the bear is turned in the same way as at the house. Then men are selected to "clean the playground for the shooting," to remove branches and bushes that might interfere.

When this is accomplished, the host takes the man he has chosen (who must be one of the gusi or gamasun) by the hand and leads him to the place from which the bear is to be shot. The host gives him a bow⁵ and arrows. The onlookers retire, forming a vast circle. The host and his family stand behind the bowman. The bowman strains the bow, tries the string, adjusts the arrow, and then lets it fly toward the bear high over the tops of the trees. This is a symbolic performance called "the cleaning of the way." Then the bowman comes up close to the bear, and having watched a favorable moment, shoots the arrow into the bear's heart. If the bear is not killed with three arrows, the host becomes angry because of "the tortures caused to his relative" (the bear), and fines the bowman, who must give him a dog. When the bear falls, the host and his relatives shoot at him, "finishing" the dying or even dead animal. After that, three or four men, relatives or strangers, take bushy willow sticks and by means of these press the corpse to the earth. Then the chains are taken off and the host cries, "E-is, e-is."

The chains are carried away, the bear laid on a mat of bushy willow sticks with shavings, and a fire lighted on the araču. The sledge is emptied of its contents, which are laid near the posts to which the bear was tied. All present sit down directly on the spot despite the frost and eat various kinds of fish and porridge. While the guests are eating, the host and his clansmen eviscerate the bear. When the meal is ended, the eviscerated bear is loaded on a sledge and driven home. If the bear was male, one circuit of the house is made; if female, two. The bear is then brought into the house. Boys of three to ten years of age meet the procession at the door, and as the head of the bear appears in the doorway, attack it, catching hold of the fur. The house is entered only by the clansmen of the host. They build a dais on the malu where they lay the skin of the bear, taken off together with the head.

A meal is then served, consisting of the remainders of fish from earlier meals. Toward the end of the evening the guests are called into the house

⁵ The bow of the Olcha belongs to the "simple" type that differs essentially from the complicated glued bow of the Gilyak. The Orochi also use a simple bow.

and seated on the plank-beds. A feast is begun, which ends only toward sunrise. Raw fresh fish (usually carp) hacked to small pieces is served at first. When the fish is eaten, soup is served, followed by brandy. After all the brandy is drunk the guests go home, but at the doorway each of them receives from the host a piece of frozen mosi and porridge, which they take with them.

Tenth to fourteenth days. These are days of tranquility. The guests are departing. Only the sisters and clansmen remain. The meat of the bear is boiled. On the twelfth day brandy is drunk again. On the thirteenth day the whole bear is boiled, except the forequarters, which are boiled on the fourteenth day.

Fifteenth day. The head is cut from the skin and boiled. A ngarka is then arranged, but only if the bear was captured in the forest by the host of the festival or his clansmen, never if it was purchased.⁶

The chief act takes place in the house of the host. Twelve clans, each represented by several individuals, take part. They are divided into two moieties, six clans in each, this division being retained at all the festivals. The partners sit down on the plank-beds in a definite order. One moiety is seated on the puskū, the other on the goči, with one elder clan of each moiety at the back of these sections of the plank-beds. The moiety that sits on the goči is called duanteni, "men of the forest." They are considered to be the clans of the best hunters. The other is called temuni, "men of the water." Seated thus, they vie with each other in eating. When all is eaten, the partners of the ngarka go out into the street, where the other partners of the festival pelt them with snow. Then they return to the house, sit down in the same order, and the rivalry continues. This is repeated thrice. All this is in the nature of a competition—to see which moiety is able to eat its portion in shorter time.⁷ But at last the ngarka is finished. Red bilberries are served then in wooden troughs. After that, all dishes and crockery of Chinese origin are taken away, the floor covered with fir branches, and the bear meat served. The gusi are served first. The eldest gusi receives the upper jaw. The elder brothers of the wife and fathers of the wives receive

⁶ The following is the first description of the ngarka to be published.

⁷ The Olcha compare the rivalry of moieties with the race of two sledges, each drawn by six dogs. By this analogy they explain the number of the clans in each moiety. But the participation of six clans in each of the competing moieties finds, in fact, another explanation. Twelve clans were evidently originally divided in two exogamic moieties. Afterwards through merging, migration, influx of new clans, and division of the old ones the number of the Olcha clans increased more than twice, but the conservative ritual of the festival has maintained the original number of six in each moiety, which has acquired a sacred character.

the mandible. Afterwards the head, the grease from the intestines, and that from the back are served. Then begins the eating of the bear meat, served adhering to large bones. The host personally offers to every one present a large slice of bear meat, but cuts from every slice a small bit for himself.

The host, his clan, and all the numerous guests regardless of age and sex take part in devouring the bear. But in the distribution of meat a number of rules must be maintained. The women and children are not permitted to eat the bear's tongue. But women eat the paws, which are forbidden to men. If a man eats a paw he will be devoured by a bear in the forest. The biceps and the genitals of the bear are eaten by the oldest man present. During the meal, the men, women, and children all sit apart from one another.

While the head is being eaten, a dog is led into the house and tied to the post. The bare bones of the bear are accurately laid together, none of them being lost. The eaters take care not to drop grease on the plank-beds, else it must be burned out. The bones are gnawed clean. A mash of various sorts of frozen fish hacked to small pieces is served. When all the meat is eaten, women give water to everyone. Following this, the host takes the dog by the ears and pushes it with its nose against the post, then against the edge of the plank-bed, then the side of the hearth, and finally against two thin fir trees thrust into the snow in front of the house out of doors. After that the dog is let free. Then all the bones are gathered. Every partner of the festival returns them to the host safely wrapped in a piece of bark. The bones are put into a trough and carried away to the *araču*, where they are kept in a receptacle.

The bone collection finished, the guests begin to drive away to their houses. The host and his family go out to accompany them. Before their departure the host gives them a slice of bear meat of a strictly determined size. This slice (*jali*) must be given to the whole clan without regard to the number of its members present. It is carried home and there eaten by the whole clan.

The host gives his dogs to the guests—those who took part in the *ngarka*—to drive home. When they are already some hundred yards from the village, the host takes a sledge, overtakes the guests, makes a tour around their sledge, and returns home without saying a word. This he does with each partner of the *ngarka*.

This is the end of the fifteenth day and virtually the end of the festival. But before passing to the sixteenth day, in which only the inhabitants of the host's village take part, one very curious custom must be described which occasionally occurs during the eating of the bear meat. The source

of such episodes are in mocking that may have taken place some years before the bear festival. An Olcha may have mocked some member of another clan, "You are beggars," or at a bear festival during the meal some one of the guests might have said, "The bear has eaten grass. He is dry. Why have the guests been called?" The offended one conceals the hurt and complains only to his relatives. The relatives decide to take revenge. They buy one or several bears and feed them for several years. When the bears are grown large and fat, a festival is arranged. The long-awaited revenge takes place on the sixteenth day. Especially immense bits of grease are served to the offender and all his relatives. "Glut and guzzle for your mockery!" Custom forces the guest to eat the whole portion. But when the guest-offender, quite tired of eating his huge portion, asks for water, he is served with a bailer full of melted grease, and all begin to mock and laugh at him. The offender, who has now taken the place of the offended, says nothing but "Look what is to be done," and drinks the grease. If he declines to do so, it is a disgrace to him. It is a sign of capitulation, of defeat, and he drinks the grease. But this is not yet the end of the revenge. When the time comes to go home and all the clans receive the jali, an immense bit of meat—a whole bear carcass—is loaded on the sledge of the offender, so that it breaks to pieces to the great joy of the host and confusion of the offender. He is forced to borrow a sledge to carry the carcass home. All his relatives of the paternal clan are considered insulted: "Our brother is offended," they say. They meet, arrange a still more luxurious festival, invite their offenders, and so on without end. This rivalry of clans lasts for many years, and in earlier days used to end in some cases with armed combat.

Sixteenth day. Early in the morning all the inhabitants of the village are invited into the house of the host, where they sit down on the plank-beds. The remaining meat is served. When the rest of the meat is eaten, the guests go home. Only the host and his clan remain. The bones of the bear are wrapped in ritual shavings. The mat of fir trees, all the implements of the bear festival, and all the bones are loaded on the sledge and drawn to the araču. The kettle, the axe, knife, and other articles are placed in the sildi together with the remnants of the meal. The bones of the bear are placed not far off in a receptacle. A fire is lighted on the araču; food is placed beside the posts to which the bear was tied. The skull of the bear is smoked in the fire. Meanwhile an old man sings:

"Smoke, smoke, the body will change.
If you run away I will catch you with a hook.
If you try to attack me, I will kill you with a spear."

Then the skull is fastened in a split trunk of a young birch tree, the remains of the food are thrown into the fire, and fish is eaten. The rest of the fish is brought home. In the evening some old men and women, relatives of the host, drink brandy. Fish is eaten afterwards. This concludes the bear festival.

"THE PEOPLE OF THE FOREST"

The bear festival of the Olcha, Gilyak, and Ainu is a ceremony of extremely complicated construction, in which numerous heterogeneous elements are involved. We shall try to examine them, first, from the native point of view, to gain a knowledge of their comprehension by the Olcha themselves.

The Olcha are inclined to minimize the religious importance of the bear festival; they move other aspects into the foreground. They usually are opposed to the Russian name of the ceremony, "bear festival," and call it, when they speak Russian, "play with the bear," which is a literal translation of the Olcha's *bojum hupu*. If an Olcha is asked, "Why do you play with the bear?" he usually answers, "There is such a law;" that it was done from ancient times; and why and for what he does not know at all. But if the inquiry is continued, it is possible to receive more detailed answers. It may be answered that "the bear is played:" (1) for meat; (2) as a reason for the relatives to come together; (3) the clan who often "plays the bear" has good fortune in hunting and fishing, (4) the bear is fed and brought up as *panja* in commemoration of a dead person.

Two features have thus a profane and two a sacred significance. And in fact the bear festival is not a homogeneous unit but a combination of these two principles. The third point is the most essential in the complex of ideas connected with the bear festival. For the Olcha this is virtually an universally acknowledged explanation of the indispensability of the festival. This is the very point that reveals the connection of the bear festival with the belief in the "forest men."

For the Olcha, the bear is not a god, nor "a dog of the forest men" (as Sternberg has it), but a forest man himself who has changed his appearance. The Olcha and Goldi believe that the forest is inhabited by forest men (*duanteni*) who live there in villages like ordinary humans. But they are more powerful, are masters of the *tayga* and its beasts, and require sacrifices. They very often take the form of a bear, and a bear is nothing other than a *duanteni* in a fur dress. This is evident from the fact that the bear, who receives such tokens of honor during the festival, is treated quite otherwise if he kills a man. In such a case revenge overtakes the bear—repre-

sentative of the forest men—just in the same way and manner as it would a strange clan of common men. If they succeed in killing the bear soon—the Olcha attack him fiercely—they cut him to small pieces, throw them far apart, and mock at him in every way. The traditions of the Olcha relate how common men fell among the forest men; how they saw the bears turn to forest men and the forest men to bears. The bear killed in the festival does not die; he returns to his relatives, the forest men. He passes nine mounds, forty mountain ranges, cleaning the dirt off himself by the way, changing his body, and comes to his relatives, the men of the tayga. Just this sending away of the bear to his relatives is considered by the Olchas to be the chief point of the bear festival.

The Ainu, when they kill the bear at the festival, also do not say that he is killed, but that he is “sent away.” Before the bear is killed, a man sits down opposite him, and says that he is going to send him to his relatives in the mountains. He prays him not to be angry. If he is a real bear, he will return again.⁵ In their invocations the Gilyak also send the bear to the master of the mountains.⁹ The Orochi, finally, on killing the bear, say to him, “Go fast; go to your master; put a new fur on, and come again next year that I may look at you.”¹⁰

It is evident that the bear is sent away to his relatives, to the forest men, the masters of the mounds, and brings them the sacrifices of the bear festival. On the way he cleans away the dirt, changes his body, and becomes a forest man. When he comes to his relatives he relates how the festival was arranged. If all was arranged correctly, the men of the forest will be contented and will send to the common men happy hunting. There exists a long tradition which relates in detail why and how the ritual of the bear festival must be maintained. From this tradition it follows that the sacrifices made for the bear are intended for the forest men and reach them through the bear killed on the festival. That is why the Olcha say of the bear festival that the bear has visited them to take the provisions. It is furthermore supposed that all the provisions eaten by the guests at the festival go to the men of the forest. Hence all the provisions must be of the highest quality. The bear festival can be represented from this point of view as a powerful prayer for good luck.

The tradition mentioned above provides the reason and order of the bear festival organization. It narrates that once a common, mortal woman,

⁵ Batchelor, *The Ainu and their Folk-Lore*, pp. 206–207, Batchelor, *Ainu Life and Lore*, pp. 492–93.

⁹ Hawes, *In the Uttermost East*, p. 199.

¹⁰ Sternberg, *Gilyaks*, p. 439.

who lived with two brothers, dreamed that she was called by the men of the forest. She went to them, lived with them, and gave birth to bears. When the children grew up, she made her way with them to the forest "to get provisions from her brothers." Her brothers indeed soon found her tracks, which led them to the lair, stabbed their sister, the she-bear, and took with them her cubs which they began to feed for a festival. But the wife of the elder brother did not keep all the laws, and the insulted elder bear-cub died, that is, he went to the men of the forest. When he reached them he complained that the common men did not keep the law, and made his way to the master of the mountains to seek satisfaction. On the way he took part in all the ceremonies of the festival, and having erased his dishonor, returned to his mother. This tradition gives a detailed account of all the principal rules of the bear festival and the laws that must regulate its arrangement so that the men of the forest may send good luck to the common men. This tradition shows even more definitely than the invocations that the Olcha consider the killing of the bear an act of reincarnation, an act which causes the bear, killed on the festival, to go to his parents, the forest men, and bring them the sacrifices of common men.

The Ainu believe likewise that besides the land of men (Ainu) exists a land of gods (kamui moshiri), which is inhabited by gods in human form. They build houses, light fires, and live on the whole as mortals do. When these gods go to the land of men, they are clad in bear, wolf, and other garments. The Ainu consider their meat and furs as gifts from the gods, and sacrifice brandy, gruel, etc., in acknowledgement of their goodness. The slain animal is called shumanan, returning, according to the Ainu, to its original divine form.¹¹ The Ainu call the bear killed at the festival "son of a mountain man."¹²

Frazer, not without reason, compares the bear festival with the custom of killing a sacred animal, which is widely spread among agricultural and pastoral peoples. Common to both is the idea of reincarnation, the idea of the annual return of the divinity, and a yearly occurrence of the festival in connection with it. The bear festival of Gilyak, Olcha, and Ainu can be considered a link in a widespread cycle of killing a worshipped animal with which is associated a belief in reincarnation. This belief explains the rule that forbids the breaking and hacking of the bones of the bear. This rule is strictly maintained, not only by the peoples of the Far East, but also by the Ostyak.¹³ The bones of the bear must all be gathered together and buried

¹¹ Masao, *Die Bärenfest bei den Ainu*.

¹² Dobrotvor-sky, *Ainu-Russian Vocabulary*, p. 41.

¹³ Gondatti, *The Cult of the Bear, etc.*

in a special receptacle. The loss or injury to a bone can evidently do an injury to the forest man into which the bear is to be transformed.

Finally, the manner of killing the bear is connected with the idea of reincarnation. The Ainu of Yesso tie the cub and drag him towards two poles called "the throttling poles." One is laid on his neck, and the other under his throat.¹⁴ Some one then shoots an arrow at him or pierces his heart with a knife. The blood must be carefully gathered into a vessel; not a drop may be spilt. If even one drop falls to the earth, the forest men take vengeance.¹⁵ The Ainu of Sakhalin, the Gilyak, and the Olcha kill the bear with an arrow. But in early times they used to choke him in the same way. Sternberg writes: "Although the death is usually immediate, several men attack the killed animal and begin to throttle it as though to precipitate the death and to stop its torment."¹⁶ It is quite evident that this symbolic throttling of the slain bear is nothing but a survival of an ancient custom of real throttling which was once practiced. This ritual throttling is still more strikingly expressed in the custom of the Olcha of "pressing" the dead bear with bushy willow sticks. This is also the reason why the Gilyak throw snow over the blood from the bear's wound, for fear some one might tread on it. The Ainu of Sakhalin also "press" the slain bear with a stick.¹⁷ Once they also had a fox and eagle festival besides the bear festival. The fox and eagle were killed by throttling.¹⁸ The Gilyak kill their dogs in a number of cases by throttling them with a leather loop.¹⁹ If in the course of the Sakhalin Ainu bear festival the bear is not killed in the required time, the host becomes furious and fines the bowman, taking one of his dogs, which is throttled on the spot and sent after the insulted bear with the aim of deprecating his anger.²⁰ The Vogul often throttle the sacrificial animal.²¹ The throttling of the sacrificial animal is evidently an ancient rite, connected with the idea of reincarnation of the animal, not a drop of whose blood may be shed.

THE TOTEMISTIC BASIS OF THE BEAR FESTIVAL

It would be wrong to satisfy oneself with the conclusion that the bear festival originated with the animistic idea of "the forest men" and the idea

¹⁴ After that some men press on the upper pole with the weight of their bodies and thus choke him

¹⁵ Batchelor, *The Ainu and their Folk-Lore*, pp. 208-209

¹⁶ Sternberg, *Gilyaks*, p. 61.

¹⁷ Pilsudsky, *At the Bear Festival of the Ainu*

¹⁸ Dobrotvorsky, *op. cit.*

¹⁹ Kreinowitch, *The Dog-Breeding of the Giliak*.

²⁰ Pilsudsky, *op. cit.*

²¹ Gondatti, *op. cit.*

of the reincarnation—the idea of a dying and reviving god. It is easy to show that this latter and best preserved layer of notions was preceded by another more ancient complex of ideas.

The Gilyak host and his clan do not eat the meat of the reared bear, which is all served to the guests, mainly to the narhi (sister's husbands), but instead kill a special dog and eat it at the conclusion of the festival. The Ainu have no such prohibition, as they have no clan organization, but the women that have suckled the bear do not eat his meat. This is evidently a survival of an ancient totemic clan tabu, which prohibits not only killing the bear, but also eating him. He is served to regale another clan—a clan of which wives are taken and to which wives are given. The Olcha host, together with his kinsmen—and this is the principle difference between the Olcha and the Gilyak festivals—eats the bear, but he has no right to kill it. Totemistic prohibition of eating the bear has evidently disappeared among the Olcha. The Olcha have kept up to now some totemic names of the clans (the clans of the hare, the fox clan). The problem, consequently, lies in showing why the bear has become a cult object of the whole tribe, and how the totemism is to be linked with the ritual killing of the bear.

It may be supposed that a process of selection and substitution of the totemic animal had taken place here, connected with the dying out of the totemic ideas, but with the retention of the formal ritual. N. Harusin,²² who has devoted a special study to the totemistic basis of the bear oaths of the Vogul and Ostyak, has already shown that the process of totem selection, of the forcing out of many totems by one and its transformation into a cult object of the whole tribe, is a common phenomenon in the religious history of primitive society.

Facts support this theory. The Ainu reared for their festival not only a bear but a fox and civet dog, both killed and eaten with the same ceremonies as the bear.²³ The Ainu also kept eagles which they considered good divinities. On a certain day they were throttled to make them “fly” to the god, and the same parting words were addressed to them as addressed to the bear. The Ainu performed the same ceremonies with a falcon and a kite.²⁴ So we know altogether of six animals which were worshipped by the Ainu in the same manner as the bear. This supports our supposition that in ancient times the Ainu had as many totems as there were clans. When the totemistic ideology of the Ainu was destroyed under the influence of migrations and contact with the Japanese, only the ritual killing of the animals

²² Harusin, *Bear Oaths, etc.*

²³ Dobrotvorsky, *op. cit.*, Pilsud-ky, *op. cit.*; Zolotarev, *Survivals of Totemism, etc.*

²⁴ Batchelor, *The Ainu and their Folk-Lore*, pp. 410–15.

and a festival connected with it remained. It is natural that only those animals most convenient for the arrangement of the festival, which were able to give an occasion for games and meat for the repast, have survived this cataclysm. In short, the animals most convenient for the cult, which before were totems of individual clans, turned into objects of tribal cult in proportion to the decay of totemistic culture. New ideological requirements, besides the acquisition of a meat resource, played a certain role in this selection. If to a totemistic minded man the whole of nature was anthropomorphic, to an animistic minded man the anthropomorphic particularities are left only to those animals most reminiscent of man by their conduct or appearance, the bear and the fox.

At the end of the nineteenth century, the Olcha and Gilyak still kept eagles in cages and sold their feathers to the Japanese and Chinese. But it is possible that, as with the Ainu, the keeping of these birds had also another significance not noted by the first explorers and forgotten by the present generation of Olcha. At any rate it is quite certain that the fox, hare, tiger, and wolf were totemic animals of the Olcha. It is interesting to note that the Gilyak bury the bones of the grampus and tiger in special containers similar to those used for bear bones and make sacrifices to them.²⁵ The Ainu and Orochi do the same with the bones of the grampus, and the Oroki (Sakhalin) with the bones of the seal.²⁶ The Olcha and Goldi bury the bones of the tiger in special receptacles. The Tungus and Samagir consider the kite a sacred bird. The Samagir believe that their genealogy takes origin from a kite. If they find a dead kite they bow to the earth before him, take him up, and put him on a tree.²⁷ All these tokens of respect are paid to the remains of animals who earlier were undoubtedly totems of separate clans. They are similar to the ritual of the burying of bear bones.

A further support for our hypothesis is the Olcha name of the bear festival, bujum hupu. Bojo, buju in Olcha, is not only a bear but also an animal in general. The Transbaikal Tungus call a wild reindeer and an elk bojun.²⁸ The Tungus of the lower Tunguska call bojun every wild hoofed animal.²⁹ The Orochi call boju an animal in general.³⁰ The meaning given the word by Olcha and Orochi is evidently the most ancient. The exact translation of bujum hupu is thus "play the animal." Hence the original meaning con-

²⁵ Zolotarev, *The Survivals of Totemism*, pp. 15-16.

²⁶ Vasilieff, *The Chief Traits of Oroki Ethnography*.

²⁷ Lipsky, *Mandchur-Tungusian Peoples*, p. 17.

²⁸ Titov, *Tungusian-Russian Vocabulary*, p. 22.

²⁹ Vasilevich, *Tungusian-Russian Vocabulary*, p. 19.

³⁰ Leontovich, *Russian-Orochi Vocabulary*, p. 71.

cerned not only the bear but various animals, animals in general. Our supposition is proved again: the bear is only a later substitute of the originally numerous totemic animals.

The theory of the totemistic basis of the bear festival corresponds well with the fact of the killing of the totem. The members of the totem "witch-etigrub" of the Arunta tribe gather a great number of grubs during the magic ceremony for the multiplication of their totem, and eat them after a special ceremony. The eating of their totemic animal is ordinarily prohibited to them. Besides, they richly regale the members of the other moiety with grubs. The members of the kangaroo totem also arrange a ceremony for the multiplication of the kangaroo. At the conclusion of the ceremony young men of this totem go to hunt kangaroo. The animals killed are brought to the old men, who remain in the camp. The elders eat a small piece of each animal. After that the meat is distributed among all present. The members of the kangaroo totem have also the right to eat on that occasion some meat of kangaroo from certain parts of the animal's body, while other parts they dare not even touch.³¹ The totemic tabu of the bear festival is even more strict. The bear of the Gilyak is killed by a sister's husband. All the meat of the totemic animal is given to the sisters' husbands and other guests. Only each of the old men of the host's clan receives a piece of the freshly killed bear's heart. Evidently the eating of bear meat was in early times forbidden to the host's clan among the Olcha. Instead of the bear they had to eat a dog. That is why a dog was brought into the house of the host on the day of the bear eating. Suppose that the bear is merely a substitute for numerous totemic animals and the resemblance with the Australian customs attains a degree of likeness.

THE DUAL ORGANIZATION

The foregoing description of the ngarka shows that this custom mirrors an ancient division into two exogamic moieties now vanished from everyday life. One of these moieties was called "Men of the Waters," and the other "Men of the Forest."

The Olcha house of ancient construction had two hearths. One, called "the fire of the forest," was devoted to the Men of the Forest. It was very strictly superintended. It was forbidden to stir this fire with a knife or to cook fish on it. Bear meat only was boiled there. "The fire of the water" on the contrary was not usually a sacred fire. It could be used for every purpose.

Each moiety consisted of six clans. The representatives of these clans

³¹ Frazer, *Totemism*, Vol. 1, p. 169

sat on the plank-beds in a strictly determined order. Each moiety had its leading clan, whose representative sat on the plank-bed nearest to the malu, the honorable part of the house. When the representatives of the twelve clans had taken their places, they began a rivalry in eating.

The picture revealed here is well known to every ethnologist. The division into two exogamic moieties which are very often called, "Men of the Water," "Men of the Forest," like the Olcha, is widespread in Australia, Melanesia, and North America. It is everywhere accompanied by a kind of curious hostility or rivalry between the members of the moieties. This rivalry finds its expression in ball games, mutual mockery, and all kinds of competitions. There is no need to describe the well known facts of this order.

It can be supposed that the division into two moieties once was known not only to the Tungus-Manchurian peoples, but also to the Gilyak. The Amur Gilyak have in their houses, besides the central big hearth, a small hearth called "bear hearth." A perpetual fire is kept on this hearth and it is strictly prohibited to cook anything on it.³² The keeping of a number of tabus is necessary while sitting on the plank-bed near the bear hearth. An origin of such a division of the house without any connection with the dual organization is improbable. But on the other hand it is possible that it is borrowed from the Olcha and Goldi together with the construction of the winter-dwelling, for the underground lodges of the Sakhalin Gilyak had but one hearth. In the works of L. Sternberg is found a desultory remark on the ngarka custom of the Amur Gilyak.³³ Unfortunately Sternberg's records do not show the social significance of this custom. Such information would have made possible a closer examination and solution of the problem of

³² Schrenk, *Natives of the Amur*, Vol. 3, pp. 76, 82, 99-100, Derbek, *Bear Festival*.

³³ Sternberg, *Gilyaks*, p. 217.

In the course of fieldwork in 1936 I found the ngarka custom among the Gilyak essentially similar to that of the Olcha. But the number of men who took part in the rivalry was not determined. Some clans included five, others seven. In the party sitting at the forest side of the house was one man more.

One of the clans seated on the plank-bed was the one that took the women, the other gave the women to the host's clan. The word ngarka derives from Gilyak ngarka pud, "the entertainment of the sons-in-law." In old underground dwellings of the Gilyak the hearth was divided into two parts, sacred and profane. These data revealed the connection of the ngarka custom not only with dual organization but with a system of closed intermarriage [between—?] three clans also, making the problem very complicated. It seems to me that in early times the Gilyak also had a dual organization, which was forced out by the system of closed intermarriage of three clans. In consequence the ngarka custom was also adapted to the new condition.

survivals of dual organization among the Gilyak. A survival of the moiety rivalry remains in the bear festival of the Negidals,³⁴ who borrowed their bear festival from the Gilyak. This fact shows the existence of a dual organization among the Gilyak also. But at present the data are too few for a definite solution of this complicated problem.

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³⁴ Ivanoff, *Amur Tribe*

HOPI AND NAVAJO CHILD BURIALS

By DONOVAN SENTER AND FLORENCE HAWLEY

FOR obvious reasons grave digging is not a profitable pastime for ethnologists, whose chief object may be gained only through friendliness with the Indian group. Moreover, as Southwestern funerals are usually private affairs and as informants are loath to speak on a subject so fearful to them, data on modern disposal of the dead has been scanty. Consequently, we were interested in the details of one Hopi and of two Navajo children's burials which had been removed by an anthropological hobbyist whom we chanced to meet.

The Hopi bury their dead in the talus slope of the cliffs leading down to a bench on the mesa. From reluctant informants, the Beagleholes¹ obtained our best description of Hopi burial customs. According to their two informants, men from the Second Mesa, the Hopi are very much afraid of their dead and avoid being in the room with a corpse or taking part in the funeral proceedings if possible. The clothing worn at death is left on the body, which is flexed after the hair has been washed and tied in place.

The father, or some member of the clan of the deceased, makes prayer feathers, one of which is tied to the hair of the corpse. One of the plumes is placed under each foot, one in each hand, and one over the navel. Cotton, symbol of the future existence of the dead as a cloud, is placed over the face. Food for the death journey consists of wafer bread and a gourd of water, these are placed where the thighs flex against the body. The corpse of a man is wrapped, preferably in a buckskin, but if that is lacking a woman's white wedding dress is used. The body of a woman is wrapped in her large or small wedding blanket, or in both. The burial bundle of either sex is then tied up in a blanket belonging to the deceased and is carried to the burial ground, to be placed, facing west, in a shallow grave, quickly covered with sand, and marked with a stick which serves "as a ladder for the soul (breath) to depart westward." On the next day a bowl of corn meal and five prayer sticks are placed on the grave, a prayer is said, and four parallel marks are drawn on the trail to close it so that the spirit may not return to haunt the village.

The Beagleholes' information on child burials consisted merely of the statement that children were buried in the children's cemetery and that infants were placed in graves in the same cemetery or in fissures in the cliffs.

¹ Ernest and Pearl Beaglehole, *Hopi of the Second Mesa* (Memoirs, American Anthropological Association, No. 44, 1935).

The burial procedure is the same as for adults except that the infant is wrapped in a "cradle quilt" provided by the maternal aunt. The spirit of an infant is believed to be reborn in another child of the opposite sex.

The Hopi child burial for which we have data had been taken from the children's cemetery on the bench at the eastern point of the Second Mesa, below Mishongnovi. The sites were heaped with low mounds of stones, and a short stick protruded a foot or two from the top of each. If the burial was in a crevice the stick projected from the earthfilled crack. On top of the graves were placed pottery bowls, granite pans, and baskets of food. Other funerary offerings placed on the graves were personal belongings of the dead. Offerings, bowls, sherds, granite pans, and baskets in various states of decomposition were found scattered over the talus slope, where they had been washed by the water which pours over the cliffs above after heavy rains.

The burial was that of an infant, probably about six months old. Below the pile of rocks on the surface was a covering of earth. Below the earth a sheepskin protected the burial bundle itself. This pelt probably took the place of the buckskin used in adult male burials. The bundle was wrapped with two small hand-pieced quilts, inside of which was a wrapping of old coarse muslin. Beneath this was another quilt like the others, securely tied by the corners. Another quilt was beneath, and inside it a cloth which had to be peeled away to find the mummified body of the baby.

The legs were drawn up and the arms were pulled down to the sides, in the flexed burial position. A prayer plume had been tied to a lock of hair on the forehead, and one was beneath each arm and each foot. Another was laid over the navel. The child was naked, except for a pair of moccasins made of rabbit skin with the fur turned to the inside. A small black, white, and green plaque, about four inches in diameter and of the coiled type made on the Second Mesa, was placed upon the abdomen over the prayer plume. Two folded cloth diapers were among the wrappings. A pair of knitted booties tied together by a ribbon was laid under the legs of the child, and a homemade cloth baby bonnet, decorated with blue feather-stitching and tied with ribbons, was under the head. Coarsely ground corn was heaped under the upper part of each arm. The face and front section of the head was covered with a piece of fine cotton cloth in which a mouth slit and two nasal apertures had been cut. The hair was covered with finely ground meal. There was no evidence of cotton over the face or head, but it is possible that the mask of cloth was intended to replace it.

No surface offerings were found with this burial, but on top of one rock pile marking a grave in the cemetery was found a weather-dried rubber

ball, a crudely carved wooden duck about four inches long, a wooden peg-top, a celluloid comb such as are used by women at the back of their hair, and a handful of tiny blue and pink glass beads, evidently the remnants of a modern necklace. On top of another pile of stones was a small bundle tied up in white cloth. Within the covering was a calico elephant from which the sawdust stuffing was leaking, an empty Post Toasties box, an empty milk can with two holes at the top indicating that the can had been emptied by intention or at least had been opened and left to evaporate on the grave, a pair of rubber baby pants, a tiny white dress of commercial make, a pair of baby stockings, and a white stocking cap, home-made, with a pink tassel of crochet thread at the peak.

The type of offerings left with these child burials makes it difficult to believe that the Hopi picture their dead ancestors, relatives, and children only as animate cloud beings or *katchinas*, who certainly would not need food—to say nothing of rubber pants. For at least a short period after death, the spirit apparently requires not only clothing and sustenance for the death journey but also personal treasures, such as toys.

Navajo child burials, as well as those of adults, appear to carry the same conception of future need of food and raiment for the spirit. It is well known that Navajo adult burials have been raided from time to time by white marauders looking for silver concho belts, turquoise rings, bracelets, and necklaces. The Navajo are afraid of their dead and may ask a white man to assist in the burial, but they are careful to equip the spirit with the possessions he cherished when alive.

Navajo burials are likewise in crevices, in talus slopes, or even out in the open and covered with a pile of stones. The two child burials shown us came from the central Rio Grande district of New Mexico. One had been marked with a large stone at the head, the other with a stone on which was cut a cross, evidently evidence of Christian influence. Both were in pine boxes. The bodies were dressed in Navajo costume and were wrapped in several layers of cloth. With one was a box of Crackerjack and a tin cup. The other had a stick of red and white striped candy and a spoon. Their necklaces were of commercial beads; the only silver ornaments were buttons down the blouse of one. These children were both about six years of age; in the mummified face of one the first premolar could be seen to have just erupted. The age of these children may explain the fact that they were clothed rather than nude like the Hopi infant.

A comparison of Navajo and Hopi burials with those of the prehistoric Southwest should provide data on the rate of acculturation in customs which are sometimes considered as among the last to be affected by outside

influence. Kroeber, in 1927,² laid out data from various parts of the world to show that burial customs are far less deeply rooted in a culture complex and hence are more changeable than hitherto has been supposed. He pointed out, however, that in dynastic Egypt and in the Southwestern Pueblo area mortuary customs had remained approximately stable over long periods of time. The Hopi and Navajo child burials examined confirm this by suggesting little change since prehistoric times in major traits of funerary custom, but they likewise show an appreciable influence from white culture contact in minor characteristics.

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² A. L. Kroeber, *Disposal of the Dead* (American Anthropologist, Vol. 29, pp. 308-15, 1927)

SAMUEL JAMES GUERNSEY, Curator of Archaeology in the Peabody Museum of Harvard University, died at Arlington, Massachusetts, on May 23, 1936. Born in Dover, Maine, in 1868, Guernsey attended Foxcroft Academy, Eastern Maine Conference Seminary, Wesleyan Seminary, and the Cowles Art School in Boston. He had been interested since boyhood in the Indians and their artifacts; and after moving to the vicinity of Boston to practice his profession as an artist he came into touch with Professor F. W. Putnam and Mr C. C. Willoughby of the Peabody Museum. These contacts led to an engagement to prepare figures, and later complete groups, for the Museum's series of models illustrating Indian life, and eventually to a full time appointment with the Museum, which he served as Hemenway Assistant (1910-1914), Assistant Curator of Archaeology and Ethnology (1915-1920), Assistant Director (1920-1927), and as Curator of Archaeology (1928-1935). During his later years he was also associated with Theodore B. Pitman, in the firm of Guernsey and Pitman, an organization which produced models for many institutions.

Guernsey contributed significantly to the development of museum technique and to knowledge of American archaeology. In the latter field he collected many specimens and much valuable information from New England. His principal activity, however, was in the Southwest, where, between 1914 and 1931, he conducted a long series of explorations and excavations in the Kayenta district of northeastern Arizona.

When Guernsey began his field work in the Southwest, almost nothing was known regarding the origins of Pueblo culture. The Wetherill brothers had, it is true, unearthed remains in southeastern Utah, which they believed to be different from, and older than, those of the Cliff-dwellers. But their findings, published in summary form by Prudden and Pepper, were looked upon with some skepticism. Nor had any serious attempt been made by other students to check their results and to determine whether or not there was genetic relationship between Basket Maker and Pueblo.

In 1914, during his first expedition to Arizona, Guernsey discovered Basket Maker burials in Monument Valley. His study of the accompanying specimens convinced him of the authenticity of the Wetherills' materials. He considered that they probably represented a very early stage of Southwestern development; but he realized that if such were the case, there must somewhere exist remains representing the period of transition. These, he reasoned, could only be identified on the basis of intimate knowledge of

both ends of the postulated series, and their intermediate position could not be established save on the basis of stratigraphic evidence.

Guernsey's subsequent researches were carried out in accordance with a carefully thought out plan. He undertook a meticulous comparative study of such Basket Maker and Cliff-dweller materials as existed in museums; and, in the field, devoted himself to an intensive search for caves which might yield further data. This was rewarded, in 1916, by the finding in Skeleton Mesa of the richly stocked and excellently preserved Basket Maker burials of White Dog Cave; and in 1920 by discovery, in Tsegi Canyon, of the long-sought intermediate remains. These consisted of houses and burials of what is now known as the Late Basket Maker, or Basket Maker III, culture, underlying deposits of an, until then, very imperfectly understood culture which he and others have since described as Early Developmental Pueblo (Pueblo I).

Thus were laid the foundations of our present knowledge of Pueblo development. Guernsey's work in this field was carried on with unflagging energy, keen intelligence, and a remarkable flair for the implications of technological and stratigraphic evidence. He also published fully, and as promptly as was consistent with thorough analysis of all available data. His investigations, together with the approximately contemporaneous studies of N. C. Nelson in the Rio Grande and E. H. Morris in the Aztec region, instilled new life into Southwestern archaeology: time-perspectives were lengthened, perception of problems was sharpened, and what had hitherto been a more or less haphazard hunt for museum specimens became a purposeful research upon the history of a culture.

Guernsey was also a very able museum man. Trained in the exacting Putnam-Willoughby school, with its strong emphasis upon detailed knowledge of specimens, he became intimately acquainted with the collections under his charge; and he had an unusual gift for the effective and instructive installation of cases. His greatest contribution, however, lay in the preparation and utilization of models. In this outstandingly important branch of museum technique he was preeminent. His models were marked by meticulous accuracy of detail, delicacy of craftsmanship, and beauty of composition. Anthropological models by Guernsey and his associates are in the Peabody and Buffalo Museums; historical groups in the Concord Antiquarian Society and the Newton War Memorial; and a long series of forestry models, which were his culminating achievement in this line, are to be placed in the museum of the Harvard Forest at Petersham, Massachusetts.

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CAMBRIDGE, MASSACHUSETTS

BOOK REVIEWS

NORTH AMERICA

La América Indígena: el Hombre Americano; los Pueblos de América. LUIS PERICOT Y GARCIA. (Historia de América y de los Pueblos Americanos, Vol. 1. xxxii, 727 pp., 8 color pls., 341 illus., map. 70 pesetas. Barcelona. Salvat Editores, S. A., 1936.)

This is the first of twenty-three volumes which are to comprise an ambitious work dealing with the aboriginal cultures and history of the Americas. Nearly a score of scholars will contribute one or more volumes to the series. Antonio Ballesteros y Beretta, who is director of the work, contributes a preface to the present volume.

The volume is separated into an introduction, a preliminary chapter, and two major parts or sections of three chapters each.

The introduction is devoted to a very useful history of studies made of the American indigenes. The time span between discovery and the contemporary scene is divided into four parts for this purpose and the text is illustrated by bibliographic material of South America taken from Nordenskiöld.

In the preliminary chapter the author performs a real service by sketching the "geographical frame." Forty pages are given over to a vivid description of the location, form, and dimensions of the land masses of the Americas, the geology, natural resources, coasts, climate, flora, and fauna, altogether an admirable picture of the settings in which we are to meet the natives with whom the remainder of the volume is concerned.

The First Part, called "The American Man," is an attempt to classify the American aborigine in terms of race and origin and to arrive at some understanding concerning the duration of his occupancy of the Americas. Accordingly the author is forced to review the classifications and controversies which have grown up around these questions. The first of the three chapters which comprise this First Part begins with a discussion of the place of the American man in the classification of human races. This is in reality a rather neat sketch of human classifications from the time of Linnæus. A more detailed description of physical traits is next offered, followed by a brief review of linguistic characteristics. Then comes a section on ethnographic traits, an excellent summary in forty-eight pages of the distributions of the most important cultural elements organized under such headings as dwelling, dress and ornament, agriculture, weapons, etc. Pericot draws heavily from Wissler for his distribution maps, and for South America he makes ample use of Nordenskiöld's work. He often turns the limited space to good account by bringing together on one page illustrations of a series of house types or boat forms to give a picture of diversity and distribution. The section which comes next, a review of the ethnography of America according to the Culture Historical School, is one which should be of particular interest to the ethnologists of this country, for its subject matter is rarely treated in books written in English. It is illustrated by four maps taken from the writings of Schnadt.

Chapter Two of the First Part is devoted entirely to the problem of Quaternary Man in America. The archaeological finds and the claims made for them are reviewed in detail in the course of over a hundred pages. The author rejects the claims for their extreme antiquity, but he does state as his opinion the impossibility of denying that the American aborigine existed before the end of the Pleistocene (p. 409). He sets the probable entry after the end of the glacial epoch (p. 412).

The third and concluding chapter of this massive First Part deals with the problem of the origin of the American native. Every theory once proposed, no matter how fantastic, and whether advanced for scientific or Biblical reasons, is faithfully reviewed. In his conclusions Pericot holds that the only route of entry into America known with certainty is that of Behring Straits. He sees the brachycephalic, mongoloid type as the predominating physical strain of the native population, but insists that the American native is far from a unified stock and that the prevailing physical characteristics of recent times were superimposed upon long-headed peoples of a different physical type. The determination of the route by which these earliest settlers reached the Americas is treated as one of the important tasks of further anthropological research, and the author leaves the door open for an Oceanic solution of the problem.

The Second Part of the volume, entitled "The Peoples of America" is a rapid tribal survey in which each group is treated according to name, divisions, linguistic affiliations, physical characteristics, and origins and movements.

There are two additional features which greatly enhance the value of the book. One is the full and scholarly bibliographic notes to which over a hundred and sixty pages are devoted and which may serve as a point of departure for anyone who wishes to know more details about a particular subject than could be included in the text. The second is the beauty of the volume. The 341 figures and eight color plates which embellish the book are as artistic a choice of photographs and illustrations as can be found between two covers. The volume is provided with a detailed table of contents and an index of maps. This book is to be followed by a companion volume of the same title which will be concerned more particularly with the American cultures. It is to be hoped that the author will include in this second book an alphabetized index to both volumes.

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Evidence of Early Man in North America. EDGAR B. HOWARD. (Museum Journal, Vol. 24, Nos. 2-3, 1935. Reprint: 158 pp., 26 pls. \$1.50. Philadelphia: University Museum, 1935.)

Howard's paper presents the results of six years of field, laboratory, and library work on the evidences of early man in America. Opening with a clear presentation of the location, finds, and technique of excavation at Burnet Cave in the Guadalupe Mountains of southern New Mexico and in the ancient lake beds near Clovis, New Mexico, he points out that in this complex problem, which rests between archaeology and geology, even the best archaeological technique is not adequate. Conse-

quently, Howard, in his effort to understand the connotations of the flint point found with bones of extinct animals far beneath what appears to be a Basket Maker level in Burnet Cave and of the knife-like scraper taken from near bison bones in a pit near Clovis, illustrates an admirable use of the cooperation offered by technicians in other sciences. Increase and decrease of salinity during deposition of gravels near Clovis is indicated by diatom analysis. Identification of ancient trees indigenous to the district is made from charcoal taken from ancient hearths. Pollen analysis, which was hoped to solve something of the sequence in climatic changes, offered little aid in this case because of the scarcity of the pollen. Identification of invertebrates and of vertebrates associated with the evidences of man pointed to the pluvial Pleistocene period with a climate very different from that of today.

After a careful analysis of his own finds, Howard briefly synthesizes all other data available on the presence of early man in North America. Modern scientists are evaluating this data on five bases of evidence, varying in importance: physical type, association with extinct animals, stratification, permineralization, and patination of artifacts. Distribution of the Folsom and Yuma artifacts is listed, together with a distinction of the former into two groups, the Folsom and the Folsom-like points, based upon an average of measurements and observations taken on the points found at Folsom, New Mexico. While many stone points will undoubtedly fall into the borderline between the two groups, such a classification may aid in clearer comprehension of verbal descriptions of those points which we may expect to find reported in the future from the far corners of the continent.

Howard concludes that man could have entered America via the Behring Straits while that district was above water in the ice age, 40,000, 20,000, 15,000 to 10,000, or 5,000 years ago. During a period when the much discussed "open corridor" was open, he wandered southward. The finds of recent years make it evident that ancient man settled over a large part of America. What he made, where he lived, and what animals he hunted are becoming better known year by year, and although the man himself has not yet been found, that discovery may be expected through just such careful work as that described by Howard and as that of Howard himself. The solution of the problem of the exact date of man's entrance into America and of his occupation of any one area will take many years of concentrated scientific effort, but the impetus afforded by accounts of meticulous work and the attempts to deduce unbiased theories concerning early man in America will push the study sturdily onward.

FLORENCE M. HAWLEY

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Yale University Publications in Anthropology, Nos. 1-7. EDWARD SAPIR AND LESLIE SPIEGEL (eds.). (145 pp., 1 pl., 5 figs. \$2.00. New Haven: Yale University Press, 1936.)

This new series of publications will

embody the results of researches in the general field of anthropology which are directly conducted or otherwise sponsored by the Section of Anthropology of the Department of the Social

Sciences in the Graduate School, the Department of Anthropology of the Peabody Museum, and the Department of Anthropology of the Institute of Human Relations. The issues, ranging in size from brief papers to extensive monographs, are numbered consecutively as independent contributions and appear at irregular intervals [Editorial note].

The publications in this volume cover a diversity of topics and all except one deal with the aborigines of North America. It will only be possible to indicate briefly the scope of each contribution here.

1. Clark Wissler, *Population Changes among the Northern Plains Indians* (20 pp.). This is a compilation and analysis of the relative size of the Blackfoot group of tribes, the Assiniboin, and Western Cree during the fur trade and later reservation period. The author is "concerned with population changes, rather than absolute size." The data suggest some correlation between increase of population and the expansions in power and territory during the earlier period. The reservation system has acted as a stabilizing force. After white contact smallpox was conspicuous as a menace but there was a rapid recovery of numbers. An optimum population, characteristic of each successive dominating period, appears to have been an important controlling factor, representing a saturation point.

2. Peter H. Buck, *Regional Diversity in the Elaboration of Sorcery in Polynesia* (19 pp.). Five regions, each represented by an island group (Tonga, New Zealand, Tahiti, Marquesas, Hawaii), are selected for analysis and comparison. Offensive, defensive, and protective techniques used in sorcery are reviewed and there is a summary table (p. 18). In conclusion it is pointed out that the study illustrates how a "complex based on a common pattern underwent different developments in different parts of the same culture area," and stress is laid upon the fact "that a local development within a culture cannot be accepted as a characteristic trait of that culture for comparison with similar traits in other cultures to prove theories of diffusion."

3. Leslie Spier, *Cultural Relations of the Gila River and Lower Colorado Tribes* (22 pp.).

Recent investigation centering on the Maricopa and allied Yumans lying on the Gila between the Pima and the lower Colorado groups makes it clear that the Gila and the Colorado form one culture province. Maricopa culture is nearly identical with that of the Lower Colorado Yumans; Pima and Papago also are in large measure participants in the same culture. The purpose of this paper is to establish this relationship and in particular to show the cultural relations of the Maricopa to other Yumans to the west and to the Pimans to the east.

An analytic tabulation (pp. 16-22) exhibits the culture elements peculiar to the Maricopa, the Pima-Papago, and the Lower Colorado tribes, respectively, and those shared in common by all three groups, by Maricopa and Lower Colorado Yumans, and by Maricopa and Pima-Papago.

4. Ernest Beaglehole, *Hopi Hunting and Hunting Ritual* (26 pp.). The hunting of antelope, deer, mountain sheep, rabbit, eagle, and smaller mammals and birds is described particularly "with reference to the ritual that accompanies each stage of the hunting cycle" and the prevailing attitudes towards animals. Some comparative notes on other Pueblo peoples are added.

5. W. W. Hill, *Natcho Warfare* (19 pp.). A descriptive account of "the formalized pattern of warfare and its relation to the Squaw or War Dance and the natchit."

6. H. Scudder Mekeel, *The Economy of a Modern Teton Dakota Community* (14 pp.). Taking as his point of departure the values associated with the concept of the Good Man, Mekeel discusses the "master ideals" expressed "in the ancient economy and what force they exert today." Agriculture has never entered deeply into the cultural pattern. Unlike the aboriginal economic patterns, "it is not an integral part of the social whole, coalesced by sentiment, reenforced by value, and maintained by prestige."

7. Cornelius Osgood, *The Distribution of the Northern Athapaskan Indians* (23 pp.). This contribution provides an invaluable Baedeker to an enormous area in North America, much of which still remains ethnographically unknown. Osgood distinguishes twenty-five major groups and maps their boundaries (p. 4). The text is systematically arranged, giving for each designated group: *Range* (historically oriented), *Comment* (e.g. sub-units which have been recognized), *Reference* (sources of general information), *Handbook* (correlative term in *Handbook of American Indians*), *Name* (reasons for the selection of the name used for the major group). This publication closes with a general discussion of cultural and linguistic classification within the northern Athapaskan area as conceived by other scholars.

It is sincerely to be hoped that the Yale Series will prosper and make available at frequent intervals further contributions of the intrinsic worth and high level of scholarship evidenced by these initial publications.

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Geographical Names of the Kwakiutl Indians. FRANZ BOAS. (Columbia University Contributions to Anthropology, Vol. 20. 83 pp., 22 maps. \$4.00. New York: Columbia University Press, 1934.)

The bulk of this volume consists of an alphabetical list of the place names (46 pp.) and the 22 folded maps with the necessary keys, listing some 2,323 places which are located on the maps by means of numbers. Since most of the maps are necessarily on a small scale, the locations are seldom exact enough to enable any future traveler who might wish to do so to determine the location of a given place.

A few pages are devoted to a discussion of the general type of place name. The Kwakiutl as a sea-faring people naturally incline to naming points along the coasts, islands, rivers, etc. Expectably there appear place names for campsites, names derived from the peculiar form or aspect of the spot, and designations of localities noted as sources of food. Others refer to things mentioned in traditions, such as mythical beings, the supposed place of origin of the group, and so on. The author's all too brief discussion (pp. 18-21) of the influence of linguistic forms on the formation of and therefore to a certain extent on the type of place names is more stimulating. Thus the Kwakiutl language employs locative suffixes which evidently lead to the employment of names with such terminations. In Eskimo such forms are

missing and Eskimo place names differ accordingly. There are brief references to the influence of language on place names among the Aztec, Tewa, Keresan, Zuñi, and Dakota.

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Kwakiutl Culture as Reflected in Mythology. FRANZ BOAS. (Memoirs, American Folk-Lore Society, Vol. 28. xii, 190 pp. \$3.50. New York: G. E. Stechert and Co., 1935.)

In 1916 Professor Boas published his discussion of Tsimshian tales. The volume under review adds supplementary analyses and comparisons to the earlier and more extensive work. The purpose of both of these analyses of Northwest mythologic material is best conveyed in the author's own words.

The underlying thought of this attempt was that the tales probably contain all that is interesting to the narrators and that in this way a picture of their way of thinking and feeling will appear that renders their ideas as free from the bias of the European observer as is possible . . . The crucial test of the value of such a comprehensive study can appear only when different cultural groups are compared.

In these two sentences we probably have as pregnant an expression of Boas' objectives in the field of cultural anthropology as he will permit himself. Professor Boas would be the first to object to generalizations on the basis of his suggestive statements, yet I feel that we have epitomized here the threefold purpose of his insistence upon careful comparative analyses. First, they may reveal certain historical trends; secondly, they may yield those features common to a region or even to mankind in general, and lastly they give the truest appreciation for the uniqueness of each tribal configuration. It is particularly the two latter purposes which are emphasized in the present volume.

In the actual analysis of material nineteen published sources are drawn upon. Attitudes and descriptive data are organized about an orthodox framework of ethnographic presentation. The work is concluded with nineteen pages of detailed "Comparison of Cultural Reflections and Style in Kwakiutl and Tsimshian Mythologies." This section is again a resumé of modes of life in terms of orthodox ethnographic organization. An important difference in motivation between the tales of the two cultures is brought out.

The Tsimshian like to give a moralizing tone to their tales . . . The principal motivation in Kwakiutl tales is the unlimited desire to obtain new crests, names, dances and other privileges . . . (pp. 183-84).

If this careful and scholarly volume is open to any criticism, it is the inability of the reader to distinguish between the theoretical structure of Kwakiutl culture as presented in their own mythologic fantasy and the practical functioning of their everyday life. This criticism probably holds for much of Boas' invaluable archival records of Kwakiutl culture as published to date. This is naturally no criticism of

work done, but rather a plea for further elucidation. It would be highly interesting, for instance, to see the same careful analysis of the myths paralleled as far as possible by actual custom either as observed by the ethnographer or reported in non-mythologic terms by informants.

CORA DU BOIS

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The Archaeology of the Deer Creek-Cosumnes Area, Sacramento Co., California. J. B. LILLARD AND W. K. PURVES. (Bulletin, Sacramento Junior College, 1, 1936. 21 pp., 36 pls.)

Sacramento College has for several years been exploring the lower Sacramento Valley, an archaeologically almost unknown area, under the leadership of President Lillard. The present bulletin is welcome as the first-fruits of this work. It reports on three sites in an inter-stream tongue of bottomland in the lowest foothills east of the lower Sacramento. Three cultural levels are distinguished, Early, Intermediate, and Recent, the last containing metal, buttons, and beads. The differentiation between the two older levels is, as always in California, not particularly striking, but seems definite. The Early phase is characterized by: burials extended as well as flexed; cremation rare; spearpoints numerous, arrowpoints absent; charmstones plentiful in spots. The Intermediate phase is marked by: burials flexed only; cremation; spearpoints rare, arrowpoints abundant; charmstones apparently not common; baked clay balls; incised bird bones; clamshell disk-beads; stone beads; etc. This line-up accords fairly with the non-stratigraphic time-classification tentatively suggested by Schenck and Dawson for the Lodi-Stockton area immediately south. On account of the rarity of time distinctions in the available data on California archaeology, the Sacramento College findings are important. The work on which they rest appears competent and sound. The chief defect of the bulletin is its over-brevity. The footnotes suggest a background of fuller and areally more extensive observations and comparisons of which the present report is only a fragment. It is to be hoped that President Lillard and his associates will do their broader data justice by publishing them at first opportunity.

A. L. KROEBER

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Yuman Pottery Making. MALCOLM J. ROGERS. (Papers, San Diego Museum, No. 2. vii, 44 pp., 10 pls., map. \$1.25. San Diego: San Diego Museum, 1936.)

Mr Rogers has produced a valuable and much needed account of pottery making in the Far Southwest, which forms a fine counterpart to Dr Guthe's exposition of pottery making in the Near Southwest (*Pueblo Pottery Making*, 1925). It is a technical as well as a geographical counterpart, for it deals primarily with the paddle-and-anvil technique of pottery manufacture, while Guthe's work deals with the manufacture of pottery without paddle and anvil.

A wealth of detail for various groups, much of it from personal observation, is presented. Moreover, archaeological material is utilized in an apparently conserva-

tive, but helpful, fashion to round out the deficiencies in the ethnological data. A fine series of pictures of potters at work, of implements, of pot outlines, and of decorative motives visualizes the points of Mr Rogers' excellent essay. For orientation an outline map is included.

Possible external relations of the paddle-and-anvil method are not discussed, and probably do not warrant discussion in the present state of knowledge. However, the implication that the mushroom-shaped pottery anvil is a local invention and probably by the Cocopa (p. 36) leans too heavily on the thesis of independent invention, in view of the wide distribution of this implement in the Mississippi Valley. To be sure no connection with the Mississippi Valley has as yet been established. Yet the conservative view would be to regard the problem as unsolved.

American anthropologists in general and Southwestern archaeologists in particular will be grateful to Mr Rogers for his fine exposition of Yuman ceramics.

E. W. GIFFORD

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The Mogollon Culture of Southwestern New Mexico. E. W. HAURY. (Medallion Papers, No. 20. 146 pp., 34 pls., 32 figs. Globe, Ariz.: Gila Pueblo, 1936.)

Some Southwestern Pottery Types, Series IV. E. W. HAURY. (Same series, No. 19. 49 pp., 4 pls., 9 figs. 1936.)

These reports record results of excavation in two early sites of the Mimbres area, and advance theories of broad implication regarding the prehistory and ethnic relationships of early Mimbres people.

Three distinct stages of development, or phases, preceding the classic Mimbres are defined. They are characterized by distinct house structures and changes in ceramic styles, and are equated in time with Basket Maker-Pueblo development by tree ring dates from one phase. Because of differences in material culture and physical type it is suggested that this early culture is distinct from Basket Maker III and Hohokam and that it be called the Mogollon. The theory that the Mogollon people are racially related to a Caddoan stock is based principally on measurements of one skull, and observational data from sixteen others, which showed undeformed brachycephaly and a low cranial vault. Some of the changes which culminated in classic Mimbres culture are attributed to Pueblo influence, some to Hohokam. In explanation of the final disappearance of Mimbres culture the theory of absorption by Chihuahua people is advanced.

The format of the report introduces an innovation which has much to commend it. Data are first presented by means of a series of plates, brief descriptions and explanations being given opposite each. Findings are then summarized and theories stated in a section headed "Discussion." The advantages of such a treatment both for convenience in reference and because of the necessary separation of fact and theory are at once evident. The reader may question whether description is not abbreviated but this does not seem to be an inherent defect of the system. Inadequate illustration is noticeable, however, in the pottery report. The pictorial record

is to a certain extent made subservient to the presentation of theories of development of form and relationships of design. Shapes are shown by outline drawings, a large number of which (43 percent) are restored from sherds, and unfortunately parts which are conjectural are not distinguished from the known.

The pottery descriptions are concise and systematic, and design is logically analyzed. The pottery types are, with one exception, well established and have long been recognized by workers in this field. San Lorenzo Red-on-brown is distinguished from Mogollon Red-on-brown by quantitative differences only. Particularly important is the fact that even with Dr Haury's meticulous methods of excavation, it was not established stratigraphically, and its omission from all tabulations of sherds suggests that its classification was uncertain. The comparison of Mogollon and Basket Maker III painted pottery implies a relative technological advance for the Mogollon people which it would be difficult to defend.

Certain unproved theories regarding classic Mimbres culture are stated with finality; for example, that the common burial custom was intramural, and that red ware continued to be made. There is a noticeable contrast between the assurance of statements that Mogollon Red-on-brown "gave birth" to many other pottery types, some of which are far removed in time and many of which have not yet been systematically studied, and the caution of the discussion of the relation of Mogollon and Hohokam, the culture for which Dr Haury is best prepared to present the facts.

The impression is unfortunately conveyed that a hitherto unknown culture has for the first time been recognized. In justice to a previous worker, it should be stated that the essential features of the developmental sequence were established by the excavations of the late Wesley Bradfield, and although never published, his results were well known to his colleagues in this field.

Sufficient data have not yet been obtained to permit a critical examination of the many theories of relationship advanced by Dr Haury, but his investigations have made available information regarding an important culture stage, and his interpretations of evidence will prove most stimulating to Mimbres students. He has presented fundamental problems which must be considered by all future workers in this field.

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LABORATORY OF ANTHROPOLOGY

MEXICO AND SOUTH AMERICA

The Comparative Ethnology of Northern Mexico Before 1750. RALPH L. BEALS (Ibero-Americana: 2. vi, 133 pp., 28 maps. \$1.35. Berkeley: University of California Press, 1932.)

The Distribution of Aboriginal Tribes and Languages in Northwestern Mexico. CARL SAUER (Ibero-Americana: 5. vi, 94 pp., map. \$1.00. Berkeley: University of California Press, 1934.)

Although the series *Ibero-Americana* (edited by Bolton, Kroeber, and Sauer) has been in existence for several years, it has not yet had the attention its impor-

tance warrants. Designed to cover Latin America cultures, pre-Columbian and historic, its importance for Americanists to date lies in its concentration in the cultural no-man's land of northern Mexico.

Of outstanding importance is Beal's survey of the whole area. The principal source material is documentary (avowedly not an exhaustive study, but certainly comprehensive); modern ethnographic material is collated, sufficient of the American Southwest is included to link the whole with surveys north of the boundary. The occurrence of individual traits is given in a series of tables and spot-maps in the manner of Nordenskiöld's South American studies, appended to a discussion of the nature of the traits and their historic relations.

A second section tentatively defines culture provinces, and third is an interpretation of the relations of Southwest and Southeast to Northern and Central Mexico, and the latter pair to South America. Northern Mexico is far from homogeneous: the greatest cleavage separates the nomads of the central Sierra from the agriculturalists of the west coast and Tamaulipas on the east. The old assumption of a gradual transition from Pueblo to central Mexican culture for the entire area must be abandoned. So far as it is true, a line separating the two falls just south of the Cahita and Tarahumare—a line cross-secting agricultural and nomadic areas. Trait distributions indicate the principal contacts with the Southwest by direct routes along the west coast and along the Sierra, secondarily via Tamaulipas. The Southeast shows not only similarities to the Southwest and northern Mexico, but is linked by another set of significant traits with southern Mexico.

Equally fundamental is Sauer's thoroughgoing revision of tribal and linguistic groupings which supplants earlier mapping by Orozco y Berra (1860) and Thomas and Swanton (1911). The area cannot be mapped as of a single date: Sauer's presentation follows the northward moving Spanish record, 1531–1768. Earlier attempts at mapping suffered in their failure to recognize the extent of shifts, telescoping, and replacement of the native population under Spanish control (especially notable in the more southerly areas), and failed to note sufficiently that the Spanish commentators frequently rested on the statements of their predecessors. Sauer's careful sifting of the evidence, here given in necessarily abbreviated form, guards against these pitfalls, adds much new evidence, and is tempered by his own familiarity with much of the country. On the whole, it is true that this is "a study of contemporary sources, their evidence and judgments are accepted," but the author frequently injects conclusions of his own where the evidence is confused. Tucked away are some highly suggestive and shrewd hints of cultural and linguistic relations (see, e.g., page 82).

Beals also offers a tribal map: that the two do not quite agree is intelligible in view of the nature of the documentary testimony. Reconciliation will await further documentary and ethnographic data. Both certainly need revision in the Gila River region of Arizona, where Beals may be following an erroneous mapping of the present reviewer, Sauer resting on Bolton's study of Father Kino.

Both works make it very clear that our present-day ethnographer of northern

Mexico must not only be wary in discriminating Mexican from aboriginal elements, but must reckon with extensive cultural and tribal shifts.

LESLIE SPIER

YALE UNIVERSITY

An Historical Legend of the Zapotecs. PAUL RADIN. (Ibero-Americana: 9. 30 pp. \$0.35. Berkeley: University of California Press, 1935.)

Aboriginal Population of Northwestern Mexico. CARL SAUER (Ibero-Americana: 10. 34 pp., map. \$0.35. Berkeley: University of California Press, 1935.)

Dr Radin's contribution is the text and translation, with commentary, of a legend relating to the marriage of the last Zapotec king (Kosixwesa) to the daughter of the Aztec ruler Ahuisotl, the father of Montezuma. The text, obtained in 1912 from a young Zapotecan of the pueblo of Zaachila, former capital of the old Zapotec kingdom, differs in some significant points from the Burgoa version of the same legend made use of by Bancroft and Brasseur. After considering and discounting the possibility that the version here recorded is but a post-Columbian distortion or romantic retelling of the Burgoa story, a tale which many modern Zapotecs have had opportunity to hear, Dr Radin inclines to the belief that he has secured a version not based on a written source but deriving from a persistent oral tradition. Such being true, we have here a Zapotec rendering of a tale which, though possibly more romantic and less historically exact, has until now been known to us only from the Burgoa version based upon Aztec sources.

The second work is "an attempt by a historical geographer to consider density of population for one particular area." And a noteworthy attempt it is! Dr Sauer has gone beyond most investigators in his search for population figures: not only has he consulted the original Spanish records in print and in Mexican archives; he has repeatedly visited the regions in which these populations lived, has checked their crops and technology, the soil and climate, has considered the effects of disease and famine, has scanned present population figures and means of subsistence, and has reconnoitered the archaeological remains of ruins, with the result that his estimates, computed for the most part by converting documentary statements of numbers of warriors, families, baptisms, and houses into total population figures, are further validated by actual observation of whether or not a given population with a given technology and cultural adaptation could have subsisted in the area attributed to it. His conclusions, contrary to much anthropological opinion, sustain the oft discredited larger population estimates of the Spanish recorders, giving to "Northwestern Mexico" from the Gila to Rio Grande de Santiago a prehistoric population of over half a million, "almost three-fourths of the number now living [1920] in that part of Mexico." Separate figures are given for the various ethnic (linguistic) groups in the region. Dr Sauer has set a fine methodological example for future investigations of like nature.

A table of aboriginal population and density of population compared with the

1920 figures for the same area, and a map showing aboriginal tribal distribution and subsistence are valuable features of the report.

JOHN H. PROVINSE

UNIVERSITY OF ARIZONA

Studies of the Yaqui Indians of Sonora, Mexico. W. C. HOLDEN, C. C. SELTZER, R. A. STUDHALTER, C. J. WAGNER, AND W. G. McMILLAN. (Bulletin, Texas Technological College, Vol. 12, No. 1. Lubbock, Texas, 1936.)

This collection of short papers, resulting from an expedition organized and directed by William Curry Holden of Texas Technological College, is an excellent example of what can and cannot be accomplished without adequate ethnological training. The parts by Holden on society and some phases of religious rituals are meticulous in the account of things *observed*, superficial and full of misinterpretations in the explanatory passages.

In this respect the sections on "Yaqui Architecture" by McMillan, "Medical Practices of the Yaqui" by Wagner, and "Yaqui Agriculture" by Studhalter are superior; they confine themselves almost entirely to concrete description and do not attempt any interpretation. Even so they are occasionally marred by flights of naïve romanticism.

The contribution by Seltzer on the "Physical Characteristics of the Yaqui Indians" alone conforms to professional standards, although, as Seltzer points out, a larger series of measurements will be required to settle some of the problems he encountered. Points of primary importance are the high degree of heterogeneity in the Yaqui population and the presence of an important percentage of negroid characteristics which are ascribed to a racial basis. The author does not commit himself to the view that the negroid element is either pre-Columbian or post-Columbian, suggesting, however, a thorough study of the historical literature. So far as the better known literature is concerned, the reviewer is confident no evidence of post-Columbian negroid mixture of significance will be found anywhere in northwest Mexico. It would be interesting to know if similar negroid characters occur among the closely related Mayo or the Pima.

RALPH L. BEALS

UNIVERSITY OF CALIFORNIA AT LOS ANGELES

The Dominican Mission Frontier of Lower California. PEVERIL MEIGS, 3RD. (231 pp., 19 pls. Berkeley: University of California Press, 1935.)

Dr Meigs' study is primarily one in historical geography, based on a combined use of field observations and documents. Owing to the virtual extinction, long since, of the Indians of the peninsula, the ethnologist must operate in this area mainly with data secured in non-ethnological connections. To such studies the present one is obviously a notable contribution. The landscape, the historical development of the missions, and especially the chapter on aboriginal population (pp. 133-42) are

directly important. The area is that north of latitude 30° ; the tribal groups are the Cochimi, Kiliwa, Akwa'ala-Paipai, and Southern Diegueño-Kamia; the period is 1767-1855.

A. L. KROEBER

UNIVERSITY OF CALIFORNIA

The Head-Hunters of Western Amazonas RAFAEL KARSTEN. (Societas Scientiarum Fennica, Commentationes Humanarum Litterarum, Vol. 7, No. 1. xvi, 598 pp., 35 pls., figs., map. Helsingfors, 1935.)

This volume is a general ethnography of the Jibaro and Canelos Indians of eastern Ecuador and Peru. The term Jibaro covers several distinct tribes, closely affiliated in customs and language, but politically independent and frequently hostile to one another. The Canelos are a mixed mission group, composed of Jibaros, Qichua-speaking Indians from the Sierra, and Záparos. Most interest adheres to the account of the Jibaros, which forms the larger portion of the study.

Most of the present volume has appeared previously in the form of separate papers published in widely scattered sources in the United States, South America, and Europe. Few are normally accessible to students in this country except *Blood-Revenge, War, and Victory Feasts Among the Jibaro Indians of Eastern Ecuador* (Bulletin, Bureau of American Ethnology, 79, 1923). Consequently the present republication will be appreciated for its convenience, moreover it presents a more coherent picture.

The principal additions to the collected papers seem to be the Introduction (20 pages) and a section entitled "My Journeys in Western Amazonas" (64 pages). The latter apparently is intended to have popular appeal. Although useful in giving a view of the environmental setting, it seems needlessly long for scientific purposes. Perhaps its greatest value is in revealing some rather naïve attitudes on the part of the author towards primitive peoples, not the least interesting part being the appearance of the personal pronoun as many as fifteen times to a page. One may also question the propriety of criticizing the superficiality of other South American ethnographers when an exceptionally long stay in one house group by Karsten turns out to be two weeks.

Important is the difference in Karsten's fundamental approach from that of many other recent South American workers. Although one will seek in vain for the meticulous accounts of material culture and extensive comparative analyses to which Nordenskiöld, Métraux, Tesson, and others have accustomed us, there is much recompense in this attempt at a balanced picture of the full life of a South American people treated in terms of definite ethnic units. Too often has our South American data come to us in the form of an atomistic collection of culture traits presented in terms of areal occurrence. Karsten fails to give technological details of material culture, frequently dismissing such matters as too difficult to describe, although the crudest of sketches would solve the difficulty; on the other hand he presents valuable data on incantations, major and minor ceremonials, dances, and

glimpses of the intellectual furniture of the Jibaro mind which are rarely to be found in other writers.

The treatment of social organization, although fuller than usual, fails to grapple with fundamental problems. Cross-cousin marriage is described without any indication that its significance is recognized. The specialist would have appreciated concrete data on genealogies, the composition of households, etc.

North American ethnologists should be impressed by the many additions Karsten gives to the evidence for resemblances between the simpler tribes of North and South America. Portions of the Victory Feast and elements of the mythology are particularly striking.

RALPH L. BEALS

UNIVERSITY OF CALIFORNIA AT LOS ANGELES

AFRICA AND OCEANIA

The Present State and Future Development of Ethnographical Research in South Africa.

I. SCHAPER (ed.) (Bantu Studies, Vol. 8, No. 3, pp. 219-342, Johannesburg, 1934.)

Workers in any science should pause occasionally to take stock of what has already been accomplished and of what apparently remains to be done. The present report, prepared by a committee of South African ethnologists, does this with considerable success. It begins with a brief account of the development of ethnological studies in South Africa but is devoted mainly to a listing of the various native groups within the area, with clear and direct statements as to possibilities of further work with each, tribes or aspects of culture on which adequate information is lacking, etc. It concludes with a brief discussion of facilities for training and field work in the region, and with a selected bibliography which will be of great value to all African students. The approach is practical rather than theoretical and the importance of studies of present as well as past phases of native life is stressed throughout. A series of similar reports dealing with other regions would be of great value.

RALPH LINTON

UNIVERSITY OF WISCONSIN

Uganda. H. B. THOMAS AND ROBERT SCOTT (559 pp. \$5.00. London: Oxford University Press, 1935.)

Anthropology in Action. G. GORDON BROWN AND A. McD. BRUCE HUTT. (272 pp. \$2.75. London: International Institute of African Languages and Cultures, Oxford University Press, 1935.)

Though *Uganda* is a handbook "compiled from official records," it is not an official publication, and it has studiously avoided being a mere compilation, though not with entire success. It contains a great deal of information carefully organized into chapters of startlingly different interest, style, and thoroughness. It is particularly useful as the only up-to-date guide book for the prospective visitor, with roads mapped, hotels and their rates indicated, and descriptions of climate conditions,

beauty spots to visit, and so forth. There are chapters on the flora and the fauna meaningful only to the expert, detailed accounts of the government of the protectorate, and some picture of its economic condition and prospects.

The material on the native populations is on the whole accurate, but not comparably detailed or adequate. There is a fair enough presentation of current theory about the early history of the region, but the information on physical types, language groups, and the native way of life is decidedly cursory. There is not even a map of tribal distribution, which would have been of distinct service. The only one of the eight maps which is of any ethnological significance is one of population densities.

The book contains a good though not exhaustive bibliography and a very adequate index.

Anthropology in Action is not written especially for anthropologists. It deals with the Hehe, a rather typical Bantu people of Tanganyika, but it does not attempt to present a full picture of their culture. The book is the report of an experiment in collaboration between a government officer and an anthropologist. Questions asked by the administrator limited the field of the inquiry, and only data relevant to them were included in the report.

This work is among the early fruits of the campaign of the African Institute for the application of the systematic understanding of the culture of a people to the problems of its administration. It is as a demonstration of the fruitfulness of such application—a point which has not been so clear to administrators as it has to anthropologists—that this study is of most value. To this end its practical conclusions are particularly important. Mr Hutt obviously benefits from the knowledge that the privilege of fining tends to be abused by Hehe magistrates; that the people misunderstand the plural wives' tax and assume it is to be borne by the women themselves; that local headmen wield more influence than their official position credits them with. And that he must so benefit should be clear to other government officials reading the book.

There is promise even in the defects of the work. Certainly had Dr Brown, a very competent observer, been left to his own devices to present an account of Hehe life with an emphasis upon modern changes, he would have made us more thoroughly familiar with the people, and incidentally supplied the data to answer most of the questions asked. But in this investigation, as will no doubt be the case in future ones, the very answers to his specific questions led the administrator to the asking of more general ones, and at least in the field of law convinced him that the inquiry would have been more fruitful had its scope been less limited. With growing efforts to make such applications, administrators will come to appreciate that the interrelatedness of the various aspects of a people's culture makes a fuller and more systematic knowledge practically useful. Toward this end such preliminary attempts as this are to be welcomed. For the anthropologist too it promises a broadening of his scope into fields where he has not always been free to tread.

MAY MANDELBAUM EDEL

BROOKLYN COLLEGE

The History and Coinage of the Sultans of Kilwa. JOHN WALKER. (Numismatic Chronicle, 5th series, Vol. 16, pp. 43-81, London, 1936.)

This notable article by Mr John Walker of the Coins and Medals Department of the British Museum is of importance for the history of the Swaheli.

The genealogies show the descent of these rulers from Shirazi (Persian) colonists from the Persian Gulf who settled in East Africa. They were fugitives and founded a dynasty which at the time of the arrival of the Portuguese ruled an extensive area of East Africa. The Kilwan empire was founded by Moslems in the tenth century and was destroyed by the Portuguese after their occupation of the coast. Another Shirazi colony was founded at Pate in the Lamu Archipelago and this dynasty survived in consequence of its inaccessibility until the nineteenth century.

The settlements at Kilwa and Pate were distinct and there is no evidence to show that Kilwa conquered Pate.

The colonists intermarried with native women or descendants of previous Asiatic settlers and the negroid strain became predominant. The records of both places are, as usual in such cases, thinly disguised accounts of murder, assassination, and intrigue for local rule, both before and after the Portuguese occupation.

It is believed that the descendants of these colonists from the Persian Gulf are the Swaheli of today. The architecture of their ruined and deserted mosques, tombs, etc., is of the medieval Persian type and many non-African customs survived until a recent period. The Kilwans exploited the gold trade of Sofala and their vessels traded with those of Pate to India before the arrival of the Portuguese.

Mr Walker's paper is the result of much historical research and although he does not deal with the anthropological problems, the information which he has published should be consulted by all interested in such matters.

The authorities cited by Mr Walker regarding the Kilwa are comprehensive and complete, and form a valuable bibliography for students.

ARTHUR E. ROBINSON

ST. ALBANS, HERTS, ENGLAND

Kamba Tales, II. Tales of Supernatural Beings and Adventures. GERHARD LINDBLOM. (Archives d'Études Orientales, Vol. 20, No. 2, iv, 142 pp. 12 cour. Lund: Berlingska Boktryckeriet, 1935.)

This volume is complementary to Part I in which Dr Lindblom presented a collection of Kamba tales of animals. In the initial volume the author gave a general survey of Kamba folklore and classified the stories on a psychological and ethnological basis.

The majority of tales in Part II relate to ogres, giants, and man-eaters, but a secondary group is aetiological or explanatory of origins. Dr Lindblom thinks that the tales afford little evidence of borrowing from other cultures, but mentions No. 32, "The Story of the Bird Lue," as a possible exception, since the tale may be of Mohammedan-Arabic origin. There has perhaps been some acquisition of Masai tales by the Kamba but this is uncertain.

With every translation the original text is given in the Machakos or other dialect according to the provenience of the story. Valuable linguistic and ethnographical notes occupy forty pages, and the only remaining task is a fuller psychological study of the latent content of the stories.

Such study might give further evidence respecting symbolism, especially of the various ogres. The notes (pp. 102-41) make some analysis of the reciprocal relation between ethnological facts and folklore, but further study might more fully reveal the reflection of cultural history in these Kamba tales. A further profitable line of investigation would be a comparative study of Kamba dreams and folklore, since both may be different means of expressing ideas and wishes that are usually suppressed. The volume is a valuable source of material for comparative study, the foundation of which is laid in the section entitled "Linguistical and Ethnographical Notes."

WILFRID DYSON HAMBLY

FIELD MUSEUM OF NATURAL HISTORY

Mangaian Society. TE RANGI HIROA (P. H. BUCK). (Bulletin, Bernice P. Bishop Museum, 122. 207 pp., map. Honolulu: Bishop Museum, 1934.)

The Legends of Maui and Tahaki. J. F. STIMSON. (Same series, 127. 100 pp. 1934.)

Outline of Hawaiian Physical Therapeutics. E. S. CRAIGHILL HANDY, MARY KAWENA PUKUI, AND KATHERINE LIVERMORE. (Same series, 126. 51 pp. 1934.)

Notes on the Ethnology of Pukapuka. GORDON MACGREGOR. (Occasional Papers, Bernice P. Bishop Museum, Vol. 11, No. 6. 52 pp., 4 pls., 15 figs. Honolulu: Bishop Museum, 1935.)

The first monograph treats of Mangaian social and political structure, religion, legendary origins, and history, and touches but lightly on material culture. Information from early writers, especially from W. W. Gill, is critically sifted and incorporated with Te Rangi Hiroa's own findings during his five month visit in 1929-1930. The paper is well documented with texts and other linguistic material and includes many valuable comparative observations based on Te Rangi Hiroa's extensive knowledge of Polynesia.

The author believes that distinctive features of the origin legends were deliberately fabricated to conceal the humble origins of the Mangaian in Raratonga. Apparently these people were less sticklers for accuracy than the Polynesians are commonly thought to be!

Fifty pages of history describe a series of intertribal struggles which invested warfare with importance unusual in Polynesia. Temporal power was acquired through victory rather than mere hereditary status. The temporal lord could even "depose the hereditary high priests of Rongo" (p. 161). Successful warriors received liberal grants of land and their leaders were awarded chiefly positions. After death, persons killed in battle went to a pleasant spirit world above, whereas those dying naturally went to extinction below. (Neither concept is of Christian origin.) Appar-

ently these material and spiritual rewards of warfare explain the surprising fact that relatives who, because of tribal exogamy were often members of opposing factions, quite unhesitatingly took one another's lives

Te Rangi Hiroa explains tribal exogamy as a probable crystallization of an ancient habit of acquiring wives from outside the tribe because of a shortage of women (p. 92). It is not obvious, however, why local women, being thus at a premium, should not have been married also. In view of the patrilineal land succession, patrilocal residence (pp. 96-97), and the Polynesian genealogical sense, it would seem that a relationship would have been recognized between all members of a small community or tribe and that the tabu on cousin marriage (p. 92) would have made tribal or local exogamy inevitable.

Land succession became matrilineal at times, however, owing to the alternate sharing of children by father's and mother's tribes and occasional adult adoption into the mother's group. It is remarkable that these factors and the reallocation of land after each war did not destroy the patrilineal, patrilocal, and exogamous features of the tribes

It is of interest that the gens-like features of the tribe did not affect kinship terms.

The second bulletin contains the text and English translation of the legends of Maui and Tahaki from the island of Fagatau in the Tuamotu Archipelago. It is well elucidated by ten pages of notes. Two pages carry an annotated bibliography to other polynesian Maui and Tahaki legends. There is also a short section on the "Music of the Tahaki Chants" by E. G. Burrows.

As ethnographic monographs rarely accord physical therapeutics the attention it warrants, the stimulating and excellent paper by Handy, Pukui, and Livermore should be gratefully received.

Approximately half is devoted to medical aspects of reproductive functions, physical therapy, pharmaceuticals, and the preparation and administration of remedies. The remainder includes a thirteen page glossary of native terms of pathology, anatomy, and physiology. Twelve more pages are devoted to a list of medicinal plants, animals, and minerals. As but few of these are mentioned in the descriptive portion of the paper, it is regrettable that their uses could not have been specified.

MacGregor's notes are based on a three days visit to the small island of Pukapuka, a survey of the literature and a study of specimens in the Bernice P. Bishop Museum. Descriptions of material objects are exact and useful. The remainder of the paper, however, seems to demonstrate that three days is not sufficient to gather material for a clear and convincing, to say nothing of complete, description of non-material culture. Much is confused, as, for example, the relationship of such land divisions as village "sections," "districts," and "pieces of land." Some statements are actually contradictory, such as that on page 17 that "the custom of tapu or avoidance between brothers and sisters . . . exists in Pukapuka" and on page 18 that "avoidance is not practised between true brother and sister."

JULIAN H. SEWARD

GENERAL

Tylor, R. R. MARETT. (220 pp., plate \$1.75. New York: John Wiley and Sons, 1936.)

The Doctrine of Survivals. MARGARET T. HODGEN. (192 pp. 5s. London: Allenson and Co., 1936.)

As anthropology comes of age it contemplates its infancy with mixed emotions. Here we find Marett approaching his preceptor, Tylor, with an attitude of hero-worship at the same time that Miss Hodgen ruthlessly examines one of his major concepts.

It is worth while to return occasionally to the nineteenth century and survey the problems which confronted the anthropologists of that time. Tylor tackled these problems with a vigor and clarity which enabled him to escape the full force of evolutionary criticism; these qualities likewise forestalled critical examination of his leading concepts.

The best introduction to Tylor is still his *Researches* and *Primitive Culture*—one of Marett's main objectives is to encourage a more careful study of these two works. Beyond that Marett's contribution is more in the nature of a commentary than a critique, since his approach precludes effective criticism. After a brief biographical sketch and an account of Tylor's initial inspiration through a chance visit to Mexico, Marett surveys the contents of *Researches* and *Primitive Culture* in a series of short chapters. In this process we get a great deal of Marett as well, a welcome addition in the chapters on religion, but confusing at times.

Miss Hodgen's essay is an expansion of her earlier article in this journal¹ dealing with the history of the concept of "survivals." Her thesis is that the legacy of the social sciences consists of concepts, hence the student of method must be a historian of ideas. Since "concepts are modified only in periods of criticism and doubt" (p. 10), she places the concept of "survivals" in such a frame of reference. Specifically the controversy between developmentalism and degeneration is presented as the critical period which led Tylor to the formulation of the concept in an attempt "to retain the idea of progress as an organizing principle in the study of man" (p. 34).

She shows that Tylor's formulation of the concept is vague, clothed in analogies and metaphors. While he synthesized the assumptions of his predecessors and contemporaries, he failed to clarify them or make them logically consistent. Yet so compulsive was the idea of "progress" that his generation of anthropologists turned *en masse* to "survivals" as the key to social evolution. A full generation passed before they began to inquire into what they were doing; Tylor survived the resulting wreckage largely because he had made so little use of his concept.

In a brilliant last chapter, Miss Hodgen asks: "What, then, is to be done with survivals, the developmental hypothesis, and the problem of social change?" (p. 176.) She sees no hope of retaining the doctrine, based as it is on the crumbling walls of developmentalism, particularly as the doctrine in its Tylorean form raises

¹ *The Doctrine of Survivals* (*American Anthropologist*, Vol. 33, pp. 307-24, 1931).

more problems than it attempts to solve. In accordance with her thesis she expects "new instruments of navigation" to develop out of this period of criticism and doubt. In support of her expectation the reviewer presents the trends of modern anthropology, particularly those in the fields of the analysis of culture and cultural change.

FRED EGGAN

UNIVERSITY OF CHICAGO

A Hundred Years of Anthropology. T. K. PENNIMAN. (400 pp. \$4.50. New York: Macmillan Co., 1936.)

Mr Penniman divides the history of anthropological thought into four periods. The first, or Formulary, period begins with Herodotus and ends in 1835. The second, the Convergent, begins in 1835 and ends with the appearance of *Origin of Species*. There seems to be little justification for the dividing line 1835 except that the book was to be called *A Hundred Years of Anthropology*. The Constructive period begins with Darwin's great work and ends in 1900, "when Mendel's earlier discoveries first became generally recognized." The Critical period takes us up to 1935.

In the reviewer's opinion, by far the best part of this history is that portion which deals with anthropological thought prior to 1880, or thereabout. In an interesting and effective manner, the author shows how various streams of thought, flowing from travel and from geological, paleontological, biological, sociological, and philosophic studies grew and gradually merged, forming the science of anthropology, the "master science" which synthesizes and integrates all of the scientific studies of Man. The account of modern anthropology, say from 1870 or 1880 on, while broad in scope and rich in facts, seems to be lacking in grasp of the basic problems and main currents of anthropological development. A great deal of this history consists of a recitation of names of men and their books, arranged under logical headings rather than a critical exposition of what different groups of anthropologists have been doing and are now trying to do. Although acknowledging the "tremendous importance" of "those who collected the evidence," the author has very little indeed to say of those who did the work, what they have done, and the methods by which they have accomplished it. His emphasis has been rather upon "the use made of the evidence." As a consequence, the work bears the stamp of the library; the flavor of the field worker is virtually absent.

It seems quite plain that Darwin is the real "hero" of Mr Penniman's story. It was Darwin "who provided the evolutionist view of nature which . . . integrated all these studies, and thus made possible the Science of Man as a unified subject" "With Darwin, the history of Anthropology as a single, though many-sided science, begins." And "for the next forty years, the great pioneers in the study of Anthropology in all its branches followed Darwinian principles." And today, anthropology is about to get "a fresh start from Darwin's principles." In the reviewer's judgment, the author has greatly exaggerated the role played by Darwin, especially in cultural anthropology, but this point cannot be argued here.

It is no doubt easy for one to overestimate the importance of scientists of one's own country and, correspondingly, to underestimate those of other lands. But the reviewer believes that many non-American anthropologists will feel, as he does, that Mr Penniman has failed, on the whole, to give American anthropology and anthropologists the attention and credit to which they are entitled. Morgan excepted, there are only passing references to American anthropologists prior to 1900. Franz Boas is disposed of in three sentences, plus a short passage written by him to characterize the work of another. Hrdlička gets three sentences; Goldenweiser, Dixon, and Gregory one each. Bandelier, Holmes, C. Thomas, and J. W. Fewkes get two sentences in all; N. C. Nelson shares one sentence with two others. There is mention of Sapir's *Time Perspective*, but not a word of his linguistic studies. Hooton, Kidder, and Cushing are listed in the bibliography at the end of the book but are not mentioned in the text. There is no mention whatsoever of Kroeber, Lowie, G. A. Dorsey, Goddard, Gatschet, MacCurdy, Mooney, Starr, A. F. Chamberlain, Tozzer, Spinden, Morley, Spier, Parsons, Cole, Speck, Michelson, Laufer, Linton, Putnam, Guthe, Strong, F. H. H. Roberts, Pilling, Todd, J. P. and M. R. Harrington, A. C. Parker, Swanton, Hodge, Gifford, Waterman, *et al.* Neither is there mention of the studies of primitive music carried on in America by Densmore, Helen H. Roberts, Herzog, and others; nor of the fruitful researches in ethnobotany, dendrochronology, and ceramic analysis.

A number of errors unfortunately mar this work: A. B. Lewis for A. B. Ellis (p. 240); Caspian for Capsian (p. 333); E. L. Hewitt is listed as author of *Orenda and a Definition of Religion* and also of *Ancient Life in the American South-West* (p. 379). J. N. B. Hewitt is the author of the first, E. L. Hewett author of the second.

The appendix contains "A chronological list of men and events in the history of anthropology" (which, incidentally, includes *Uncle Tom's Cabin* but not *The League of the Iroquois*, which Powell called "the first scientific account of an Indian tribe ever given to the world"), a list of "anthropological museums, societies, and periodicals in various countries of the world," and a twenty-one page bibliography.

It is not difficult, of course, to find fault with a work as comprehensive as this, which, after all, was written for the layman rather than the specialist. Encyclopedic knowledge, fine balance and judgment, and keen insight are all required to an exceptional degree. With the exception of Haddon's little book of a quarter of a century ago, this is the only history of anthropology that we have in English. Mr Penniman is to be congratulated for having undertaken so formidable a task. As for achievement, let him who can do better begin at once.

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Materials from the Archives of Lewis H. Morgan. I. VINNIKOV (ed.) (Academy of Sciences, USSR, Moscow, 1935.)

This volume contains thirty-four letters written by various persons to Morgan. There are five from Bachofen, four from Lorimer Fison, two short ones from Darwin, two from Maine, two from Powell, and one each from Henry Adams, Bandelier,

Herbert Spencer, W. P. Garrison, Hale, W. C. Bryant, and others. The letters appear in the language in which they were originally written (four of Bachofen's are in English) and also in Russian translation. The editor's notes on these letters, however, appear only in Russian.

A very interesting table, prepared by Morgan (for inclusion in *Ancient Society* as the reviewer believes) is published here for the first time. It consists of two parallel columns and is almost four pages long. In the left hand column are listed "Progress of Inventions and Discoveries" through the various stages of savagery and barbarism. In the right hand column are listed events in "The Growth of Institutions," showing the correlation of the social culture with the technological.

A list of Morgan's published and unpublished writings, including a brief description of some of his scientific journals, and two English reviews of *Ancient Society* (one by Tylor; both in Russian) are included. There is also a list of corrections to *Ancient Society* made by Morgan himself; they are, for the most part, typographical in character. There are several photostatic reproductions of letters, and two photographs: one of Morgan, the other of his residence in Rochester.

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Tales That Dead Men Tell. J. E. PEARCE (Anthropological Papers, University of Texas, Vol. 1, No. 1 [Bulletin, No. 3537]. 123 pp., 14 pls., chart. Austin, Texas: University of Texas, 1935.)

This small and unpretending book is a gem of penetrating and analytical philosophy presented in the guise of an archaeological treatise. It is pure gold in the sense that it is pure intelligence. The time-vision and the space-vision of Pearce are both of widest extent. Half a million years at least of human history, made known to us by archaeology, by history, and by kindred sciences, are drawn upon for concrete facts; and nearly all regions of our little earthball provide each one its quota of illuminating knowledge.

In short, Pearce shows us here how great a teacher the past can be for us if we are but generous and wise enough to learn the lesson. Too often we of the human kind have not been so; dogmatic faith divorced from factual bases has often led us astray; or a blind and irrational adherence to traditional forms which have lost their meaning and therefore their justification has sometimes produced a general cultural stagnation. In such cases the past's lessons have been perverted in such a way as to do far more harm than good. But, as Pearce makes very clear (especially on pages 84-98), it is in the freedom to select, to appraise, to analyse, and to adapt the offerings of the past that the true and fruitful value of archaeological and historical study lies. To enhance the perfection of these different, but allied, processes we need properly organized museums with appropriate collections, and, equally, we need libraries replete with the best interpretative and descriptive literature. Specimens and books together aid the student to understand the lessons of the past without becoming intellectually enslaved by its previous phases in their authoritarian aspect.

Pearce's message is this. We, willingly, or not, are the product of all that man has done or has been in the entire history of human life. If we can learn to draw upon our heritage without losing our intellectual freedom of choice and of adaptation, we can build for the future a society strongly rooted in a beneficial tradition. But the past must be our teacher, not, as too often hitherto, our master. Properly used, the past, working through the channels provided by archaeology and history, can show us beauty of many kinds, a wide range of technical knowledge, and innumerable social formulæ to be adapted or rejected as we deem best. These, and many other things, constitute the message which Dead Men would convey to us through the tongueless artifacts which they have left behind. It is for us to see to it that the ears of our minds be sufficiently acute to apprehend and to utilize their faint and subtle whisper.

PHILIP AINSWORTH MEANS

POMFRET, CONNECTICUT

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DISCUSSION AND CORRESPONDENCE

DIFFERENT TYPES OF CRANIAL DEFORMITY IN THE PUEBLO AREA

Physical anthropologists are agreed only on the technique for obtaining certain fundamental measurements. As long as an observer gives his basic measurements according to the established rules, he is free to analyze them with modern biometric methods, or not, as he sees fit.

The same cannot be said of descriptions of morphological characters. Although these are probably more important than measurements in racial analysis, they are more elusive and certainly more tedious. Nevertheless, it has long been customary to describe in some detail many of those characters that are not easily recorded by metrical means. This procedure is especially desirable in dealing with small numbers of specimens.

The recent report by Dr Gerhardt von Bonin¹ on the skeletal material from the Lowry area, southwestern Colorado, is disappointing chiefly for the reason that it neglects simple description for the more spectacular methods of the Pearsonian school. I wish to call attention to an observation that escaped Dr von Bonin and which leads to a more important conclusion than any of those which he derives by more elaborate means.

One of the most characteristic features of the Pueblo skull is its artificial deformity. Dr von Bonin represents the type of this deformity in his specimens with a few contour drawings and photographs, and dismisses the subject with the following statements:

Unfortunately, occipital flattening, present in almost all skulls, has played havoc with the shape of the brain case so that for purposes of comparison only the face can be used (p. 146).

Deformation in the Southwest was a comparatively simple and in all likelihood quite unintentional affair, due to no more than a hard cradleboard, to which the babies were firmly strapped and which exerted a pressure on the occiput, centered apparently in the obelionic region (p. 166).

To those familiar with cranial deformity in the Southwest the thought may occur that obelion is rather high to be the center of occipital flattening. As a matter of fact Dr von Bonin is not describing the ordinary type of deformation. Had he compared his specimens with those illustrated in the Pecos report² and also in Retzius' report on Nordenskiöld's material from the Mesa Verde,³ he would have noticed that a type of cranial deformity occurs in southwestern Colorado that is different from

¹ Paul S. Martin, *Lowry Ruin in Southwestern Colorado: Masonry of Lowry Ruin and of the Southwest*, by Lawrence Roys; *Skeletal Material from the Lowry Area*, by Gerhardt von Bonin (Anthropological Series, Field Museum of Natural History, Vol. 23, No. 1, 1936).

² E. A. Hooton, *The Indians of Pecos Pueblo, a Study of their Skeletal Remains* (New Haven, 1930).

³ G. Retzius, *Human Remains from the Cliff Dwellings of the Mesa Verde* (Stockholm, 1893).

that at Pecos. In the latter case the plane of flattening is essentially at right angles to the eye-ear (Frankfort) horizontal and truly may be called "occipital;" in the former, however, this plane is inclined at an angle of 50°-60° to the horizontal and has been named "lambdoid"⁴ (lambda, not obelion, is the center)

In all justice to Dr von Bonin, it may be said that this oversight is due in part to an almost universal disrespect for the feature in question. Many physical anthropologists seem to believe unconsciously that cranial deformity exists solely to thwart their studies, and hence when working with such material dismiss this feature summarily. That cranial deformity has important implications in the Southwest should become evident from the following considerations

I have in preparation at the present time a report on the skeletal remains from Chaco Canyon, New Mexico, and especially Pueblo Bonito. On the basis of this study I stated⁵ at a recent meeting of the American Association of Physical Anthropologists held in New Haven, that the lambdoid type of deformity prevails in the Chaco Canyon (in at least 90 percent of cases⁶) in contrast to Pecos where the occipital type prevails (90 percent⁷). Also I pointed out that from an examination of the collections in the United States National Museum (see *Catalogue of Crania*⁸ for localities), as well as from the literature, it appears that the lambdoid type of deformity is limited, except for odd cases, to the Chaco area and to southwestern Colorado. Roberts' collections from Zuñi Reservation, New Mexico, and Allantown, Arizona (both Pueblo I)⁹ show the same type of deformity and are here grouped with the Chaco.

From Dr von Bonin's illustrations it is manifest that the crania from the Lowry area of southwestern Colorado include five with typical lambdoid deformity. There is no information concerning the other eight. Obviously, this finding in the Lowry area, as far as reported, agrees with my earlier statement.

Now it appears that the Lowry ruin supplies cultural evidence that points to the neighborhood of Chaco Canyon. Martin concludes¹⁰ that

The presence at this site of Chacoan pottery, masonry, and massive walls can only be explained by postulating that certain cultural elements from the Little Colorado-Puerco focus moved northward, reaching the site under discussion some time in the eleventh century of our era.

⁴ Hooton, *op cit.*, p. 37.

⁵ See abstract No. 29 (American Journal of Physical Anthropology, Vol. 21, No. 2, Suppl., 1936).

⁶ A. Hraděka, *Catalogue of Human Crania in the United States National Museum Collections; Pueblos, Southern Utah Basket-Maker, Navaho* (Proceedings, United States National Museum Vol. 78, Article 2, 1931).

⁷ F. H. H. Roberts, Jr., *A Prehistoric Village on the Zuñi Reservation, New Mexico* (Explorations and Field-Work of the Smithsonian Institution in 1930, Publication 3111), pp. 177-86, *An Arizona Village of a Thousand Years Ago* (Explorations and Field-Work of the Smithsonian Institution in 1933, Publication 3235), pp. 41-43.

⁸ Martin, *op cit.*, pp. 204-205.

It should be apparent that the finding regarding cranial deformity fits in with this cultural evidence. However, it should not be assumed that the situation is entirely clarified: skeletal materials from southwestern Colorado are still scarce and the occipital type of deformity does occur there. Indeed, the geographical distribution and archaeological horizons of these types of deformation remain to be more fully defined for the whole Southwest.

It may be admitted, with Hooton,⁹ that the lambdoid type of deformity is difficult of explanation. Nevertheless, I am convinced that it is of an artificial nature. Unless some other explanation is forthcoming, the cultural implication seems to be that cradleboards differing in type or with different accessories were in use in these two parts of the Pueblo area. Since the few cradleboards that have survived in the regions concerned¹⁰ show few differences in the principles of construction, this assumption cannot be verified. However, the matter is seemingly of sufficient importance to warrant bringing it to the attention of cultural anthropologists.

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THE GEOGRAPHICAL DISTRIBUTION THEORY AND AUSTRALIAN SOCIAL CULTURE

In a review published in 1930 Professor Radcliffe-Brown takes me to task for interpreting certain aspects of Australian social culture in terms of the geographical distribution theory.¹ In general his objections seem to be concerned more with the method of approach because it is historical than with the conclusions expressed. He stresses the point that some of my sources of information are unreliable and inaccurate in details—which I do not question, since much of my evidence was taken from the early writers—yet it is important to note that he makes no claim that their correction would in any way alter the general results. In respect to some of the conclusions it is said that there is nothing very new. No claims were made that they were new, and he seems to have completely missed the point that the distributional approach had independently arrived at the same results attained from other points of view.

In my application of the theory in 1928 I was interested in determining the chronological relationship and the respective places of origin of the ideas of dividing

⁹ Hooton, *op cit.*, p. 37.

¹⁰ A. V. Kidder and S. J. Guernsey, *Archaeological Explorations in Northeastern Arizona* (Bulletin, Bureau of American Ethnology, 65, 1919), pp. 164-67; N. M. Judd, *The Excavation and Repair of Betatikin* (Proceedings, United States National Museum, Vol. 77, Article 5, 1930), pp. 65-66; S. J. Guernsey, *Explorations in Northeastern Arizona* (Papers, Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. 12, No. 1, 1931), pp. 105-106.

¹ A. R. Radcliffe-Brown, Review of D. S. Davidson, *The Chronological Aspects of Certain Australian Social Institutions as Inferred from Geographical Distribution* (Oceania, Vol. 1, No. 3, 1930).

the tribe into two, four, and eight social divisions. On the basis of the incomplete and even inaccurate data available to me at that time the essential conclusions reached from the distributional angle were that the eight sub-section system developed from the four section system, that both were indigenous to Australia, that the latter was an outgrowth of the moiety system, and that moieties had their origin outside of the continent. These conclusions are admitted by Professor Radcliffe-Brown even though he disapproves of the means by which I reached them. He states

... The development of the eight sub-section system out of the four-section system is indeed fairly obvious and hardly to be disputed, and we may quite well suppose that it had its origin somewhere within its present area of distribution. There also seems to be no harm in supposing that the system of four sections had its origin in Australia, since it is unknown outside the continent.

In respect to moieties he continues

... The view that the division into moieties came into Australia from Melanesia is, however, not by any means so acceptable. Dr. Davidson entirely neglects to consider the possibility of the functional correlation of the moiety system with the Australian-Dravidian type of kinship system. If we must have hypothetical reconstructions of history a much more plausible view would be that a single original form of social organization which still survives in South India, and of which a division into exogamous moieties is a frequent *though not invariable* part, spread in ancient times from southern Asia into Australia and also into Melanesia.²

Thus he proposes that a moiety system was carried several thousands of miles to Australia and Melanesia but condemns me for suggesting that it traveled over the last part of this long route. In view of his claim that all peoples are constantly experimenting with and changing their culture, I do not understand on what basis he can infer that the "survival" in *modern* India is the same as or even similar to the social structure of that region at that ancient time, undoubtedly many thousands of years ago, when the Australians left Asia. In addition he admits that moieties are not an invariable part of what he terms the Australian-Dravidian system, hence it is not inconsistent to suggest that the Australian tribes which lack moieties may have come without them before the idea of moieties arrived.

Professor Radcliffe-Brown also objects to my regarding as similar in social structure the marginal Narrinyeri, Kurnai, Yuin, Melville Islanders, etc., all of whom lack sections and moieties. Incidentally evidence more recently published by him and by Dr. Elkin³ permits us to include now the peripheral Bardi of Dampier Land, the Nanda of the western coast, and many tribes along the Australian Bight and inland therefrom. However, Professor Radcliffe-Brown claims that these tribes cannot be considered as a unit because "... we know that they have different forms of social organization."⁴ The system on Melville Island he describes as

an abnormal and quite exceptional modification of the same type of system that in the

² Italics are mine.

³ *Op. cit.*, p. 386.

⁴ A. R. Radcliffe-Brown, *The Social Organization of Australian Tribes* (Oceania, Vol. 1, 1930); A. P. Elkin, *The Social Organization of South Australian Tribes* (Oceania, Vol. 2, No. 1, 1931).

⁵ Review, *op. cit.*, p. 386.

Kariera tribe underlies the division into four sections, a type which is to be found in different varieties from Dravidian India to Fiji. . . . The Narrinyeri, on the other hand, have a system which is an aberrant form of the same type as that which in its full development and systematization leads [*sic*] to the formation of the eight sub-sections⁵

Thus it would appear at first glance that I erred in regarding them as basically similar and as representative of Australian social structure before moieties and sections were present. Obviously there are differences. However, my general statement that their social organizations represent a pre-moiety condition cannot be so much in error as claimed, for Professor Radcliffe-Brown later speaks of the same Melville Island and Narrinyeri systems as ". . . two very special varieties of the *general Australian type* of social organization."⁶ Such a basic historical relationship, distant or otherwise, is all that I meant to imply.

In respect to initiation ceremonies Professor Radcliffe-Brown has produced no facts to cause me to question the general conclusions in chronology I have drawn. Indeed he admits these conclusions are plausible. Should more complete evidence indicate different solutions I am more than willing to accept them.

Professor Radcliffe-Brown goes on to say further of the geographical distribution theory that ". . . as a method of analysis, if it is to be really valid, it must be capable of being applied to any and every aspect of culture."⁶ And I maintain that it is capable of being so applied, providing sufficient information is at hand. Unfortunately data are seldom as numerous as one wishes and too often are lacking for crucial areas. But such difficulties of application cannot be considered as valid condemnations of the theory itself, nor can the theory be regarded as worthless if satisfactory results cannot be attained in *every* instance as he insists. In respect to his own hypothesis that there is a ". . . correlation between the terminological classification of kindred or relatives with the social classification" he does not ask for invariable correlation but admits that ". . . an absolute, one-hundred-per-cent" consistency is not expected, but only a fairly close correlation ". . . in the majority of human societies."⁷ He further admits that as the result of testing in various parts of the world not more than ". . . a fairly satisfactory degree of verification has been attained" and insists that ". . . if some inconsistencies are found in certain societies this does not invalidate the hypothesis."⁷

Where then, I ask, is the basis for criticism of the geographical distribution theory which makes no more pretentious claims for one hundred percent perfection and which also has been applied in various parts of the world with a fairly satisfactory degree of verification. That many mistakes have been made because of incomplete and inaccurate information and in interpretation is admitted, but these can be rectified when we know more about the application of the method through testings with diverse types of traits under various conditions in different cultures. I can make no better reply to Professor Radcliffe-Brown than his own remark to

⁵ *Ibid.*, p. 386. The italics are mine.

⁶ *Ibid.*, p. 370.

⁷ A. R. Radcliffe-Brown, *Kinship Terminologies in California* (American Anthropologist, Vol. 37, 1935, pp. 530-35), p. 531.

Professor Kroeber, that if he

should ask me to prove my hypothesis, I would answer (1) that I am doing my best, and (2) that it is easier to disprove a false hypothesis than to prove a true one.⁸

It is interesting to note that Professor Radcliffe-Brown now maintains that . . . It would save a great deal of unnecessary misunderstanding if ethnology on the one hand (related as it is to archaeology and history) and social anthropology or comparative sociology on the other, were recognized for what they are, two different disciplines having different aims, different methods, and different interests in field investigations.⁹

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KULTURKREISE AND STATISTICS

I read with considerable interest Mr Kluckhohn's article on the *Kulturkreislehre* which appeared in this journal, April-June, 1936.¹ It is almost unique in its consistent description of ethnological theory in the language of the epistemologist and logician. It is encouraging to find an ethnologist who realizes that it is impossible to draw any kind of inference or put forth any kind of argument without employing certain premises or assumptions regarding the source material, that it is desirable to distinguish between the logical validity of an argument and the truth of its premises, and that any generalization must be based on a representative sample of the totality of cases. These are elementary axioms but too often completely ignored.

In reviewing contributions to the *Kulturkreislehre*, it seems to me rather strange that Mr Kluckhohn did not mention a number of statistical studies, most of which have appeared recently. These I have listed below. Not that these papers are indispensable to an understanding of such theory, but that they express a part of the method in a much more explicit form than the verbal efforts which he cites. These studies differ in a number of ways from the usual thing. They define their territorial and cultural universes. They list specifically the tribes or local groups, the culture elements, and the presence or absence of the elements among the local groups. They weight such data by treating each presence or absence as an equivalent unit. The correlations which follow yield a type of distributional summary which logically follows from the assumptions regarding the data. This is a relatively direct form of induction. Whether the selection and weighting of the tribes and traits, the correlation formula, or the inferences drawn from the statistics are acceptable to the reader is another matter. The author of such a study has at least committed himself and is open to criticism or confirmation. While Mr Kluckhohn seems to fear the "atomization" of culture necessary for such treatment (footnote 32), his frequent references to correlation and probability are completely meaningless without it.

In the language of the contemporary empiricist, a *Kulturkreis* is simply a group

⁸ *Ibid.*, p. 532.

⁹ *Ibid.*, p. 535.

¹ Clyde Kluckhohn, *Some Reflections on the Method and Theory of the Kulturkreislehre* (American Anthropologist, Vol. 38, 1936, pp. 157-96).

of traits which correlate relatively high amongst themselves and lower with other traits, a culture area a group of tribes which correlate relatively high amongst themselves and lower with other tribes. Boundary lines can be determined from natural breaks in the intercorrelation table or can be arbitrarily drawn at certain limits such as $Q_6 = .50$. "Centers" or "climaxes" are simply groups of tribes or traits within culture areas and *Kulturkreise* which show the highest correlations with each other as well as the other tribes or traits in the area or *Kreise*. The relation between these two types of distributional summary, which Mr Kluckhohn suggests in footnote 52 might be worth knowing, is shown by Klimek in his work on California Indians.

While it may be true that certain correlations which existed in the past have left no record in historical sources or contemporary ethnographical data, the obvious procedure is to start with demonstrable correlations and to end with hypothetical ones. Whatever the error inherent in historical reconstruction, it is obvious that this will only be multiplied by the half-hearted distributionalism which is still largely the rule. A mere recitation of what one thinks ought to be the salient features of a few intuitively determined *Kulturkreise* or culture areas is no proof whatsoever that such units are natural ones and have or have had any real existence. The most satisfactory proof so far devised is the correlation technique, examples of which include the following.

1911 Jan Czekanowski, *Objektive Kriterien in der Ethnologie* (Korrespondenzblatt der deutschen Gesellschaft für Anthropologie, Ethnologie und Urgeschichte, Vol. 42, pp. 71-75).

1934 Christoph von Furer-Haimendorf, *Völker- und Kulturgruppen im westlichen Hinterindien, dargestellt mit Hilfe des statistischen Verfahrens* (Anthropos, Vol. 29, pp. 421-40).

1934 S. Klimek and W. Milke, *An Analysis of the Material Culture of the Tupi Peoples* (American Anthropologist, Vol. 37, pp. 71-91).

1935 Helmut Petri, *Typenanalyse der Geldformen Ozeaniens* (Archiv für Anthropologie, Vol. 23, pp. 309-17).

1935 S. Klimek, *Culture Element Distributions I. The Structure of California Indian Culture* (University of California Publications in American Archaeology and Ethnology, Vol. 37, pp. 1-70).

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PHYSIOLOGICAL PATERNITY IN AUSTRALIA

Ever since the publication in 1899 of Spencer and Gillen's *The Native Tribes of Central Australia*, the work in which it was first declared that there were aboriginal tribes in existence in Australia who possessed no knowledge whatsoever of the relationship between intercourse and pregnancy,¹ there has been a great deal of discussion concerning the nature of this ignorance and not a little controversy. The ground had long been prepared upon which such a discovery could fall with fruitful

¹ B. Spencer and F. J. Gillen, *The Native Tribes of Central Australia* (London, 1899) pp. 122-25.

advantage by the writings of the evolutionists, whose views were given a syncretic summarization in this connection by Hartland in 1894 in the first volume of his *The Legend of Perseus*, in which the suggestion was put forward that "the world-wide-story-incident of Supernatural Birth" probably originated in a period when the physical relationship between father and child was imperfectly understood, or rather, not understood at all.² It remained for Spencer and Gillen to provide the proof that tribes exhibiting such a nescience of the facts of procreation actually existed in central Australia.

In 1903 W. E. Roth published the first independent confirmation of the existence of a similar nescience among the North Queensland tribes.³

In 1904 Spencer and Gillen published their observations on the northern tribes of central Australia among whom they found to exist a precisely similar nescience of the facts of procreation as they had first reported for the Central tribes.⁴

In September 1905 J. G. Frazer published his now famous development of Spencer and Gillen's findings in this connection in the form of his theory of conceptional totemism.⁵ Not quite two months later Andrew Lang launched his attack upon both this theory and the findings of Spencer and Gillen.⁶

And from that day to this there has been an unceasing stream of writing devoted to this subject, reports of field-workers, discussions original and otherwise, and a great deal of expressing of doubts. I have calculated that during the last thirty years an average of about ten papers and books *per annum*, largely devoted to the discussion of what still remains the problem of the procreative beliefs of the Australian aborigines, have regularly made their appearance, not to mention the numerous references to the subject in the more general sociological literature.

In recent years the subject has received a definite fillip as a result of the publication of Professor Malinowski's researches among the Trobriand Islanders.⁷

To read all this published material is enough to turn one's hair gray, but if one is able to struggle through to the end what transpires is this:

By far the largest number of field-workers assert that the Australians are ignorant of the relationship between intercourse and childbirth.

Some field-workers state that whilst the tribes investigated by them have held the "mystical" beliefs of the Arunta type, yet they have also known that coitus is necessary before childbirth can occur.

² E. S. Hartland, *The Legend of Perseus* (London, 1894), Vol. 1.

³ W. E. Roth, *Superstition, Magic, and Medicine North Queensland Ethnography* (Bulletin, Home Secretary's Department, No. 5, Brisbane, 1903).

⁴ B. Spencer and F. J. Gillen, *The Northern Tribes of Central Australia* (London, 1904), pp. 281-332.

⁵ J. G. Frazer, *Beginnings of Religion and Totemism among the Australian Aborigines*, Pt. II (Fortnightly Review, Vol. 84, 1905, pp. 452-66).

⁶ A. Lang, *The Secret of the Totem* (London, 1905).

⁷ B. Malinowski, *Buloma. The Spirits of the Dead in the Trobriand Islands* (Journal, Royal Anthropological Institute, Vol. 46, 1916, pp. 353-430), *Fatherhood in Primitive Psychology* (London, 1927); *The Sexual Life of Savages* (London, 1929).

The discussers of the evidence are of two kinds, critical and uncritical. One group of critical discussers assert that the Australians may be ignorant of the relationship between intercourse and pregnancy to-day, though even this is doubtful, but granting that it is so, it seems highly probable that they were not always so ignorant, and that their present ignorance is due to the repression or gradual obfuscation of knowledge which became incompatible with later developed spiritual beliefs; psychology, as Lang put it, obscuring physiology.

Another group of critical discussers assert that there is no reason to believe that the present nescience was ever preceded by a knowledge of the facts.

The uncritical discussers simply "naively" accept the statements of the early investigators that the nescience exists and is complete.

It is unfortunately quite impossible within the scope of this article to attempt to consider the merits either of the field reports or the discussions of them, but since in the pages of this journal⁸ Mr Donald Thomson has recently joined forces with those discussers who believe that the ignorance of physiological paternity is not primitive wherever found to-day, but is a condition resulting from the superimposition of certain animistic beliefs upon an older dogma, and since, as Mr Thomson says, the matter is of interest and importance, I propose a discussion of his paper with a view to indicating something of the difficulties with which the ethnological mind has to contend when confronted with a matter ostensibly so simple as the determination of whether or not a particular people makes a connection between intercourse and childbirth.

In an extremely interesting paper on the Koko Ya'o and neighboring tribes on the east coast of Cape York Peninsula⁹ Mr Thomson stated that these tribes possessed a knowledge of physiological paternity, the child being regarded as the product of the seminal fluid of the father, whereas the mother was regarded as "nothing."

Mr Thomson appears to have been under the impression that such findings were unique for any Australian tribe, but as long ago as 1893 B. H. Purcell reporting on the Workii, a tribe who live not far distant to the northwest of the Cape York peoples, make known the existence of a very similar belief in the virtues of seminal fluid. Thus he writes in connection with the Bora ceremony:

After the third initiation into this remarkable ceremony the youth is made to drink semen that is taken from six or as many young clean gins and blacks as may be in the camp at the Bora ground. No gins are admitted to the ceremony other than these. When an old man is dying, they do exactly the same. They hold that as semen brought them into the world, it should keep them alive and from dying; and when a man dies they think that the semen germinates and even comes through the earth again and appears in the form of a white man or something else, often a star.¹⁰

⁸ D. F. Thomson, *Fatherhood in the Wik-Monkan Tribe* (American Anthropologist, Vol. 38, 1936, pp. 374-93).

⁹ D. F. Thomson, *The Hero Cult, Initiation and Totemism on Cape York* (Journal, Royal Anthropological Institute, Vol. 63, 1933, pp. 453-537).

¹⁰ B. H. Purcell, *Rites and Customs of Australian Aborigines* (Verhandlungen, Berliner Gesellschaft für Anthropologie, Vol. 25, 1893, pp. 286-89).

Writing in 1931 Professor W. Lloyd Warner gives an account of his experiences among the tribes of northern Arnhem Land, North Australia. During his first nine months of work among them he "could find no indication of any knowledge whatsoever about physiological conception," yet "there were strong indications that there was an understanding of the true nature of the physical function of the father."

The second time [writes Professor Warner] I entered the area I determined to go into this matter further, since the people I studied were but a continuation of the central tribes on which Spencer and Gillen reported. An occasion arose in which I could inquire directly of certain old men just what the semen did when it entered the uterus of a woman. They all looked at me with much contempt for being so ignorant and informed me that "that was what made babies."¹¹

Thus when Mr Thomson published his first paper there were already in existence at least two separate and independent accounts for the general region reported upon by Mr Thomson of tribes possessing a knowledge of the role played by the seminal fluid in producing conception.

Mr Thomson has taken great pains to show, and he has done so very successfully, the considerable effects produced by Papuan cultural influences upon Cape York culture. But what neither he nor Professor Warner have taken any pains to point out is that for the last thirty years this region has also been steadily infiltrated by appreciable white influences. Apart from the possibility that the tribes in this general region may have acquired their "knowledge" of the virtues of seminal fluid from Papuan sources, for which the evidence is exceedingly strong, we have also to reckon with the possibility that these tribes may have acquired this knowledge, in part at least, from white sources. In this connection W. E. H. Stanner, reporting on the Daly River tribes of North Australia, has some illuminating remarks. He writes:

It is clear . . . that two theories of sex exist side by side (a) a mystical theory of the type commonly found in Australian cultures, and (b) a barely understood, confused version of orthodox theory learned from whites. The emphasis in belief ranges from tribes like the Mulluk Mulluk and Madngella, which have completely forgotten their own mystical theory (which undoubtedly existed) to bush tribes with only the most imperfect knowledge of white beliefs. In tribes (like the Marthiel) where the beliefs co-exist in some definite form, the framing of the question governs the answer one receives . . . In the pure native theory the sexual act seems to have mostly an erotic significance, but in the altered belief it is considered to be in some way concerned with pregnancy. How or why is not known.¹²

Mr Lauriston Sharp reporting on the Yir-Yiront, who live along the western coast of Cape York Peninsula in the vicinity of the mouth of the Coleman River, states that according to native belief "children are sent out from spirit centres only when people copulate, but it is not the intercourse, but rather the immigration of the 'spirit child' which causes a pregnancy."¹³

¹¹ W. Lloyd Warner, *Birth Control in Primitive Society* (Birth Control Review, Vol. 4, 1931, pp. 105-107).

¹² W. E. H. Stanner, *The Daly River Tribes, a Report of Field Work in North Australia* (Oceania, Vol. 4, 1933, pp. 10-29), pp. 27-28.

¹³ L. Sharp, *Ritual Life and Economics of the Yir-Yiront of Cape York Peninsula* (Oceania, Vol. 5, 1934, pp. 19-42), pp. 23-24.

Miss Ursula McConnel, reporting on the Koko-yalunyu of the Bloomfield River district, North Queensland, writes that "The Koko-yalunyu quite definitely consider sex-contact to be necessary to child-bearing. They frankly admit, however, that they do not understand *in what way* it is necessary."¹⁴ An eponymous ancestress "sends all the babies to their mothers and so is the mystic cause in individual cases."¹⁵

It is apparent that, among these peoples, intercourse is recognized to have some connection with pregnancy, but precisely what connection remains obscure to them, and would not, it seems, be of much concern to the native. Intercourse is vaguely regarded as a condition which makes the entry of a spirit child into a woman possible, but clearly the spirit child is not regarded as deriving either from the father or the mother to whom it is born, but is considered to originate from an ancestral source. Incidentally, it may be mentioned that in the region of which Miss McConnel writes white settlers have been permanently established for at least sixty years. Yet writing in 1893 of the tribes in this region W. E. Roth was able to find no evidences of a belief in any sort of a connection between intercourse and childbirth. It is, of course, nonetheless possible that the belief existed and escaped Roth's attention, for there is good reason to believe that the belief in a connection between intercourse and childbirth is universally present among the tribes of Australia, apart altogether from any question of contamination by white influences or not.

It has generally been overlooked that Spencer and Gillen stated that such a connection was recognized among the Central tribes investigated by them; here are their own words:

... We have amongst the Arunta, Luritcha, and Ilpirra tribes, and probably also amongst others such as the Warramunga, the idea firmly held that the child is not the direct result of intercourse, that it may come without this, which merely, as it were, prepares the mother for the reception and birth also of an already formed spirit child who inhabits one of the local totem centres. Time after time we have questioned them on this point, and always received the reply that the child was not the direct result of intercourse.¹⁶

If these words mean anything then they mean that it is the general belief among Arunta and neighboring Central tribes that intercourse is necessary before a spirit child will enter a woman, although it is considered that in certain cases it is possible for a woman to be entered by a spirit child without the preliminary preparation of intercourse.

Strehlow¹⁷ and recently Fry¹⁸ have independently confirmed Spencer and Gillen's

¹⁴ U. McConnel, *A Moor Legend from the Bloomfield River, North Queensland* (Oceania, Vol. 2, 1931, pp. 9-25).

¹⁵ *Ibid.*

¹⁶ B. Spencer and F. J. Gillen, *The Native Tribes of Central Australia*, pp. 137-39; *The Arunta* (London, 1927), p. 22.

¹⁷ C. Strehlow, *Die Aranda- und Luritja-Stämme in Zentral-Australien* (Frankfurt, 1907-1921), Part 3, pp. x-xiii.

¹⁸ H. K. Fry, *Body and Soul, a Study from Western Central Australia* (Oceania, Vol. 3, 1933, pp. 247-56).

findings in this connection among the Arunta and Loritja as well as among neighboring tribes.

It will be recalled that Malinowski found a recognition of a precisely similar connection between intercourse and the entry of a spirit child into the woman among the Trobriand Islanders, who were shown by Malinowski to have no knowledge of the relationship between intercourse and conception.¹⁹ Among the Trobrianders as among the Australians, intercourse is regarded merely as a preparer of the woman for the reception of a spirit child, but it is not in any way regarded as the cause of conception.

It may well be asked whether this recognition of intercourse as a preparer of the woman does not constitute the strongest evidence that the animistic beliefs have simply pushed the knowledge of the bare physiological facts out of the way? The world of the Australian is a spiritual world, and material acts are invested with a spiritual significance. Human beings have a long spiritual history behind them, and the spiritual source of every member of the tribe is known. The spiritual origin of children is the fundamental belief, and amongst the most important stays of the social fabric. It were absurd then to think, in the face of such knowledge, that an act such as intercourse could be the cause of a child. Yet in a vague sort of way the Australian knows that without intercourse there will not, as a rule, be a child, but at the same time he knows that intercourse is the least important factor in the whole cycle of events. Intercourse is a condition, and a dispensable condition, but is never the cause of conception. Such facts, I suggest, render unnecessary any assumption that the animistic beliefs have been responsible for the repression or elimination of a pre-existing knowledge of the facts. What such beliefs do secure is a state of mind, of affective-imaginative associations, which assist to make the establishment of a significant relationship between intercourse and conception virtually impossible, and there is no good reason to believe that this state of affairs was ever otherwise, that intercourse was ever recognized to play a more significant role in the production of conception than it does today. In short, though a great deal of energy and ingenuity has been expended in arguing the point, and by no means unattractively, that such beliefs as the Central Australians are alleged to hold in connection with the process of procreation are not primitive, but are the result of their spiritual philosophy which has caused the knowledge of the relationship between intercourse and childbirth to become gradually repressed, the connection cropping out of the tufa of consciousness in the demoted form of a purely ancillary act in the service of a spiritual function, I do not think that anyone has thus far succeeded in making out a case beyond what is purely speculative. As I have already indicated, I do not consider that there is a single factor of whatsoever nature in any Australian culture which upon examination would lead one to believe that the Australians ever knew more than they do today of the connection between intercourse and childbirth: the probability is that they knew much less.

¹⁹ B. Malinowski, *The Sexual Life of Savages* (London, 1929)

Mr Thomson writes that

The Wik Monkan recognizes, and freely affirms, the fertilizing influence of seminal fluid (tankarrä), but on the physiological aspect of conception and pregnancy, his knowledge is less exact. He recognizes that pregnancy results from the introduction of seminal fluid, but as to how the embryo is produced, his ideas are as vague as those of any white man who possesses no biological knowledge. His belief is that the seminal fluid enters the uterus (pö'ö mompa) and gradually builds up the body of the embryo, and thus he insists that a single sexual act is not sufficient to produce conception, which can result only from repeated intercourse.²⁰

Mr Thomson's facts are not in question, but the role which the seminal fluid is believed to play in building up the body of the embryo reads suspiciously like the New Guinea notion of these things. Similarly the belief that it is necessary for the father to lie repeatedly with the mother that the body may be built up during the pre-natal period is a widespread New Guinea belief, occurring also over a wide area of Melanesia.

Mr Thomson describes the Wik Monkan notions of conception in some detail (p. 377), and these indicate that this tribe is quite well informed concerning the mechanism of conception and the changes which follow it.

It is to be noted, however, that the Wik Monkan notion of the relationship between intercourse and conception is really quite shaky, for they are quite unaware of the fact that a single act of coitus is sufficient to produce conception: they insist on the necessity of repeated acts before conception can be produced. It is, of course, quite possible that they have acquired this notion empirically, though it seems difficult to believe this. What would seem to be nearer the truth is that at one time the procreative beliefs of the peoples of this region were of the same general pattern as those encountered throughout the rest of Australia, and that the present beliefs of the Wik Monkan and neighboring tribes have been acquired lock, stock, and barrel from their Papuan neighbours, with a possible recent addition from white sources. Whereas Mr Thomson has fully emphasized the considerable influence of Papuan hero cults "that had modified considerably the indigenous culture" (p. 374) of the eastern tribes of Cape York Peninsula, he has somehow failed to take into consideration the possibility that the Cape York procreative beliefs, which constitute so striking an aberration from the general Australian pattern, may be attributed to the same modifying influences of the indigenous culture as have affected practically its every aspect.

It seems to me clear that if we admit, as I believe we must, that everywhere else in Australia the fertilizing influence of seminal fluid is not in any way understood, then it follows that wheresoever in Australia there does exist such a knowledge this is probably of extra-Australian origin.

Thus whilst we may accept Mr Thomson's statement that physiological paternity is recognized among the Wik Monkan and neighboring tribes of Cape York Peninsula, it must at the same time also be recognized that the conditions in this

²⁰ Thomson, *Fatherhood in the Wik Monkan Tribe*, p. 375

part of the continent are unique, and that they cannot in any way be generalized for the rest of Australia.

Mr Thomson writes:

On account of the controversial nature of the subject, and the fact that the knowledge of physiological paternity is in conflict with the findings of previous workers both to the north and south of this region, considerable care was exercised in the collection and checking of information and in the testing of informants (p. 391).

But there is here no conflict. Surely each tribe may be allowed to enjoy its own particular version of the nature of things without its being described by an intruding ethnologist as in conflict with the beliefs of some particular tribe whom he has arbitrarily elected as the standard of reference? I do not see where any question of conflict arises here. Had a number of independent workers reporting on the same horde of the same tribe given conflicting versions of the conditions found by them the matter were different, but such a case has not arisen, and it is therefore difficult to understand Mr Thomson's remarks in this connection.

Mr Thomson feels "that the ignorance of physical paternity was taken for granted by many of the early workers who had come under the influence of 'group marriage' beliefs" (p. 392).

But the truth is that the most highly trained and intelligent workers of the last decade and more, who have been completely divorced from anything like "group marriage" beliefs, have also found this nescience to prevail among the tribes investigated by them. In the exceptional cases where there has been some doubt it could be demonstrated (I am not referring to the Cape York and neighboring tribes) that mission or other white influences had been at work.

The fancied incompatibility which Mr Thomson sees between his own findings in North Australia and Malinowski's conviction—which he thinks, mistakenly, to be based upon the latter's Trobriand experiences—that ignorance of physiological paternity is an original feature of primitive psychology, does not exist.

The Trobrianders, Mr Thomson would appear to argue, are clearly a culturally more advanced people than the Cape York peoples, yet among the one it is stated that a knowledge of physiological paternity does not exist, whereas in the other this knowledge demonstrably exists. Hence it is somewhat strange that since the less advanced people possess the knowledge that the more advanced people should not possess it. It would seem, therefore, runs Mr Thomson's argument, that the nescience cannot be primitive. Mr Thomson writes in his final paragraph:

The existence of this knowledge of physical paternity, which has a fundamental character, in this region lying midway between the areas of Central Australia and Papua, where such beliefs are said to be absent, presents some difficulties. The conclusion that seems to be inevitable is that the ignorance of physical paternity is not primitive but that, as Professor Carveth Read suggested,²¹ where it does exist at the present time among primitive peoples, true knowledge has been masked by animistic beliefs, superimposed upon the primitive condition. I consider that the evidence now presented from the Wijk Monkan tribe is conclusive, and that

²¹ *No Paternity* (Journal, Royal Anthropological Institute, Vol. 48, 1918, pp. 146-54).

if such knowledge is absent from more primitive people to the south, and from the more advanced peoples to the north, the facts admit of no other conclusion (p. 393).

Apart from the numerous objections which such an argument at once raises, the simple fact alone, that there is every reason to believe that the Cape York peoples obtained their knowledge of the nature of procreation from an extraneous cultural source already in possession of it, is alone sufficient to explain how it may come about that a culturally less advanced people may be in the possession of knowledge not in the equipment of an otherwise culturally more advanced group.

It is hardly correct to state that in Papua a knowledge of physiological paternity, or the connection between intercourse and conception, is "said to be absent." As far as I am aware no one has thus far reported such a state of affairs among any Papuan people: on the contrary, such reports as we have of them show that their knowledge of the nature of procreation is practically identical with such a knowledge as Mr Thomson has reported for the Wik Monkan. Mr Thomson's argument that since the Cape York peoples are, as it were, in a geographic position intermediate between the Central Australians and the Papuans, it is more than likely that the Cape York peoples present the truly fundamental primitive conditions of knowledge on the subject of procreation, and that among the peoples to the north and south of them there has been a masking of the original fundamental knowledge, therefore defeats itself.

I submit that the facts lead to quite an opposite conclusion to that arrived at by Mr Thomson.

M. F. ASHLEY-MONTAGU

NEW YORK UNIVERSITY
NEW YORK CITY

ARCHAEOLOGICAL OPPORTUNITIES IN JAPAN

To the Editor:

None of the Japanese archaeologists I have talked with here has recently seen an American interested in that subject. Yet, as I have noted the great number of students that come to this country each summer, it has been impossible to believe that archaeologists will not come, if, indeed, they are not already coming. Perhaps some of them will have only limited time and will find it advantageous to land with at least a rough program. Here are a few suggestions from my own experience:

There are seven "Anthropological Laboratories."

(at) Tokyo Imperial University, Tokyo.

Ohyama Institut fur Praehistorie, 9 Onden, Aoyama, Tokyo.

Torii Anthropological Research Institute, 21 Kasumi-cho, Azabu, Tokyo.

(at) Kokugakuin, Tokyo.

Nishioka Anthropological Laboratory, Denenchofu, near Tokyo.

(at) Kyoto Imperial University, Kyoto.

Jodai Bunka Kenkyojo, Kyoto.

(Any hotel clerk could get one to these places with the addresses given.)

The Imperial Universities at Tokyo and Kyoto both have archaeological museums. At Kyoto, three of the staff are particularly eminent among Japanese anthropologists. Dr Kenji Kiyono as a physical anthropologist, Dr Kosaku Hamada and his assistant Mr Umehara as archaeologists.

Prince Kashiwa Oyama, who maintains the Ohyama Institut, is a specialist in Jomon culture. He has a splendid lot of archaeological material collected by himself and his own men from over 120 shell mounds. It is classified and accessible to the student with even limited time. Prince Oyama has had European training in prehistory, speaks German fluently, and publishes in that language as well as in Japanese. He also speaks English well enough for ordinary conversational purposes.

The material collected by Dr Ryuzo Torii has been distributed to various institutions. Dr Torii himself has a large library and extensive field experience. Moreover, this field experience has been largely in the Kuriles and in Asia proper. His son and daughter are also anthropologists. He does not speak English, tends towards the older Japanese scholarship, and would probably be of more assistance to the ethnologist than to the archaeologist with an American background.

Mr Hideo Nishioka is still a student in Keio University, but he has done considerable field work, has a collection of material, and is excellently informed on his subject. On account of his intelligence, his youth, his enthusiasm, and his command of English, the visiting American archaeologist would possibly get more from him in a limited time than in any other way.

In addition to the collections above mentioned, there is a very interesting archaeological collection in the Agricultural Museum, Motoyama Room, near Osaka. This museum is the property of the owner of the Osaka Mainichi Shimbun (a newspaper). Also, as in our country, there are many small collections, but these are not likely to engage the man with limited time.

There is no prehistoric material discernible in Japan that is earlier than Neolithic. Pottery is not only present in the earliest sites, but is elaborate. Two cultures are recognized: the Yayoi and the Jomon. There is apparently ample evidence to identify and characterize each and to show by stratification that Jomon is the older.

Yayoi culture extends into the historic period in Japan, say to around 600 A.D. It extends from the south to about the latitude of Fukushima (roughly 38° N.) It would appear to represent the influences that came from the south. For this reason it is of less interest than Jomon to one preoccupied with American prehistory.

The Yayoi culture takes its name from a site in Hongo, Tokyo. Its main index is its pottery and the fact that it does not occur in shell-mounds. Pottery tends towards pot-bellied vessels, reddish in color, and not showing the Jomon marking. Prince Oyama recognizes four periods:

4th Historical. Buddhist influence shown.

3rd Pottery without decoration. Bronze in Kyushu and some iron.

2nd Pottery with some decoration and some color.

1st Oldest. Pottery more decorated than any other. Yayoi decoration mostly in the form of incised lines.

Jomon culture apparently came from the north, and spread all over Japan to (including) the northern edge of Kyushu. Bronze and iron are unknown. Vessels of pottery are of very characteristic and rather elaborate shapes. Pottery is thick and bears the basketry-like mark which gives the culture its name. (The character pronounced according to the Chinese manner "jo" = "nawa" in Japanese = cord: "mon" = crest).

Several sub-types are recognized in Jomon culture. The time of a given sub-type depends on the place. The two northern groups are most likely to interest an American.

Hokkaido or Northeastern Japan Jomon consists of two sub-types:

I. *Ichioji*. The oldest. Characterized by cylindrical or "high-hat" pots, and scrapers with an extremely characteristic protuberance.

II. *Kamegaoka*. Pottery smaller and more highly decorated. Much bone work, including detachable harpoons. Many small excellent arrow points in various materials. (These points are very reminiscent of those of the Columbia River region.)

Kwanto (Tokyo region) Jomon is divided into three sub-types by Prince Oyama:

I. *Morioso* (named for the type site north of and near Tokyo). Oldest. Appears in shell mounds about 16 meters above present sea-level. Pottery cylindrical and not plentiful. Carved bone work. Shell ornaments. Almost no points.

II. *Katsusaka* (named for the type site some fifty miles southwest of Tokyo). Appears in shell mounds 11-12 meters above present sea-level. Pottery very thick; elaborate shapes; plentiful; some red-painted. Few points.

III. *Omori* (named for the type site within the city limits of Greater Tokyo). Appears in shell mounds 7-8 meters above present sea-level. Pottery thinner, smaller, delicate shapes. Bone work elaborate, including harpoons. Sea-otter teeth ornaments. Stone clubs. Clay figures and clay ear plugs (This type is the most suggestive of the California shell mounds.)

The theories that have been deduced from this material have not been very fully set down even in Japanese. This is especially true of the period likely to be most enlightening—the last decade or so. However, the theories that have been advanced, the men responsible for them, and the publications in which they have appeared would apparently be necessary starting points for further research. Accordingly I have taken an article by Mr Nishioka which summarizes the theories and have somewhat condensed it with the above purpose in view.¹ It will suggest what help cannot be expected, as well as the data which may be got, by anyone going to Japanese written sources.

W. EGBERT SCHENCK

TOKYO, JAPAN

¹ See pp 23-33 above.

NOTES AND NEWS

SOCIAL SCIENCE PRE-DOCTORAL FELLOWSHIPS FOR GRADUATE STUDY

The Social Science Research Council announces the availability of pre-doctoral fellowships for graduate study. These fellowships provide one thousand dollars, tuition charges, and traveling expense for a round-trip between the student's home and the institution of learning selected. The fellowships are designed to aid exceptionally promising students of the social sciences to obtain research training through pre-doctoral graduate study. Fellows are required to devote their full time to graduate study in some institution other than that in which they received their undergraduate training.

The fellowships will be offered again for the academic year 1937-38. The closing date for the receipt of applications on blanks to be secured from the Fellowship Secretary is March 15, 1937. Inquiries should be addressed to the Social Science Research Council, 230 Park Avenue, New York City. Each candidate must submit a letter from the chairman of the department in which he has pursued his major undergraduate study, in support of his application, before blanks will be sent to him.

THE FIRST PUBLICATION OF THE FREDERICK WEBB HODGE ANNIVERSARY PUBLICATION FUND

The History of Hawikuh, New Mexico, by Dr F. W. Hodge, is to be the first publication of the Fund which bears his name, a fund being raised by voluntary contributions to commemorate his fifty years (1886-1936) in American anthropology. The purpose of the Fund is to publish works in the American field. Through the generosity of Dr George G. Heye and the Trustees of the Museum of the American Indian, Heye Foundation, the initial publication will appear at no cost to the Fund. Dr Hodge was on the staff of the Museum of the American Indian for thirteen years, leaving it in 1932 to become Director of Southwest Museum.

Contributors to the Hodge Fund will receive a copy of *The History of Hawikuh, New Mexico*, or a credit toward its purchase, on the basis of one per centum of discount for each dollar contributed. This rate of discount will apply until the amount of the contribution is exhausted, the contributor paying nothing for Fund publications so long as a balance remains to his credit. A contribution of \$50, for example, earns a discount of fifty per centum from list price. Fund publications will not be distributed gratis or exchanged.

This discount plan is made possible by applying all contributions to a capital fund, administered by the Trustees of the Southwest Museum, Incorporated, of which the income alone will be used to finance the publication program. To date more than three thousand dollars has been received. Contributions should be directed to Hodge Fund, Southwest Museum, Highland Park, Los Angeles, California.

APPOINTMENTS IN BRITISH UNIVERSITIES

Oxford University has established a professorship of Social Anthropology; the new incumbent, Professor A. R. Radcliffe-Brown, to assume his duties in October, 1937. Cambridge University will shortly appoint a Professor of Social Anthropology to replace Professor Hodson, who retires in September, 1937. Thus there will be three professorships of this title in England, at London, Cambridge, and Oxford.

WE DEEPLY REGRET TO ANNOUNCE THE DEATH OF DR ALBERT B. REAGAN at Provo, Utah, June 1, 1936, aged 65. Prior to joining the staff of Brigham Young University in 1934, Dr Reagan was connected with the United States Indian Service from 1899, being stationed at various reservations in Minnesota, Washington, Arizona, and Utah. His active interest in the ethnology, archaeology, and paleontology of the peoples and areas in which he was stationed, led to publication throughout this period in the Transactions of the Kansas Academy of Science, Transactions of the Utah Academy of Sciences, Arts and Letters, Journal of American Folk-Lore, AMERICAN ANTHROPOLOGIST, and elsewhere.

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APACHE DATA CONCERNING THE RELATION OF KINSHIP TERMINOLOGY TO SOCIAL CLASSIFICATION

By M. E. OPLER

IN a recent exchange between A. L. Kroeber and A. R. Radcliffe-Brown the latter took the occasion to state his position upon the question of the relation between the terminological classification of kindred and the social classification. To quote his own words: "The former is revealed in kinship terminology, the latter in social usages of all kinds, not only in institutions such as clans or special forms of marriage, but specifically in the attitudes and behavior of relatives to one another."¹ It is between these two aspects of social structure that Radcliffe-Brown sees a "fairly close correlation."

Toward the close of his communication Radcliffe-Brown lists, with evident approval, a number of studies of the "nature and degree of inner consistency to be found in kinship systems." Following as this does his claim for high correlation between kinship terminology and social classification, I take the implication to be that these several studies yield support for his point of view. Among the studies of tribes listed there is reference to the Apache, and I have reason to believe that in this citation Radcliffe-Brown is thinking, at least in part, of data drawn from comparative ethnological studies of Apache tribes which I have completed in recent years. It is only within the last few months that it has been possible to arrange and review the full body of Apache materials. Some of the data lately analyzed are so pertinent to the important point Radcliffe-Brown has raised, that I feel impelled, in justice to him and to those who have been guided by his reference to Apache material, to explain that a comparison of Apache kinship systems discloses some grave discrepancies between kinship terminology and what Radcliffe-Brown has defined as "social classification."

When I last saw Professor Radcliffe-Brown, he commented with satisfaction upon the fact that the Mescalero Apache do not differentiate between cross- and parallel cousins either in terminology or behavior patterns, whereas the Jicarilla Apache, who address different terms to cross- and

¹ *Kinship Terminologies in California* (American Anthropologist, Vol. 37, pp. 530-35, 1935), p. 531

parallel cousins, likewise differentiate between these two kinds of relatives in behavior pattern.² This appealed to Radcliffe-Brown as a double proof of the correctness of his thesis. He felt that his view of the necessity of internal consistency in form and function within any given tribe was thereby vindicated and that further confirmation of a comparative order was established when, in going from one Apache tribe to another, a change in terminological classification was accompanied by a change in social classification.

The facts in respect to cross- and parallel cousins for these two Apache tribes are as stated, but too hasty a conclusion must not be drawn from them. If one is willing to weigh all the evidence one will soon learn that a significant correlation between the terms which an Apache addresses to his age-mates and the behavior patterns he accords them, is far from established.

Now it is true that a Mescalero Apache does not differentiate between cross- and parallel cousins. In fact he does not differentiate terminologically between siblings and cousins of any description; he has one term for sibling or cousin of the same sex as himself and another term for a sibling or cousin of the opposite sex. But while there is nothing in the terminology to indicate it, there is a difference between his attitude and behavior pattern for a cousin of the opposite sex and his treatment of a sibling of the opposite sex. The most acceptable way to treat a cousin of opposite sex in former times was to avoid her entirely. When practical considerations made this inadvisable, restraint which amounted to avoidance was initiated between these two relatives. On the other hand, though a formal restraint existed between siblings of opposite sex, it was of a much different degree, if not of a different order. Such siblings slept in the same dwelling; a sister might prepare food for a hungry brother, etc. In terms of commonplace behavior it makes quite some difference whether a person flees from the presence of another individual or moves about in the same encampment with him. Yet this important difference in attitude and behavior has no counterpart in a refinement of the terminology.

² Theoretically there would be no difference in the behavior of a Mescalero towards a cross-cousin of opposite sex and a similar cousin more remotely connected, as, for instance, through a grandparent's sibling. If, however, there is no very close bond with the grandparent through whom the relationship is established (and this happens occasionally with a father's father among Apache since residence is matrilineal) and his descendants do not live nearby, the relationship may become blurred and the usages less strictly observed. The tendency is towards a gradual diminution of intensity in socially prescribed behavior as the range widens. Yet this may be modified by special conditions. An Apache may be more concerned about his behavior towards a distant relative than he is about behavior to a nearer kinsman simply because circumstances throw him frequently with the former.

Some may doubt the fundamental nature of the avoidance relation between Mescalero cousins and think of it as a slight heightening of the restraint between siblings of opposite sex which requires no special terminological notice. For these I cite Lipan Apache data. Mescalero kinship is rich in avoidance relations; for the Lipan the avoidance of one relative only is permitted. This relative is not the mother-in-law nor any affinal relative. It turns out to be the cousin (cross- or parallel) of the opposite sex. Yet, despite the delimitation of this avoidance to cousins, one term suffices for cousin and sibling of the opposite sex.

Let us now turn to another aspect of the question. Radcliffe-Brown noted that Jicarilla Apache cross-cousins were terminologically separated from parallel cousins (parallel cousins being classified as siblings) and that the treatment accorded the cousin group was divergent from that extended to the sibling group. But there are other data less adaptable to his interpretation.

In scanning the material we remark, for instance, that there is one term which may mean cousin of either sex. But there is another term for cousin of the same sex, *male speaking*, and this latter term is practically always used to the appropriate relative. There is no corresponding term which can mean cousin of the same sex, *female speaking*. It seems eminently sensible that the Jicarilla boy should have a term which differentiates his male cousin from his female cousin, for his relations with those two relatives are as antipodal as can be imagined. With his male cousin he carries on a robust joking relationship which even extended in aboriginal times to the good-humored theft of his relative's mate. Towards his female cousin he observes rules which vary from stringent restraint to full avoidance.

But what of the Jicarilla woman who has but one term which must serve for the two relatives to whom she reacts with equal disparity? Her pattern of behavior with respect to her male and female cousins does not differ a whit from that of the Jicarilla man. She browbeats, plagues, and humiliates her female cousin to her heart's content and makes off with this cousin's spouse on occasion. She exercises that same restraint or avoidance with respect to a cousin of the opposite sex as does the Jicarilla male. Our suspicions are aroused. Is the correlation of kinship terminology and social classification partial to the single standard?

I could list other items of a similar nature drawn from the data of Apache relationships in ego's generation, but perhaps those which have been mentioned are enough to indicate that the correlation which Radcliffe-Brown seeks to establish will hardly cover the exigencies of Apache kinship, even for this one generation. On the contrary we are confronted, even in

the few data reviewed, with terms which seemingly ignore or flout marked behavior patterns, and with important usages and behavior patterns which leave no reflection in terminological classification

It will prove somewhat illuminating if we consider the position, with regard to both terminological and social classification, of the mother's sister in various Apache groups. The prevailing Jicarilla Apache practice is to classify the mother's sister with the mother terminologically. This procedure seems altogether reasonable in terms of what Radcliffe-Brown has described as "social classification." In Jicarilla life sisters are associated in work and in play as they grow up. If one sees two Jicarilla women laboring together at a task, a justified guess is that they are sisters. Residence is matrilocal; husbands are brought to the parental camp. Therefore marriage little disturbs the bond between two sisters. Sororal polygyny was practiced; a man, if he felt inclined to indulge in plural marriage, wedded sisters of his wife as they matured. Thus sisters could be co-wives. The sororate was encouraged; a man was obliged to marry his dead wife's sister if her family so decreed.

Let us now consider the station of the mother's sister in another Apache society, the Chiricahua. Every institution and usage which has been listed for the Jicarilla is true of the Chiricahua as well. The close common bond which we noted for Jicarilla sisters is just as characteristic of the Chiricahua scene. But where the Jicarilla classify the mother's sister with the mother, the Chiricahua classify her with the mother's brother.

We pass to the Lipan Apache. The Lipan Apache view the mother's sister as do the Chiricahua in all other respects, but the Lipan did not practice polygyny, and therefore sororal polygyny drops out of our reckoning. There is, consequently, one less reason than we could list for the Chiricahua for the Lipan to identify the mother's sister with the mother. This time, we think, we may surely predict that the mother and her sister will not be classified together by the Lipan. But, on the contrary, this is precisely what happens.

Let us now turn to the White Mountain Apache and review the data on institutional life and behavior patterns. We note that sororal polygyny is here restored. We learn that all the threads which bind together the Jicarilla sisters are here in evidence too. We discover that in addition to all the circumstances which tend to identify sisters in other Apache tribes, a new and important element is added. The White Mountain Apache are organized into strong matrilineal clans, and relations between sisters are further solidified by the feeling of clan unity and common descent. We feel no hesitancy in advancing the opinion that the mother and her sister must here

be classified together. And again we are wrong. The White Mountain Apache address a separate term to the mother's sister.

Our attention is invited to the Mescalero Apache. Of all the Apache groups, the Mescalero most closely resemble their immediate neighbors, the Chiricahua. The correspondances in dialect, myths, rites, and social organization are so numerous and striking that the common origin and late differentiation of these two tribes are patent. So far as the mother's sister is concerned, the behavior patterns regarding her do not differ a whit in these two tribes. It is a temptation to predict that the Mescalero address the mother's sister as the Chiricahua do (i.e., classify her with the mother's brother). Yet the Mescalero have no less than three ways in which they may address the mother's sister. Most infrequently they classify her with the mother, they may classify her with the mother's brother (the Chiricahua practice), but commonly they call her by a separate term.

Two interesting facts emerge from the survey of the position of the mother's sister in Apache society. One is that the position of the mother's sister in terms of Radcliffe-Brown's "social classification" is everywhere much the same. The extended domestic family with matrilocal residence is the basic unit of social organization among the Southern Athabaskans, and the web of behavior patterns and usages which attaches to it has among all the Apache peoples a common flavor. The place of the mother's sister in this web of behavior patterns does not change significantly as one goes from Apache group to Apache group. When elements are introduced which might conceivably alter the status of the mother's sister, such as the clan, these changes seem to have no relation to terminological shifts.

The other impressive conclusion is that despite the unity of social classification for the mother's sister in Apache society, there is anything but agreement in respect to the terminological classification to which she is subject. We have seen, even in the incomplete data presented above, that she may be classified with the mother, with the mother's brother, that a separate term may be utilized for her, or that various choices and alternatives may be acceptable. The thesis that there must necessarily be a high correlation between social classification and terminological classification seems eloquently refuted by Apache data pertaining to the mother's sister.

From the many cases of disharmony between social classification and terminological classification which recommend themselves to my attention, I shall point to but one more. I choose this from data concerning the grandparental generation of the Mescalero and Chiricahua. Both of these tribes use four identical, self-reciprocal terms for the grandparent-grand-child relationship. The Chiricahua call the grandparent's siblings of either

sex by the same term they use to the grandparent. For the Mescalero, however, the great-uncles and great-aunts are differentiated according to sex, the maternal great-uncle being classified with the maternal grandfather, while the maternal great-aunt is classified with the maternal grandmother.

Is there any difference in behavior patterns or usage for the respective great-uncles and great-aunts of these two tribes which would render this terminology consonant with Radcliffe-Brown's views? I found not the slightest evidence of such a thing. But I did find a factor with which this procedure shows correspondance. The Mescalero are prone to recognize sex distinctions for purposes of classification where the Chiricahua are not. Witness the tendency of the Mescalero to separate the mother's sister from the mother's brother terminologically while the Chiricahua classify those two relatives together. Again, the Chiricahua and Mescalero both impose many avoidance relations upon the man at marriage. But, whereas the Chiricahua man avoids affinal relatives of both sexes, the Mescalero newlywed avoids his wife's female relatives only.

When I first recognized the sex nature of these alignments, I thought that Radcliffe-Brown's functionalism was perhaps applicable, but in a wider, more fundamental sense. I thought that there might be some signal difference in the position of women, or in the relation between the sexes for these two tribes—that a scrutiny of the division of labor, of differences of rights according to sex in ceremonial matters or in social privilege, would furnish the clue. But the two tribes proved to be unmistakably alike in all these respects and I was forced to abandon my effort to apply a view in which I was genuinely interested.

I cannot dwell upon the implications of this comparative material without some degree of personal humility, for immediately following my first two and a half months of field-work (field-work which happily continued for a much longer period of time) I wrote a paper of which I am not now too proud, which argued for the fitness of Mescalero and Chiricahua terminological classification in the light of their institutions and behavior patterns.² It is true that my path was smoothed by my own ignorance. I was quite oblivious of the cousin avoidance, for instance, and was there-

²The material upon which this paper was based was gathered on the Mescalero Reservation in connection with a summer field-training expedition of the Laboratory of Anthropology of Santa Fe. My colleagues and I found three much intermarried Apache tribes occupying the reservation and could not, considering the short time at our disposal and the fact that we were concerned with other aspects of the culture as well, settle all matters of kinship and social organization according to tribal origin. What we emerged with, though we did not know it at the time, was a general average of contemporary reservation usage. During succeeding field trips, I was able to work out the differences according to tribe.

fore untroubled by the lack of terminology to separate cousin from sibling. But I wrote persuasively and made it all appear reasonable and almost inevitable. I am convinced now that this can always be done for one tribe or for two tribes which agree as closely in terminology and usage as do the Mescalero and Chiricahua. It was not until comparative labors gave me identical social situations coupled with widely different terminological classification and, conversely, divergent modes of classifying relatives where no change in behavior pattern warranted it, that I began to suspect that I had been more mellifluous than correct. As my comparative ethnological studies proceeded and the contradictory evidence to a simplistic conception of the relation between kinship terminology and social classification multiplied, I was forced to re-examine the entire question.

For a suggestion concerning the limitations of Radcliffe-Brown's formulation as I began to see them, we will do well to turn once more to his exchange with Kroeber. Radcliffe-Brown has here enunciated his position with perfect clarity. Kroeber, in commenting upon two tribes whose modes of classifying kin differed, though they seemed to be remarkably alike in other respects, called attention to the fact that the Yurok of California classify the father's brother and mother's brother together, whereas the Tolowa distinguish them. Radcliffe-Brown responded as follows:

There are three possibilities. (1) That the Tolowa have differences in attitude and behavior towards the two kinds of uncle and the Yurok do not. In this case both terminologies are correlated or consistent with the social classification. (2) That in both tribes there is no significant difference in the attitude and behavior towards the two kinds of uncles. In this case the Yurok terminology is consistent with the social system and the Tolowa is not. (3) That in both tribes there is a significant difference in attitude and behavior towards the paternal uncle on the one side and the maternal uncle on the other. In this case the Tolowa terminology is functionally consistent with the social system and the Yurok is not.⁴

What are the undeclared premises of these instructive statements? The first, and most astonishing, is that Radcliffe-Brown conceives of social classification as a unit characteristic. The terminology agrees with the social classification or it does not. The terminology and behavior patterns are functionally consistent or they are not. For Radcliffe-Brown, a person's position in respect to any relative is so clear, so unambiguous, that it can be unerringly classified. There are no steps, no half-tones, no perplexities. It is not irrelevant that he has termed the very process "social classification." Whence comes this functionalism by ultimatum? I suspect that it is

⁴ *Ibid.*, p. 532.

not functionalism at all. I have an earnest conviction that functionalism is less schematic and more subtle than this.

Let us return to the classification of mother's sister which we touched on above. We saw that the mother's sister functioned in much the same way in all Apache groups, but that there were a number of different ways of classifying her terminologically. To narrow the argument and bring the issues to the fore, let us say we have two such Apache groups, one of which classifies the mother's sister with the mother while the other uses a special term for her. I grant that the simplest manner with which to deal with such stubborn irregularities is to explain that the terminology of one tribe is consistent with the social system and the other is not. But if we must call ourselves functionalists why not try functionalism? The truth is that there are good and sufficient reasons why a mother's sister should be associated terminologically with the mother in Apache society, and there are other excellent and convincing reasons why she should stand alone in this respect. The circumstances which validate the classification of the two women under one term have already been indicated and include matrilineal residence, the possibility of sororal polygyny, and the sororate.

On the other hand, married sisters, while they live in the same cluster of camps, erect separate dwellings. Though sororal polygyny was at all times permissible, it took a wealthy man to support so large a household, and consequently in practice it was the exception rather than the rule. The execution of the terms of the sororate depended upon the presence of unmarried sisters of marriageable years, and while unions of this type were not infrequent, they did not constitute a large percentage of all marriages. A child's own mother lives in the same dwelling with him and fondles and cares for him, sews his clothing, etc. She stays with the man he recognizes as his father. But his mother's sister, kind to him and interested in him though she may be, lives most often in another dwelling and with a man who is merely the child's relative by marriage. Usually she has children and cares of her own.

These facts, pro and con, are true for both of these Apache tribes and form the basis for possible variation. One tribe has selected the first set of conditions as the rationalization of its particular usage and the other invokes the second set. I would hesitate long before deciding which of these kinship classifications is "functionally consistent with the social system."

Incidentally we have here some hint of how the "high correlation" between terminological classification and social classification of which Radcliffe-Brown has spoken may be realized. When, in working out the kinship system of the first type, the conversation turns on the informant's use of

the same term for mother's sister as for the mother, he is not incapable of rationalizing and returning answer that it is because his mother's sister is "just like" his mother. If proof is required he will appeal to the institutions and usages listed above. And when there is occasion for a member of the other tribe to explain why he does not classify his mother's sister with his mother, he just as stoutly defends as the only intelligent one the terminological solution at which his tribe has arrived. His mother and his mother's sister are two different persons as anyone should be able to see, he tells me. His mother nursed and raised him and disciplined him when he needed it too. He confuses her with no one else. It is to be hoped that ethnologists who would be most reluctant to accept as fact a native rationalization of a myth, will be just as circumspect before offering these constructs as the key to the relation between terminology and behavior. Just as often they seem to me to be attempts on the part of natives to vindicate and impart consistency to their particular terminological usages. I question whether such statements are necessarily the most accurate descriptions of the relationship as it actually exists.

In short, since terminological classification is obviously an arbitrary and limited procedure, we have no reason to believe that it can or does adequately symbolize complex relationships. As I have attempted to show, in two groups the same relationship may be classified differently terminologically. When this happens it need not be because of functional consistency in one tribe and the lack of it in the other. It may very well occur because two or more terminological choices are implicit in the totality of behavior patterns which surround any one relative.

A brief return to the Apache mother-mother's sister relationship may illuminate this conception. If we symbolize the lives and fortunes of two Apache sisters by two circles, we shall see that at some point these circles cross-sect. There is a portion of each circle or life which merges with the other, then, and this may represent the institutions, common experiences, and obligations which operate to identify these women. A portion of each circle remains outside the other and typifies those aspects of the Apache woman's role which are entirely separated from the affairs of her sister. I hold that it is no functional inconsistency to recognize either or both of these possibilities in terminology. The functional inconsistency, on the contrary, would be the failure to appreciate the extent and diversity of the roles of these women in Apache society, and the crime against the most profound and satisfying type of functionalism would be a drab and mechanical practice of giving due notice to that fringe of social behavior only which has happened to find expression in kinship terminology.

The limitations of the attempt to use the correlation of kinship terms and social classification as a guide to the realities and functional consistency of a social system will become apparent if we consider social relationships which never could be properly expressed in terminology. An instance that comes to mind is the case of the Jicarilla grandparent.

Now the Jicarilla grandparent serves in a double role. He acts as the teacher, disciplinarian, and chief bugaboo of the child, and the measures, many of them stylized and institutionalized, to which he treats the youngster who loiters in the tasks set before him, are severe and command respect. On the other hand, when all is going smoothly, a joking relationship, replete with practical pranks and bantering, often about sexual matters, replaces the more serious tie. The Jicarilla have failed to invent any kinship terminology that will correlate with the grandparent-grandchild relationship as they conduct it, and anyone who has tested the substance of that relationship will hardly wonder at it.

But the oversimplification of the concept of social relationships, this implication that any social classification is so unitary in character that it will find expression in single modes of terminological classification, is but one of the undeclared premises of the Tolowa-Yurok formula to which exception must be taken. The corollary to Radcliffe-Brown's insistence that when the social classification of a relative cannot be correlated with the terminological classification, a functional inconsistency between the social system and the classification exists, must be the assumption that kinship terminology is *the* way to express social relations.

Is it not time that kinship terminology cease to be the sacred cow of anthropology? Let us grant that kinship terms are significant labels often. Let us agree that they become overlaid with meaning and feeling tones for the relationships they imply. Let us admit that they are often the symbols and reminders of duty and specific obligation. Let us point out that among some people verbal classifications which reflect relationship and obligations may loom most important and may be worked out with rare completeness. But above all let us recognize that terminology is but one way of many, *a* way in which recognized social relationships may be represented. Granting that kinship terminology is an economical, convenient, and much used device with which the native keeps account of matters in the social sphere, it is, nevertheless, not the only means at his command for so doing. Myths, tales, penalties, instruction, common understandings may be utilized to this end and may usurp the place of terminology in marking and perpetuating certain behavior patterns.

In my own field-work I have come upon a great number of social re-

lationships which are clearly marked and kept in mind by devices other than kinship classification. The attitude of the Jicarilla girl towards her male and female cousins is a case in point. Though the behavior patterns towards these two cousins are far from the same, these relatives are called by the same term. But the distinction between these two kinds of relatives is never blurred for an instant because of the lack of distinguishing terminology. From the moment the girl is able to understand language she is warned not to have traffic with her male cousin. She is overwhelmed with tales of what has happened to those who disobeyed this injunction in the past. She hears that social intercourse with a male cousin will be followed by a severe burn or some mishap with fire. She listens to evidence designed to prove that even lice from a male cousin can transmit to her a fatal illness. She learns that should she become mad the water in which a male cousin has washed his hair would constitute an effective remedy. Through other stories, advice, and persuasive demonstrations, a much different manner of regarding her female cousin is inculcated. Now these two social relationships are not to be correlated with any terminology manifestly, since separate terms indicating the sex of the cousins do not exist, but the social relationship is to be correlated with items of belief, with stories, with types of instruction, which are as expressive and impressive as any terminology could be. There is no functional inconsistency, considering the total cultural panorama, in the lack of differentiating terms for these relationships. From a functional point of view the culture calls attention in many ways to the relationships. No one except a person myopic to everything but kinship classification would argue that the relationships have not been emphasized and signalized enough in Jicarilla life.

Another example, pointing to the same conclusion, is drawn from the Chiricahua. When a Chiricahua Apache meets a bear, it is said that if he calls out, "Cila, do not look upon me," the bear, very much ashamed, will turn away without molesting him. The use of the phrase is an attempt to invoke sympathetic magic to avoid an unpleasant encounter. The Apache, addressing the bear by a term which signifies an avoidance relationship, makes him feel the embarrassment that any Chiricahua would feel in seeing one from whom he hides, and the precipitous flight of the bear is the result. The term *cila* means literally, "sibling or cousin of the opposite sex." There is no separate term for "cousin of the opposite sex." But, since only the cousin and never the sibling may be avoided, the word clearly refers in this context to the cousin. In other words the Apache not only retain a distinction in social relations for which they have no terms, but they *act* on the basis of this distinction. A few examples of this nature should be enough

to demonstrate that natives can build up systems of behavior and social communication, and that these can function smoothly in terms of inner understandings and other cultural correlates, without ever registering as criteria of classification in the kinship system.

One last item to suggest that behavior patterns between relatives can operate efficiently despite the lack of terminological reverberations may be in order. When I was gathering Jicarilla myths and tales, and relationship terms were given by informants, I would stop from time to time to inquire (since the Jicarilla have but two terms for grandparents, one for grandparents of each sex) whether the paternal or maternal grandfather or grandmother was meant. This itch for accuracy on my part irritated my informants at times, and I am sure that I was more than once suspected of injecting the question for its nuisance value. My informants felt that I should be able to make the identification from the context. If a child is said to run a few steps to his grandfather's home to hear some stories or carry a message, it was expected that I appreciate at once that with matrilineal residence in force only the mother's father would be so situated.

It must be understood that the native brings to his round of activity, whether that be the telling of a story, the performance of a rite, or the calling of a kinship term, a special sensitivity to the totality of his tribal life. Terms and classifications which a less sensitized anthropologist may consider essential to an understanding of the outline of a social system, may be made less necessary by some other hall-mark or ground of common understanding. Our office is to use kinship terminology when we can, and not to be ruled by it when we cannot.

UNITED STATES INDIAN SERVICE
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I

FOR a variety of reasons the present seems appropriate for considering briefly the status of Polynesian research in ethnology and anthropology, to evaluate research already done and to look at the immediate future in these terms.

The incentive to modern research in Polynesia came from a report prepared for the First Pan-Pacific Science Conference, held in Honolulu in 1920, by a group of prominent scientists, including Dixon, Kroeber, Lowie, Rivers, Sullivan, Terman, Tozzer, and Wood-Jones. This report, *Recommendations for Anthropological Research in Polynesia*, published in the Proceedings of the Conference, marks a new chapter in Polynesian research. It noted that research in all areas of the Pacific was of great importance but considered the Polynesian problem as the immediate primary undertaking, since Polynesia comprises the heart of the Pacific. The Report noted further that skilled anthropological study had had, to that date, no place in Polynesia, the greater part of available data having been gathered by untrained observers in an unsystematic manner. After surveying this material, the Report proceeded to sketch problems to be stressed in future research. These were general and specific topics of investigation paralleling similar discussions in, for instance, *Notes and Queries in Anthropology*. The Committee's remarks on linguistics were succinct. It stressed the necessity of coordinating synthetic work, pointing out that only a trained philologist devoting himself uninterruptedly to the task for five to ten years could solve the complex problems involved and leave Oceanic philology as an organized usable body of knowledge where then it was but a mass of chaotic data. Historic and psychological researches were also outlined, the former to meet the requirements of scientists wishing to study the exact cultural phase of the Polynesians at the advent of the first European discoverers, the latter embracing mental and sense testing and psychoanalytic fieldwork. Finally, the Committee felt a need for comprehensive synthetic research, especially in the fields of linguistics, cultural history, and racial affinities.

In research work carried out in Polynesia since 1920 there has been evident, in general, a desire to attack problems in the order of their urgency—the criterion of urgency very clearly being the rate at which essential evidence was disappearing. Anthropological research has seemed important because, according to some authorities, 1930 was the date beyond which the information for most islands would be of rapidly decreasing value. It was

with this assumed need of urgency in mind and with the support of the recommendations of the Committee that many detailed reconnaissance studies were initiated in 1920 and continuously carried on. This scientific survey profited much from a cooperation among the various sciences which did away with overlapping of research. By 1928 it appeared to some that, while the study of Polynesian culture was by no means complete, yet from some islands, especially those uninhabited, little more knowledge was obtainable, and for most islands about seventy-five percent of the ascertainable data had been gathered.

II

In the light of this rapid survey of what was needed in Polynesia, and what was believed to have been accomplished by 1928, it is of interest to attempt to evaluate some of the recorded research material. First, however, one feels that in laying a foundation of field work on which later syntheses are to be built, the survey method is an unprofitable method, though of value in filling in gaps after the foundation is finished. Its use by ethnologists was dictated undoubtedly by a felt need for speed. Hence in order to cover most of the Polynesian islands in ten years, only a little time was available for each. What time was available had therefore to be employed in collecting ethnological material along orthodox lines (e.g., along the lines of the Pan-Pacific Science Conference Report narrowly interpreted), and in anthropometric surveys. There is no point in discussing the validity of that preliminary survey of the Polynesian population which set 1930 as the deadline year beyond which for most islands information obtained would be of little value. In general it is safe to assume that the more old people there are participating in any culture the greater will be its vitality, but the experience of ethnologists who, for the sin of youth, have been condemned to work Polynesian communities since 1930, suggests that with a culture-conscious people like the Polynesians, a 1930 deadline was more pessimistically than soundly visualized. Be this as it may, the result is that expeditions have now been sent to most of the Polynesian islands. Much material has been collected, but I think he would be a rash ethnologist indeed who would defend the proposition that probably seventy-five per cent of the available data has been collected. My own feeling is that from most of the islands studied, the ethnologist has come back with material sufficient to establish with greater or less precision (usually less) the formal patterning of the culture studied, but with insufficient material to suggest, let alone to formulate, the more implicit patterns of the culture or of individual variation on the main pattern. Yet it is just these implicit patterns and these individual variations that help to define the

reference of the formal structure. Without information about them the task of describing how and why a Polynesian culture works has not even been faced.

The result, therefore, of the survey system is that we now have available a collection of surface data from almost all the islands enclosed within the so-called Polynesian triangle. But from all of these islands more information still is desirable and may surely still be obtained. As an example, take Samoa. Apart from early accounts, missionary and other, the last years have given us Margaret Mead's studies of Manua and Te Rangi Hiroa's work on Samoan material culture. Te Rangi Hiroa's study is already a classic, and Mead's reports have adequately covered one island of Eastern Samoa. But there remain other islands of the Samoan group. In order to study pattern and pattern variation in one of the largest and most interesting groups of Western Polynesia, there is vital need for studies at least from Upolu and Savaii. What one visualizes here is not a quick survey or an attempt to study formal structure only, but careful investigations of small isolated districts which will focus first on village life and then work out gradually to include intervillage and finally interdistrict relations. The situation must be somewhat similar for the Tuamotus. The Tuamotuan survey has covered many islands, some of them unique in that there still live on them men who have witnessed pre-Christian religious ceremonies and who have been conditioned from childhood to maturity by old-time social customs. With this situation one feels that the time of the expedition might better have been occupied, for the ethnologist at any rate, on but one or two of the islands, in order to achieve a well documented and penetrating study rather than dissipating attention in the effort to master the highlights of comparative Tuamotuan ethnology.

Granted then that the last years have seen much valuable work done and have transformed our picture of formal Polynesian culture, nevertheless one cannot but regret that the reconnaissance-survey sampling technique has been used rather than the slower but ultimately more valuable patient areal studies.

Regarding physical anthropology little needs to be said about recent work. Anthropometric measurements are an aspect of anthropological field-work that yields great success under the survey method. Work finished and in progress, first under Sullivan and later under Shapiro, has thrown much light on questions relating to the physical makeup of the Polynesian islanders. I think it yet impossible to correlate cultural distributions with physical correspondences, but the material in hand now allows us to talk with some surety about linkages of physical types within the Polynesian area.

A start is just being made with the study of blood groupings of the Polynesian peoples. The material gathered so far is difficult of interpretation and it is of interest to note that a recent worker in the field concludes: That a great deal regarding racial history is to be learned from the distribution of the blood groups, provided the clues are correctly interpreted, is obvious The deplorable aspects of the situation are the haziness of the superficial outlines of the problem, the mass of contradictory data, and difficulty of fixing on what is important.¹

It would appear therefore that it is premature to follow those who expect a revolution in our understanding of the Polynesians through present work on blood groups, though later, when work has advanced farther, general tendencies may disentangle themselves from the baffling complexity of material.

III

The linguistic situation in Polynesia is still chaotic. The injunction of the Pan-Pacific Science Conference that "expeditions be so planned that two men can handle the whole of archeology and ethnology, exclusive of linguistics," has in general been taken only too literally. Only one example of the result will be noted here. Notwithstanding the fact that phonetic records of Polynesian dialects can hardly be said to exist at present, there has been little attempt by recent investigators to clear up a matter which is of fundamental importance to comparative Polynesian philology; or at least, most investigators have attempted to clear up the phonetic problems of the areas in which they have worked only to make confusion worse confounded. In most recent ethnological publications the author has felt the need to summarize the phonetic characteristics of the dialect of his area. For whatever reason, the result is that systems employed in transcribing Polynesian texts are sometimes laughable in their absurdity. In general the obligation to be as systematically scientific in the treatment of the Polynesian dialects as, say, in archaeology or material culture, has been ignored. The necessity of building up a corpus of scientifically recorded Polynesian texts, without which no definitive comparative study is conceivable, has been ignored. This is due in part perhaps to the idea that what was good enough for early students is good for later workers, it being immaterial whether advances have taken place in the science of anthropology since the time when missionaries, struggling to reduce Polynesian sounds to a condition in which cheap printing of the Bible was possible, cast Polynesian into an alphabet at once unscientific and inadequate.

¹ William W. Howells, *Anthropometry and Blood Types in Fiji and the Solomon Islands* (Anthropological Papers, American Museum of Natural History, Vol. 33, Part 4, 1933), p. 330.

The situation was implicitly recognized by the Conference when it reported on the importance of a more adequate knowledge of the details of phonetics and a collection of native texts to supplement the missionary material already available. Presumably the Conference meant texts scientifically recorded. In any case, it will be recalled that the Conference felt that the linguistic problem should be thoroughly studied over a number of years by a trained philologist. It is a matter of regret that the linguistic situation is as vague and ill-defined today as it was twenty years ago. We are no nearer an understanding of the laws of phonetic change in the various Polynesian dialects, and none of the linguistic desiderata defined by the Conference is nearer achievement.

IV

One views the future with mixed feelings, conscious that much has been done in the past, but only too aware of the amount of work still to be done and the masses of data that may surely still be obtained from most Polynesian islands along other than old-fashioned lines of investigation. One hopes that the next years will see a continuation of the work on physical anthropology, that this investigation may be brought nearer completion. One hopes also that linguistic work will soon be initiated along the lines of the Conference Report of 1920 that this blank spot in Polynesian research be charted and mapped before it is too late. Again one hopes that in the future the lines marking the boundaries of permissible work (the "Polynesian triangle") will not be drawn with so much rigor as heretofore. It is often necessary to circumscribe research that effort may not be dissipated and energy wasted, but the time now seems more than ripe for a concerted and well planned attack on Fiji, for example, not only on the islands or areas where Polynesian influence is known or suspected, but on all the cultural variation of the whole Fijian area. Such work, well carried out, will undoubtedly add immeasurably to our knowledge of the extensions of western Polynesian culture, and, by contrast, to our present knowledge of Polynesian culture as a whole. Detailed knowledge of one of the areas where the cultures seem at present to overlap and intermingle in a confusing pattern will also enable us to evaluate with more surety the basic and superstructural contrast and similarity between Polynesia and Melanesia. Hand in hand with this attack should go work on the Polynesian outliers in the western Pacific. It might have been the part of wisdom for workers to have studied these earlier. Ontong Java and Tikopia are already worked, but the most fascinating outliers of all, Rennell and Bellona, if preliminary reports are correct, represent to this day stone age cultures presumably largely Polynesian, practically untouched by missionary or commercial exploitation. It

might have been strategic to study these outliers for the light they throw on an authentic Polynesian culture before white contact, along with studies of the more easterly Polynesian communities, instead of waiting until islands within the orthodox Polynesian triangle were completed. This is a matter of opinion. But it is surely not a matter of opinion that a study of these two outliers in particular should be carried out within the next few years. The need here is imperative. It is to be hoped that those interested in Polynesia will be able to make a complete, long-continued, and detailed study of all aspects of this culture, its formal patterning and its language, as well as the more implicit patterns and its cultural conditioning of the personalities that have grown up within it.

One would also wish for continued work in Samoa, especially along the lines of village studies in various districts; more work in the Ellice group where social organization is imperfectly known, in the Gilbert Islands, a meeting place of Polynesian and Micronesian cultures, where systematic work by trained observers might supplement the small body of material at present available. An extension of this work in the Gilberts to include the Mortlock Islands and perhaps some of the Marshall and Carolines, if such work is possible today under the Japanese mandate, would help to fill in the many gaps left by the earlier German literature.

Coming to Central Polynesia, one would expect closer attention to the material culture and technology of Tahiti, and a renewal of the Tuamotuan studies, not in the form of surveys but of reasonably long-continued and detailed areal studies of those islands that the initial survey indicates have most to offer. The Austral group is still inadequately worked. These islands might be profitably restudied in the light of manuscript and other material made available by earlier workers. A similar observation applies also to the study of the material culture of Tonga. This work, supposed to have been completed many years ago, is still not available, and it would seem that a new survey of this aspect of Tongan culture is desirable. Gaps in our knowledge will also continue until we have much more detail on the many technological processes involved in the material culture of the Marquesas.

Of Hawaii and New Zealand little need be said except to stress the necessity of coordinating work in each area. Hawaiian archaeology is fairly completely surveyed. Hawaiian linguistics will perhaps be difficult to study owing to lack of phonetic texts and probable changes, particularly phonetic, that have occurred during the past hundred years. Hawaiian material culture is distressingly incomplete. Museum study and fieldwork, however, may still bring some understanding here. Hawaiian ethnology has a dream-world character, consisting, with honorable exception, for the most part of

of accounts by untrained workers, accounts which are remarkable more for their high degree of formal systematization and structural rationalization rather than for any attempt to appreciate how ancient Hawaiian culture really worked. A skilled and summary survey of all those accounts with a careful noting of general and specific gaps seems necessary before field workers can attempt the task of reconstructing Hawaiian culture.

In New Zealand, to remind oneself of what has been done in archaeology, linguistics, physical anthropology, material culture, and ethnology is only to realize anew the magnitude of the task that awaits systematic exploration. This is, of course, no reflection on earlier workers. It is more an appreciation of their success under extreme difficulties. But it is also a realization that in the past neither university nor museum, neither public nor private interest, has had more than an elementary understanding of its obligation to further scientific work in a unique field of inquiry. Whatever be the reason for this indifference, and I am not concerned to analyze it here, it is still true that there is much that can be done in New Zealand using the techniques of modern anthropological method. Along with a linguistic study of specific cultural sub-divisions among the Maori people should go a detailed anthropometric study and research on social organization in those areas where a tribal group has maintained an integrity of residence and traditional association since the arrival of the first canoes. In social organization especially, by drawing on museum material and on older literature, one would expect work of such a nature that when placed alongside of the few earlier areal studies, one would be able more completely to see the problem of pattern and variation in those different sub-cultural groupings that we are too prone to lump together as constituting our stereotype of New Zealand Maori culture.

It is only when this work, in New Zealand and elsewhere, is completed that synthetic studies of Polynesian culture will have a final validity. Some aspects of this culture should be systematized sooner than others. This is especially true of material culture, where Te Rangi Hiroa's work during the past years has elaborated a method of analysis based on form and technology which in his capable and painstaking hands has produced measurable results. It is a method that gives an exceptionally firm support for comparative distributional studies, and enables one to appreciate more and more the specific cultural peaks that have been built in specific areas upon widespread Polynesian cultural patterns. It is likely, I think, that the concept of cultural peaks—highly developed patterns of behavior or thought constructions in social or material or artistic life—will ultimately prove more fruitful as a coordinating concept in Polynesian anthropology than

the present tendency to deal exclusively with Polynesian sub-cultures or strata of cultures. The latter tend to set up artificial conceptual barriers, the former make one continually aware both of differences and of underlying similarities in all Polynesian cultures.

Two related aspects of Polynesian culture are still, even today, almost unknown territory and may well deserve careful field study. The first concerns those complex problems relating to the impact of Polynesian culture upon the typical or aberrant Polynesian growing up within this culture. The orthodox study of a Polynesian culture is usually a presentation of abstract forms of behavior. The personal meaning of these forms is rarely considered save by a side-glance. Yet every field worker who has lived for the briefest time in a Polynesian village must be aware of extreme personality differences among his informants and friends. A few minutes' observation of a gang of playing children should be enough to prove the point. In Polynesia, as elsewhere, it seems that a good approach to the study of primitive personality lies in a careful and conscientious record of the activities of children with whom the worker is intimately acquainted. Save for a little recent work, practically nothing is known of the life of Polynesian children when they are not engaged in the playing of that interminable list of games which most of us so laboriously describe. Observation suggests that doing other things besides playing institutionalized games is a major activity of some Polynesian children at least. It also suggests that could such observation be continued over long periods, it would be possible to amass a body of data constituting a formidable challenge to both psychologist and anthropologist. Projected research in this field, however, must definitely take account of what may be termed the time-series, the importance of extended observation of the same selected children over a significant period of time. Observation of specific adolescents in a specific culture is legitimate enough. Inferences from these studies as to the general factors involved in the cultural conditioning of young children in the same culture are at best suggestive only. What is needed is life histories of young children from birth to, say, five years. When this information has been correlated with family background and cultural dynamics, it will be time for generalizations on the impact of Polynesian cultures upon the individual maturing within them.

The second aspect concerns the evaluation of the pattern configuration of Polynesian culture, the master ideas that are the guides to individual behavior. Experience suggests that Polynesia is a good field for testing the significance of the whole concept of patterns of culture, especially in those areas where European culture is as yet but a thin veneer over certain

aspects of the old cultural life. Cultural ideals are writ large in Polynesia—in the large body of traditional history well preserved on paper or in informants' minds, in the mass of chants that almost defy the translator's desire for decent English but hardly his understanding. A scientific evaluation, for instance, of the sex activities and symbolisms in Polynesian culture would reverse most current anthropological stereotypes of the dynamics of Polynesia. And with a clearer conception of the master patterns, much that is obscure in the complexity of that culture change brought about by contact of Polynesian with Western European culture is made understandable even if all difficulties are not immediately solved.

Polynesian anthropology today, in sum, has reached a cross-road where reorientation of research towards newer problems unclear to scientists of 1920 should go hand in hand with a determination to conclude successfully lines of work already well furrowed. This earlier work is the only possible basis for comparative and historical studies. Reorientation will give along with this the key to our knowledge of how and why Polynesian culture works, what gives it a continuing meaning and vitality for individual Polynesians. Neither field of research can be particularly successful unless the ethnologist is continually aware of the fact that Polynesia exists by definition only. Too rigid adherence of interest at this stage to only those groups of people living within the boundaries of the Polynesian triangle as traditionally defined can produce nothing but creeping paralysis and ultimate self-stultification of research.

HONOLULU, HAWAII

PUBLIC OPINION AND MUTUAL ASSISTANCE AMONG THE MONTAGNAIS-NASKAPI

By JULIUS E. LIPS

IN most modern states law and order are embodied in written constitutions which outline the rights and duties of the individual and the community. From time to time a change may be made in any given constitution in an effort to make it conform to an urgent current need, but the existence of a written constitution and a written body of law remain ponderables which act as strong bulwarks against tendencies toward a change.

It is generally assumed that an unwritten constitution may be better adapted to the changing conditions of life than a written or so-called rigid constitution. That this may be true without in any way lessening the force of law can be seen in primitive communities where life is guided not by a written constitution or even by a set of statutes but by public opinion. In primitive communities public opinion is in effect both the constitution and the law.

The legal institutions of the Montagnais-Naskapi Indians in Labrador are set down neither in a constitution nor a written law; yet every phase of their lives is regulated as definitely as in the white man's country. They have their supreme, if unwritten, law with its demands for their particular brand of "Life, Liberty, and the Pursuit of Happiness." But it would be futile to go about, textbook in hand, seeking an equivalent for the abstract legal terms of the white man's law. The same holds true for the rules of international law, where the theory of the enforcement of law is still in dispute and doubt.

For the Montagnais-Naskapi the maintenance of the peace of the community is fundamental, and they have various ways of enforcing peace. The strongest preventive against violation of the peace is public opinion. Although this is not formed by newspapers and printer's ink, it binds the individuals more closely to the community than in the civilized world. It is not public authority which keeps the band together, nor the chieftaincy, which appears rarely if at all. It is tradition, custom, and public opinion.

The economic situation has prevented class distinctions and variations in the accumulation of wealth. Here we have a single-class community, in contrast to the civilized world with its multiple classes--and public opinion has a totalitarian meaning, in contrast to the artificial and class-imposed totalitarianism of the modern community.

The century-old traditions and ancestral rights, fused with the later

* During the summer of 1935 the author investigated economics and law among the Montagnais-Naskapi Indians of Labrador Peninsula on behalf of Columbia University.

changes brought by the white man, are the basis of public opinion. It is in the summer meeting places that public opinion is particularly strong. The many experiences of life in the woods and all occurrences relating to rights are discussed and considered in these meeting places, and differences are usually adjusted through settlement by neutral parties. Public opinion lies dormant during the winter months, as the various families are separated by scores of miles and are dependent solely upon themselves. The settling of quarrels or differences is postponed until the following spring when the tribe gathers again at the traditional places for the purpose of selling furs to the Hudson's Bay Company.

Public opinion is a powerful force here, because the Indian cannot leave his tribe and lose himself in some other community as the white man often does when he moves to another city or state. The Indian cannot give up his hunting grounds, and should he attempt to escape unfavorable public opinion, he would starve. In these woods where the individual must turn to neighbors in time of need, as in the case of sickness, bad hunting luck, or unfavorable weather conditions, not only the security of his possessions but his very life depends on the attitude of the community towards the individual.

Public opinion also asserts itself when the peace of the community has been violated. This comes about slowly, however, and only when the peace of the community rather than that of the individual or family is disturbed. But even in that event public opinion must be prompted by the interested party. It is not the occasional trap thief, trespasser, or tent burner, but the habitual peace breaker, the constant trouble maker, who is punished by the community with expulsion. But once the community acts, he is outlawed and abandoned to starvation.

In this way public opinion can enforce law, by *active* or *passive* means. Its principal effect is, however, the *prevention* of legal infractions. It is *preventive* rather than *punitive*. Even in cases where there is no binding legal obligation, i.e., in cases where infractions against public opinion are not punished immediately, public opinion enforces the law. It would be erroneous to assume that no law exists because it is not enforced by an organized public authority or because an infraction of legal norms is not punished. Enforcement of the law or the administration of justice by public opinion is accomplished indirectly but effectively.

One of the important fields regulated by public opinion is mutual assistance. The rules of mutual assistance pervade the customary law of the Indians, running through their laws of obligations, their family law, and their criminal and commercial laws. It is noteworthy that these laws of

mutual assistance have the greatest effect in case of immediate danger of life or of starvation. In such cases they have full priority even over legal norms which under ordinary circumstances are sacrosanct. To mention a few examples:

The hunting and trapping rights of one's own hunting ground, anxiously guarded normally, may be broken if the stranger passing the territory is in need of food. In that case he may do sufficient trapping and hunting to provide for immediate needs, without, however, having the right to gather meat or furs in order to sell them. The travelling Indian, too, may catch as many animals as he needs to still his hunger while in the territory. Even the beaver, that most treasured animal, the shooting or capture of which is reserved for those entitled to hunt on the grounds, may be killed by a stranger in need. In such cases he may even kill all the occupants of a beaver house which has been marked by its owner. For if the Indian discovers a beaver house he marks a near-by tree, thereby notifying all passers-by as well as his own adult sons and hunting comrades that the beavers belong to him. This property right is thus respected even within the family: it represents perhaps the most respected property right even in our sense of the term. Nevertheless this rule may be broken by any one who is really in need.

If a person suffering from hunger arrives at the food depot of another Indian, he has the right to take one-half of the provisions without asking the permission of the owner and without paying anything. The owner cannot demand a consideration—even though public opinion requires the subsequent return to the owner of all that was taken, and even more, as soon as the man is able to do so.

The effect of public opinion on mutual assistance may be illustrated by a further example which is very important for the whole problem.

In cases of illness, accident, or famine, the Indian sets up signs calling for help, each of which has a very definite meaning. It is a signal system understood by every Indian and used only in the case of extreme need. When such a sign has been erected its purpose is not to turn "to whom it may concern" but to advise that "everybody is concerned."

There are two different kinds of signal posts: one signifies illness or accident, the other hunger and starvation. Both can be combined, since hunger and illness often occur simultaneously. In such a contingency a post, about two yards high, is set in the snow on the border of the hunting ground or on an Indian path, wherever one would expect a neighbor or others to pass by. The pointed tip of the post points in the direction of the tent of the man asking for help. At the upper third the post is carved all around; the deeper

the groove the greater is the illness and need, and the assistance required (fig. 1: nos. 1 and 2).

The distance of the person seeking aid from the place of the signal post is marked by a second post, set vertically in the ground. If the distance is small, the vertical post is set close to the end of the signal post sticking in the snow (1a). If the person asking for help is farther away, the vertical post is set in the snow at a greater distance from the lower end of the signal post (1b). If days' trips are to be indicated, one uses at times two or three vertical posts (2) each of which signifies a day's trip, and the sign is repeated along the way to the tent, the number of posts (indicating a day's travel) being diminished with the decrease of the distance.

The signal post for hunger and starvation has the same form and is set in the same way as the sign indicating illness or accident. Only in that case the groove is made in the form of an hour-glass (3) and by its depth it indicates the degree of famine and need for help. If the person or his family seeking aid is not only hungry but also ill, combined signal posts are set up in a parallel direction (4). Hence Number 4 would have to be interpreted as follows: "We, the tent community living in the direction of the signal posts, are about a day's trip from here. We are very sick and near starvation. [Note the deep notches on both signal posts.] We call every one passing by, who sees these posts, to bring immediate aid."

How does the single individual or the outer world react to this call for help? As the posts have not been set up randomly, but, in order to reach the outside world as quickly as possible, on well known travelling routes of the Indians or on the trap path of a neighbor, aid arrives in time as a rule. The neighbor or any passer-by, noticing this call for help, will rush as soon as possible to the tent of the sufferer. In most cases, however, this cannot be done at once, as the helper may not be prepared for the emergency. He may be on the way to his traps with only enough food for himself. In any event, if unable to help immediately, he will inform the caller for aid that he has seen the sign and that he is willing to bring help. This is done in the following way:

He may make a notch with his axe on the vertical post (5) which signifies: "I have read your message and I will bring aid," or he may tear off a branch, fold it into a ring and hang it on the signal post. Instead of the branch he may hang a leather or cloth knot on the signal, according to what he happens to have on hand. Generally such a ring means the same as a notch, but it indicates further details. The smaller the ring, the sooner the helper will return (6): if the ring on the other hand is very large (7), it means that it will take a long time until assistance can be given. Often the

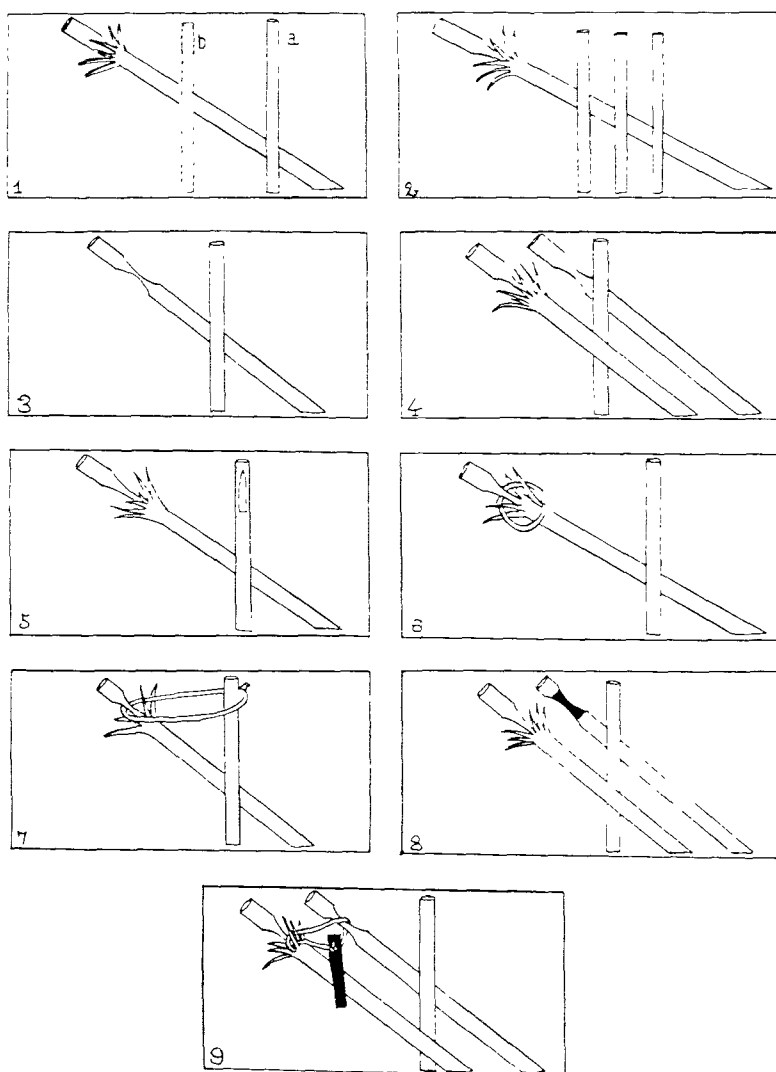


FIG. 1. Montagnais-Nasliapi signal posts indicating the need of assistance

person who has seen the signal must first return to his own tent, far away, in order to get provisions and other means of aid, before he can set out on the mission. In any case, every Indian who has seen such a signal of distress will inform not only the person calling for help but all other passers-by of his intended assistance by means of a ring or notch.

When the assistance has been rendered the signal posts are destroyed so as not to mislead those who pass by later. It may happen sometimes that the helper arrives after the suffering individual or family has died. In that event the signal post is not destroyed. The notch of its upper end is blackened with soot (8), or a blackened piece of wood (9), often in the form of a cross, is suspended on the post. It is possible also to convey the death message by blackening one side of a piece of birch bark. In order to protect it against dampness it is folded and inserted in the notch of the signal post. Upon seeing this death message every passer-by is informed that his assistance is too late and that somewhere deep in the woods a person or family died of illness or starvation.

All these signs are erected only in the case of extreme need, and Indians have told me that people to whose aid they had gone in response to such signals "looked terrible and were close to death."

This aspect of the law indicates the defensive character that pervades all branches of this culture. In response to these signals every one passing by must and will help. No instance is known where assistance has ever been refused; and the erection of such signals occurs very often in the vast woods of Labrador. Even the most hated neighbor or the most bitter enemy, who perhaps a year before had destroyed and pilfered one's own traps, must and will be assisted.

What then compels the Indian to stand by his hated personal enemy, instead of enjoying his misfortune? The answer is: public opinion. Other explanations might be given. Thus, if one asks an Indian why, in such a case, they assist even a hated personal enemy, he will answer: "If we didn't do it we would have bad hunting luck and the following morning the 'Man of the North' might order his wind to cover the game tracks with snow." However, the strongest motive prompting them to give positive assistance is the realization that in case they refuse to assist they too might be refused needed assistance—for, if a case should become known where an Indian maliciously disregarded a signal erected in extreme need, he would likewise be disregarded in case of his own need.

All this shows that the enforcement of law in earlier cultures was not in the form of positive acts but in the threat of passive conduct. It is erroneous to contend that, in cases where a law is not enforced by positive

rules and "the systematic application of the force of politically organized society" (Roscoe Pound), we cannot consider it a law. For, in this instance, public opinion enforces by negative and passive means the positive act of the individual. In any event the fear of retaliation is sufficiently strong to compel observance of the legal norms within the community.

However, this unwritten constitution of the Indians is by no means rigid, but is regulated by changing conditions. As a result of the dealings with the Hudson's Bay Company, for example, entirely new and definite rules were developed, sanctioned by public opinion and embodied in a new law merchant and law of contracts. It is clearly evident that rights and rules of law are strongly influenced and altered by economic conditions, that we cannot find one rigid and eternal law in any people, and that there is no unified and generally applicable feeling of justice in humanity.

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THE QUESTION OF RELATIONSHIP
BETWEEN THE CULTURES OF AUSTRALIA
AND TIERRA DEL FUEGO

By D. S. DAVIDSON

THE presence of similarities in the cultures of Australia and Tierra del Fuego has been noted by several writers and various theories of historical relationship have been offered. Both Graebner and Schmidt in their general studies concerned primarily with other cultures of Oceania and South America pointed out a few of the resemblances in Fuegia and Australia, and explained them in terms of the *Kulturkreis* theory without committing themselves to a specific route of migration of peoples or of diffusion of the traits involved. Within the past decade, however, attention has been focussed directly upon Fuegian and Australian cultures by Rivet and Koppers, who from different points of view have postulated historical unity, but who in reaching their conclusions have offered explanations which cannot be reconciled.

Koppers advances the theory that the similarities in the two regions represent the vestiges of an ancient culture carried out of Asia by two groups of Australoid peoples, one eventually reaching the southern tip of South America by an overland journey via Bering Strait, the other arriving in Australia after traversing the East Indies and New Guinea. In terms of his hypothesis identical appearances in the two areas concerned are neither expected nor required, for it is argued that in the thousands of years elapsed since the original migrations began there naturally would be important divergencies. Koppers therefore suffers no embarrassment nor offers any apologies when he suggests that even remote resemblances are historically related, for he quite conveniently regards them as the surviving common components of what previously had been identical.

Rivet, on the other hand, considers the Fuegian similarities to be the direct results of a movement of Australians from their historic habitat, Australia, to Tierra del Fuego. He has proposed two routes of migration, the first via the islands of Melanesia and Polynesia 3,000 years ago, a view recently abandoned in favor of the second, via Antarctica 6,000 years ago. Rivet conveniently meets the difficulty presented by the complete lack of seagoing craft in Australia by endowing gratuitously the ancient Australians with craft capable of meeting all the exigencies of such a journey regardless of what these may have been. It is obvious that such a claim, based entirely upon fancy and without the slightest fact to support it, cannot be answered with specific data. Nor does Rivet seem to feel that the burden of proof for this contention rests upon himself. But he never has

permitted practical difficulties to stand in the way of theories of trans-oceanic migrations and has advocated for years such movements by various Oceanic peoples. Convinced of the validity of the latter, apparently it was an easy matter to apply the same reasoning to the more primitive Australians.

The theory that Australians have migrated to Tierra del Fuego is based upon three types of evidence: physical, linguistic, and cultural. Not only is it maintained that the Australoid features of the Fuegians are the result of Australian admixture with Indians, but all Australoid appearances in South America are attributed to this invasion. At least Rivet in 1908 submitted evidence, accepted by Hrdlička as convincing, which showed that the Australoid Lagoa Santa type is represented in the ancient and modern populations from Ecuador and Brazil to Tierra del Fuego. However, the age of the prehistoric finds never has been satisfactorily determined. One wonders what explanation would be offered if it should be found that Australoid remains were present in this general area several thousands of years prior to 4,000 B.C. Rivet also ignores the presence of Australoid features in North America and northeastern Siberia, used by Koppers to strengthen his own argument, but stresses the appearances in South America, Australia, southern Asia, and South Africa which he contends fit into his particular scheme.

For linguistic support Rivet offers his theory of Tson-Australian relationship which he seems convinced is impeccable. Nevertheless the conclusions expressed are not shared by other competent scholars, who are critical not only of this specific case, but also of the basic methodology employed.²

It would appear, therefore, that the final word has not been written for either the physical or linguistic evidence. I therefore leave their consideration to the specialists in those fields. But even though it should become commonly accepted that there are some specific physical and linguistic similarities between Tierra del Fuego and Australia, these criteria in themselves in no way would substantiate the contention that Australians came to Fuegia by sea, for an overland migration such as proposed by Koppers always will be an alternative. Unconvincing as the physical and linguistic material presented may be to the theories expressed, the cultural evidence is even less acceptable. However, before we consider the problem of trans-Pacific relationship let us first acquaint ourselves with Rivet's method of evaluating and interpreting cultural data in respect to the Australians.

² Dvor, *The Building of Culture*, J. A. Mason, *Australoid Type in America*

To support further his claims Rivet adduces a formerly much wider distribution of the Australians. Such a distribution is inferred on both skeletal and cultural grounds, but not only are general issues of race and culture greatly confused, but the implication also is made that all members of the Australoid race were characterized by cultural unity and that this culture was basically the same as that found in *modern* Australia. It is thus assumed that the Australoid remains recovered in Java (Wadjak), southeastern Asia (Lang-Cuom, Tonkin), and India indicate the former presence in these regions of Australians and Australian culture. Certainly we can agree that the ancestors of the Australians came from Asia and that they are related ultimately to other Australoids, but, if we may draw an analogy by comparison with the other major races characterized by cultural diversity, the inference that the Australoids possessed a single culture can be considered plausible only after substantiating evidence, lacking at present, has been furnished. We also can admit that some of the traits in modern Australian culture undoubtedly are holdovers from the cultures brought in by the Tasmanians, incidently not distinguished by Rivet, and by the Australians, but just which traits may be involved is by no means easy to determine. Such a problem can be solved, at best only partially, only after very careful detailed studies have been made ethnologically, ethnographically, and archaeologically. Naturally we must exclude those traits which have diffused into Australia from other cultures and those which have evolved in Australia since the early invaders arrived. If such a classification of Australian traits eventually is satisfactorily made, the residuum could then be regarded as part of the culture of the early migrants, for it never will be possible to reconstruct this ancient culture in toto. However, in such an event it would not necessarily follow that all of these ancient traits came from Asia, for some possibly had been developed or otherwise acquired en route in the East Indies.

The cultural evidence by which is inferred the former presence of Australians in India, the Celebes, the Admiralty Islands, Espiritu Santo, and New Zealand includes three traits: petroglyphs, boomerangs, and stone spearheads. However, only petroglyphs appear to qualify from the point of view of antiquity, but even so, satisfactory identification of similarity has not been made.² In respect to boomerangs available evidence indicates

² Petroglyphs in the Benares region are said by Rivet to be identical with those in Australia, but he does not specify with which of the latter there is identity. There are many styles of rock carvings in Australia and also several techniques by which they were produced. However, the chronological relationships have not yet been clearly defined (see Davidson, *Aboriginal Australian and Tasmanian Rock Carvings and Paintings*). The figures which are most wide-

an indigenous origin in Australia.³ Lastly the Australian use of stone spear-heads appears to be not only very recent but possibly also of indigenous origin.⁴ On the basis of the claims presented by Rivet it thus appears that

spread in distribution and which, therefore, may be considered as most typical are either the outline and foot-track patterns of emu or kangaroo, fauna lacking in India, or such simple geometric forms as are found on all continents that they in themselves cannot be accepted as valid evidence of historical relationship between India and Australia. In addition there are numerous regional styles, apparently local elaborations of basic Australian patterns. The suspicion that petroglyphs enjoy a considerable antiquity in Australia appears to be warranted by their appearance in Tasmania, but it thus would seem that if they are to be associated with any physical type it should be with the Tasmanian rather than with the Australian as claimed by Rivet. It would come as no surprise if the origin of petroglyphs for this part of the world is traced eventually to the Asiatic continent (not necessarily India), but such a derivation should be demonstrated by a continuity of *the most ancient* styles or designs in Australia and the intervening regions rather than upon general theories of origins and the appearances of simple similarities also present in various other parts of the world.

The word boomerang has been used so loosely by various writers that in its popular sense it implies no more than a simple missile-stick of which there are many different forms in various parts of the world. Partly because they have been given the same convenient but misleading name and partly as the result of general assumptions which have taken a historical relationship for granted, the individual differences in form or function and other facts not in accord with the idea of a unitary origin have been minimized by Rivet or entirely ignored. Rivet accepts a common origin for the so-called boomerangs of India, the Celebes, Espiritu Santo, and other areas, and the boomerangs of Australia, but does not treat the many facts from these areas as well as those associated with the objects themselves which do not support such a conclusion.

The most important evidence, however, comes from Australia itself. Boomerangs are lacking in Tasmania, and thus it would seem do not have the same antiquity as petroglyphs with which they are historically associated by Rivet. They also are not of universal appearance in Australia, a fact which does not support the claim that they were a possession of early invaders. Furthermore the most important areas in which they are lacking, the Kimberly district, northern North Australia, and the Cape York Peninsula, are the very areas to which migrants from the north would have come. Lastly, it cannot be maintained that boomerangs were formerly present in these regions, for they are now diffusing or being traded into them from the south. For a discussion see Davidson *Is the Boomerang Oriental? Australian Throwing Stick, Throwing Club, and Boomerangs*. All available evidence seems to indicate that boomerangs have been developed in Australia by the Australians and that their resemblances to objects in other parts of the world are fortuitous.

³ The Australian quartzite spearheads submitted by Rivet as historically related to the similarly shaped and used obsidian blades of the Admiralty Islands occupy a limited distribution, centered inland, which seems to have spread recently to the coast. Diffusion and trade are still carrying these blades from western North Australia to the south and east and, it is important to note, to the northern coast in all of which regions they are not yet manufactured in spite of the presence of suitable stone. These objects have not appeared archaeologically in the few coastal shellheaps excavated, but are abundant in the upper levels of inland sites in areas where they are still manufactured. It appears, therefore, that this type of artifact has but recently reached the coast from the interior, and unless this conclusion is reversed

We have no reason to suspect that the Australians ever left Australia or participated in any expansion program in the Pacific or that their ancestors ever occupied any Pacific area east of New Guinea. Indeed there is still no cultural proof that they passed through the East Indies, but only the generally admitted assumption that they came originally from Asia via this route. It would seem therefore that Rivet's attempt to bolster his argument for a trans-Pacific migration by inferring maritime activities of Australians in nearby waters has not been successful, and this matter should be kept in mind in our consideration of his major thesis, that the Australians reached South America.

The traits appearing in Tierra del Fuego which we are asked to believe were introduced by Australians are discussed below. Rivet calls attention to the resemblances noted by Graebner, Schmidt, and Koppers as substantiating evidence for his claims, although he does not mention these traits by name nor comment on the arguments advanced by Koppers to show their derivation from Asia via Bering Strait.

Beehive Huts. The term beehive hut has been employed by many writers as a convenient catch-phrase for dome-like dwellings with variously constructed frame-works covered with grass, skins, bark, earth, or other materials. Although it seems obvious that such huts may have nothing in common other than a simple hemispherical form, the numerous differences in various parts of the world have been minimized or disregarded, and theories of historical relationship have been advanced solely on the similarity of shape.⁵

If facts secured by more extensive excavations, an indigenous origin seems indicated. On the basis of present ethnological and archaeological data we cannot assign these blades any great antiquity in Australia, nor can we support the contention of historical relationship with the Admiralty appearances, 2,500 miles distant by water. At the moment available evidence suggests an indigenous development. If it could be shown that these blades diffused inland in North Australia, their limited distribution and apparently slight antiquity might be interpreted as evidence of a visit of Melanesians or of some unknown people to coastal North Australia, but they would offer no support to Rivet's contention that Australians *equipped with these blades* formerly occupied a greater area in the western Pacific.

⁵ Assuming a unitary origin, it is an easy matter to argue that details of construction and the materials used have been changed to suit the different needs of local conditions of climate, flora, and culture. Nevertheless, there seems to be no reasonable explanation as to why the same general simplicity in form should persist nor has anyone offered satisfactory evidence why a simple hemispherical shape should be regarded as so unusual that it could not have been developed independently by various peoples. The writer makes no claim that the many appearances in the world of the so-called beehive huts represent the results of numerous independent developments, but in view of no proof to the contrary, the possibility that some appearances may be independent attainments should be recognized.

The claim that the dome-shaped dwellings of Fuegia have been derived from the "beehive-shaped" Australian huts is not supported by any tangible evidence. Aside from the general simplicity in form there is no satisfactory basis for comparison. The Fuegians' dwellings are covered with skins, the Australians' with grass, bark, or earth. All these materials are available in both areas. In addition, it should be noted that if it is permissible to assume historical unity in spite of these differences, then it is equally reasonable to believe that the Fuegian huts are historically related to the other types of "beehive" huts in the Americas with which, if there is not greater similarity, there are no greater differences. In this respect it is argued by Koppers, following the principles of identification of Graebner and Schmidt, that the beehive huts of Tierra del Fuego and Australia have been derived from a common source in Asia, and in support of this contention the various circum-Pacific appearances are cited. Although we still object to this conclusion as unsupported by reasonable proof based upon fairly comparable data, it would seem that if one insists upon historical unity based upon hemispherical form, it is much less unreasonable to posit such a contention upon the sporadic distribution in the circum-Pacific areas than upon a theory of trans-Pacific migration of Australians.

Skin Mantles. The skin mantles of southern South America and of Tasmania and southern Australia are of such simple manufacture that it would seem that they might be invented independently by any hunting people who desire protection from cold and rain or snow.⁹ It is interesting to note that Koppers, who seizes every possible similarity or distant resemblance in Fuegian and Australian cultures in his efforts to show historical unity, rejects skin mantles as of too questionable a nature to serve his purposes.

Half-hitch Coiled Basketry. This type of basketry presumably is implied by Rivet's phrase *la technique du tressage au cordon*. Of the various traits listed this seems to be the only one with characteristics sufficiently specific

⁹ The Australian mantles are not the only robes with which a historical relationship with the Fuegian cloak has been sought. Lothrop has put forth a claim in behalf of the pelican-skin mantles of the Seri of the Gulf of California. Although this theory seems equally far-fetched and unsupported by pertinent evidence, it must be admitted that if a choice had to be made, it is the more reasonable in view of the land connection between the two regions. At any rate, Rivet is thus faced with a dilemma, for if it can be admitted that the Seri robes are the results of independent origin, a similar reasoning holds equally well for the Fuegian and Australian counterparts. On the other hand, if it is denied that such simple garments could have been invented independently by either of the latter peoples, it follows that the claim of historical relationship between the Seri robes and either of the others is equally valid, and such an admission would support the Bering Strait rather than the trans-Pacific route of diffusion or migration.

to permit a fair and definite comparison. Although it cannot be granted that a theory of unitary origin is the only reasonable explanation,⁷ it can be admitted that such a theory is plausible. It does not follow, however, that such an admission supports the contention that Australians are responsible for the presence of this technique in Tierra del Fuego, for the Bering Strait route as maintained by Koppers seems not only more reasonable on general principles of early human movements but is also supported by the presence of the same technique in Japan.⁸ Furthermore, we cannot be certain as to the antiquity of coiled baskets in Australia. Their sporadic distribution in eastern Australia suggests that they may represent an old diffusion from New Guinea, but not necessarily one of great antiquity.⁹ At least we have no right to take an antiquity of 6,000 years for granted.

Bark Canoes of Several Pieces It has been admitted by Rivet in his earlier studies that none of the types of watercraft found historically in either Australia or Tierra del Fuego could have been employed in a trans-oceanic migration. He therefore was forced to suppose that some superior sea-going craft had been known to the Australians some 3,000 years ago, and that for reasons unexplained it was discarded subsequently, both by the Australians who remained at home and the Australians who made the hypothetical journey to South America. For these assumptions there is not a particle of evidence. Not only are there no data from either area which would even hint that there ever had been present any craft superior to those found in historic times, but a theory of degeneration, with its insinuation that both peoples had lost interest in the sea, is sheer speculation.¹⁰ What the interests of the Australians may have been some 3,000 years ago of course is impossible to say, but the modern aborigines, far from turning from the sea, have shown a most eager interest to acquire more advanced types of watercraft, and, if we may judge from the distribution of types of watercraft in Australia and the known directions of diffusion during the past century or more, it appears that the same conditions have prevailed at least for many hundreds of years.¹¹ All facts indicate that Australian

⁷ The technique involves two simple elements, multi-strand coils and half-hitching, which conceivably could have been combined independently in two or more regions

⁸ O. T. Mason, *Primitive Travel and Transportation*, p. 482

⁹ Davidson, *Australian Netting and Basketry Techniques*.

¹⁰ Both Cooper and Lothrop have demonstrated that the Fuegians exhibit no evidence of having degenerated as manufacturers of watercraft but, on the contrary, have indicated a readiness to adopt more advanced types and to expand their maritime activities as influences from the north have come to them

¹¹ We have an abundance of information to show the rapid diffusion during the past century of three types of Melanesian outrigger canoes down the coast of the Cape York Peninsula

watercraft have been becoming gradually more and more complex, and there is no evidence to the contrary.

Although we can give no date for the original appearance of the sewn bark canoe, its use was confined to the coasts of Queensland and North Australia, and there are reasons for believing that it may have developed in the Cape York Peninsula from a cruder bark canoe as the indirect result of the introduction of Melanesian maritime activities. Since it is generally supposed that the Melanesians have occupied the southeastern coast of New Guinea in relatively recent times, the influences which may have contributed to the development of the sewn bark canoe possibly came not earlier than several hundred years ago.

It also should be pointed out that the sewn bark canoes of Australia are far from being identical with those of Fuegia.¹²

In his more recent studies Rivet has abandoned the theory of a central trans-Pacific migration in favor of an Antarctic route supposed to have been traversed some 6,000 years ago and assumes that the sewn bark canoes were in use even at that early time, although it is not stated specifically whether the alleged journey is supposed to have been made in them or whether they were carried along in some hypothetical more substantial type of craft. In his theory that the primitive Australians could have reached the New World (and incidentally also South Africa) by following the Antarctic continent, Rivet adopts the suggestion of Correa that there was a recession in glacial conditions at that time which made this region less inhospitable than today.

That the climate of the southern hemisphere was not quite so cold as today during the closing period of postglacial times (about 4,000 years ago as estimated by Swedish scholars) is supportable by some evidence from South America and Australia. However, I have been informed by Dr Skottsberg that, on the basis of recent investigations by Swedish geologists in Patagonia and Tierra del Fuego, he knows of no evidence which would indicate that such changes would affect "to any extent worth mentioning"

and of Malay dugouts along the coast of South Australia and Western Australia. All appear to be of historic or of very recent introduction. Each seems to have been taken over with velerity by all Australians who had the opportunity to acquire them.

Taking all known factors into consideration, it appears that there has been a continuous recent movement of watercraft down the eastern coast of Australia, each type of craft being slightly more complex than its predecessor, each being eagerly adopted by the aborigines of the various local regions. See Davidson, *Chronology of Australian Watercraft*.

¹² As Lottrop has shown, the Fuegian canoes have numerous affinities in style with various other craft in southern South America. This evidence would seem to indicate that they may represent the local elaboration of some basic South American type.

the Antarctic ice sheet. Indeed the available data suggest that 6,000 years ago, about the middle of postglacial times, conditions in Antarctica were more polar than today.¹³ Thus competent opinion does not substantiate Rivet's theory that the climate of Antarctica was fundamentally different during recent geological periods, and Rivet admits that Australians and Fuegians could not live in that region under existing climatic conditions.

How cold a climate human beings equipped with Australian or Fuegian material culture could inhabit is a moot question, but it cannot be doubted that they could acclimatize themselves to temperatures somewhat colder than those in modern Australia and Tierra del Fuego. Nevertheless, the differences between the modern climates of these regions and Antarctica are too great to permit the assumption that minor variations would suffice to make the latter fit for human occupation.¹⁴ Furthermore, aside from the question of how cold a climate could be inhabited by Australians, it would seem necessary on the basis of cultural considerations to postulate a major change in mean annual temperature in Antarctica to at least freezing (32°F; 0°C), an increase of between 25°F (13.9°C) and 45°F (25°C), for since Rivet assumes that the cultural possessions of the hypothetical Australian wanderers included sewn bark canoes, and possibly other craft, and beehive huts, allowance must be made for the building of huts and new craft and the repair of old. Such activities presuppose the presence of substantial trees, and if we may judge by temperatures in other frigid regions, a tree flora would require a mean annual temperature at least between 32° and 40°F (0° and 4.44°C) or more, depending upon the size of tree. Furthermore, it also seems clear that even granted such a change in climate many thousands of years would have to elapse before the polar flora could evolve into substantial trees capable of producing satisfactory bark and wood for canoes and huts. In addition there would be the requirement of fuel, so important to the Fuegians and Australians who know nothing of animal oils for this purpose.

¹³ Correspondence with Dr C. Skottsberg, August 20, 1935.

¹⁴ It is important to note that the mean annual temperature of Fuegia, popularly regarded as so inhospitable, is 43°F (6.11°C) or only 7.3°F (4.05°C) less than that of Paris (50.3°F; 10.2°C); whereas that of Antarctica ranges from 7°F (-13.0°C) at Cape Adair to -12.7°F (-24.8°C) at Little America, or from 36°F (20°C) to 55.7°F (30.94°C) colder than Fuegia. Furthermore it is only at Ushuaia in Fuegia that a temperature below zero Fahrenheit (-6°F; -21.1°C) has been recorded. The winter extremes at Punta Arenas and Evangelista Island are 11°F (-11.7°C) and 25°F (-3.89°C) respectively. Thus the coldest weather experienced by the Fuegians ranges from 37°F (20.6°C) to 97°F (34°C) warmer than the recorded extremes at Cape Adair (-43°F; -41.7°C) and Little America (-72°F; -57.8°C) respectively. See Brooks, *Climate*; Gould, *Little America*.

Lastly, even though we were willing to grant that Australians might survive in Antarctica, it by no means follows that they could have reached it. Rivet calls our attention to the various islands¹⁵ which he maintains could have served as convenient resting places, but he fails to give due consideration to the practical difficulties involved in reaching them or to the great distances which separate them, distances which surpass the known limits of navigation of all primitive peoples, including even the early Norsemen but excluding the Polynesians.¹⁶ That any group of primitive navigators in any type of early craft, and even with the will to explore, could or would push on through hundreds of miles of rough perilous seas, into constantly colder weather, to navigate successfully the Roaring Forties, through which modern steamers find headway difficult, and find the only bits of land in thousands of miles of ocean, seems beyond even the realm of fantasy. In addition we may wonder how such navigators could have missed completely nearby Tasmania and southeastern Australia, for if they called there it should be explained why the local inhabitants, in recent times so anxious to obtain improved watercraft, ignored entirely their opportunity to acquire sewn bark canoes.

The theory of Koppers in our estimation is quite plausible in some of its more generalized concepts, but not in others. Certainly everyone will agree that the various ancestors of the Australians and the American Indians hail from Asia. It follows, therefore, that if some early migrating Australoid population *with cultural unity* broke into two groups, one going to Australia, the other to America, and that they were subsequently followed by other peoples with different cultures who succeeded them and their culture in the nearer regions, that the places, if any, where we should expect the surviving primitive culture to remain would be in the peripheral areas such as Fuegia and Australia and in various culs-de-sac along the migration routes. However, it does not follow that the finding of similarity of culture in the two peripheral regions necessarily proves the original assumptions of racial unity of the migrating peoples or of unity of their original cultures. Diffusion of culture among ancient as well as among modern populations must be admitted as a possibility, and we must also

¹⁵ The Antarctic Archipelago is some 500 miles from Cape Horn, a considerable distance to traverse even in warm tropical waters. Much greater distances are found in the Australian quadrant. Macquarie Island lies some 1,000 miles from Tasmania (where presumably the hypothetical ancestors did not embark, since the Tasmanians had neither sewn bark canoes nor half-bird hauled baskets). The next nearest land is the Bellamy Islands, adjacent to Antarctica, 800 miles from Macquarie Island.

¹⁶ See Dixon, *Long Voyages of the Polynesians*.

allow for local developments in the culture of each region as the result of environmental conditions and the tendency of peoples over a period of time to produce new elements in their cultures. It cannot be admitted, therefore, that similarities in modern cultures of peoples who show *some* Australoid characteristics necessarily imply that all Australoids possessed the same culture. Nor can it be allowed that the presence of *some general resemblances* in two modern widely-separated cultures necessarily certifies that the ancient cultures of their ancestors were alike in toto and derived from a common source.

These remarks should not be construed to mean that we rule out the possibility of some ultimate historical relationship in Asia between some culture traits of Fuegia and Australia. However, it appears that Koppers, following Graebner and Schmidt, has been given to suggesting historical relationship in some most unwarranted instances. He rightfully excludes from consideration as doubtful such general traits as may be the result of similar economic influences in primitive hunting, fishing, and food collecting, but includes various other traits, also simple and showing only superficial resemblances, which to him suggest a common origin. Furthermore, he admits that many of these traits are questionable, although he does not specify which, and maintains that if a few were to be eliminated as the result of objections raised, there still would be a large number remaining and that these would prove his argument. But this does not follow, for a theory of unity based upon superficial resemblances or implied resemblances denoted by generalized terms by no means is strengthened by the number of questionables or uncertainties. Of the traits listed half-hitch coiled basketry¹⁷ seems to be the only one with fairly comparable features. All the others are so simple, so general, or so ambiguous in meaning that there is no satisfactory basis for comparison. These include Mousterianoid stone implements,¹⁸

¹⁷ Although there is a possibility that there may have been independent development in two separated localities, the theory of historical relationship with a derivation from Asia, where this technique is found in Japan, seems reasonable as a working hypothesis.

¹⁸ That certain Australian and Fuegian stone artifacts can be described as Mousterianoid can be granted. However, it must be kept in mind that we are concerned here with very simple objects treated in very simple fashion. In addition our knowledge of the archaeology of both Australia and the Americas has not reached the point where we can say that the type of objects in question was possessed by the original invaders of each region. Indeed the most ancient lithic remains so far discovered in North America, as shown by Howard, cannot be classified as Mousterian-like. For these reasons we must still admit the possibility of local development of Mousterian-like artifacts in South America or Australia or in both regions.

The proof that both appearances had been derived from Asia, however, could not be considered as satisfactory evidence to support the broader contention that Fuegian and Australian cultures are of unitary origin. Mousterian industry apparently antedates the oldest

beehive huts, fire-drill,¹⁹ lack of a hafted ax (*sic*),²⁰ prevalence of monogamy,²¹ lack of totemic clans, lack of matrilineal class exogamy, exogamy based on blood relationship,²² food taboos,²³ animal dances for entertainment,²⁴ initiation ceremonies,²⁵ and clubs.²⁶ It seems quite apparent that these traits

known remains of *Homo Sapiens* by a considerable period of time and thus Mousterianoid objects or techniques could have been a common heritage of the direct ancestors of all modern races. As such, present Mousterianoid appearances may indicate merely that certain modern peoples have retained a common ancestral possession relinquished by others, and such retentions in themselves would prove nothing in respect to the historical relationship of other elements in their cultures

¹⁹ Here we have another simple trait, possibly of considerable antiquity, but so widely distributed that it seems of very questionable worth for the problem at hand.

²⁰ It is argued that the lack of a hafted ax in both Fuegia and southeastern Australia is responsible for the manufacture of bark canoes rather than of dugouts or plank boats. However, the hafted ax is not lacking in southeastern Australia. How long it has been there seems beside the point, for its presence has not caused any important change in the local crude bark canoes which possibly it may antedate.

²¹ Monogamy *prevails* in most human societies. Polygamy usually is either definitely prohibited or legally permitted, but unless there is a very abnormal sex ratio or peculiar conditions which cause a large number of males to remain bachelors, polygamy could not *prevail* in any society. Monogamy as an institution may be the result of legal restrictions, religious influences, personal choice, or the product of various social systems. To draw conclusions as to historical development by comparing the similar results rather than the specific causes producing them seems a worthless procedure. Unless carefully qualified, "monogamy" should be considered another loose term of no comparative value.

In respect to polygamy, Koppers recognizes that it may result from different causes. Although he concludes that Fuegian and "Old" Australian cultures are related because both are said to be predominantly monogamous, he also adds that polygamy in each region is not so rare. Polygamy in Fuegia is explained as the entering influence of the Matrilineal *Kulturkreis*, but in southeastern Australia it is ascribed to the Totemistic *Kulturkreis*. Since polygamy is thus attributed to differing influences, the appearances are not accepted by Koppers as of historical value in the problem at hand. In our opinion monogamy should be similarly excluded.

²² All societies are characterized by exogamy (another loosely employed term) based upon blood relationship in one sense or another.

²³ The complex Australian system of food taboos is well known. Some taboos are lifted after initiation, but in some areas all food bans may not be removed until a person reaches middle age. It is with this intricate system that Koppers compares the Yahgan custom whereby an initiate is not free to partake of food until he has distributed food to the gathering. It seems obvious that the two practices are not reasonably comparable. Among the Yahgan the emphasis is not placed upon a prohibition, but is concerned with a positive act, the distribution of food and the etiquette of first serving the others present.

²⁴ Here we have another ambiguous term applicable to performances of diverse nature resulting from various stimuli and found in various parts of the world.

²⁵ Great stress is placed upon the initiation ceremonies of the Yahgan and Kurnai, and it is held that historical relationship is indicated because the former reputedly *demand* at all

either are so simple that they might develop almost anywhere as the result of similar or of different psychological forces, so ambiguous that they are meaningless for comparative purposes, or of a negative character, hence not comparable for positive identifications.

Other traits of a simple nature or designated by loosely employed terms and commonly distributed in Australia and Fuegia could have been included by Koppers.²⁷ Still other traits of Fuegia have counterparts occupying restricted distributions in Australia.²⁸ Some of these quite obviously are of relatively recent introduction from New Guinea, and it must be recognized similarly that diffusion also may be responsible for the appearances in Australia and Fuegia of many of the other traits, which as the result of their greater age, now appear as well integrated elements difficult or impossible to distinguish in antiquity from still older traits. The diffusion of great

times equal treatment of boys and girls, whereas the latter *permit* the girls to participate slightly only in a small part of the preparatory performances! Koppers also believes that a historical relationship is indicated because the initiates of both tribes are exhorted to be altruistic and kind, to be peaceful, to respect old age and foreign women, to acquire self-control and fortitude. But surely such good advice tends to be universal, if not in association with formal initiation in tribes or religious groups, then as normal admonitions of the home or family. There seems to be no reason for doubting that psychological incentives might produce a set of similar instructions in any human society. Similarly we are asked to believe that the religious sanction given these ceremonies and their control by an All-father indicate a unitary origin. However, it seems hardly necessary to point out that religion generally is called upon to bless and to lend authority to all important ceremonies in all parts of the world, and that in those cultures where an All-father concept is present, it is customary to conceive of such a deity as the director of human activities.

²⁶ The striking-clubs of the Yahgan and of most Australian tribes and the Yahgan ceremonial clubs and Australian women's digging-sticks, respectively, are of such a plain and simple nature that they furnish us no specific basis for comparison. There seems to be no reason for suspecting that such objects could not have been developed independently by any people or that they could not be a common heritage of all existing races.

Koppers postulates a unitary origin for the simple ceremonial clubs used by the Yahgan men at funerals and the ordinary digging-stick of the Australian women. Since the ceremonial club differs only slightly from the ordinary Yahgan club, there seems to be no reason at the moment for regarding one as more than a variant of the other. The origin of the Australian digging-stick is obscure. It may have developed as a digging-stick or as a weapon. At least it now serves both functions. It was not present in Tasmania.

²⁷ Bone awls, simple spears, abortion, infanticide, spear-throwing as a game, medicine-men and explanatory myths. Although in our estimation their presence in the two regions indicates nothing of historical value, it would seem that they are no less valid for such an interpretation than those offered by Koppers.

²⁸ Bark buckets (Kimberley coast, see Davidson, *Transportation and Receptacles in Australia*), two- and four-pronged fish spears (eastern Australia), harpoons (northeastern coasts), masks (Cape York Peninsula).

numbers of traits in southern South America has been well worked out by Cooper. For Australia the problem has been only partially treated.

On the basis of the foregoing discussion we have failed to find any satisfactory evidence which would suggest that the basic cultures of Tierra del Fuego and Australia are historically related. Of the many hundreds of traits in the two cultures the proposers of historical unity have been able to muster only a few which they regard as similar, and these for the most part are cloaked in such ambiguous and generalized terms that the comparisons appear to be meaningless. If it is permissible to infer historical relationships on the basis of such loosely used terms and on the presence of such simple customs and objects, it apparently would be possible to imply relationship for any two cultures selected at random. Such is not our understanding of "historically related."

That the two regions may contain a few similar traits, each derived from a common source in Asia, should be not unexpected in view of the many migrations from that continent, but we must also take into consideration the possibility that subsequent diffusions may be responsible for the modern appearances of such similarities as are established on the grounds of specific characteristics and not upon generalized and loosely used terms. Indeed, we can still agree in principle with Koppers' theory of route of derivation and maintain at the same time that satisfactory evidence in support of it has not yet been presented.

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THE following account describes an actual journey for salt that was made in 1912 in prescribed, orthodox fashion. Only three men participated but the account is complete because they represented the three ranks customarily found on such journeys; one man acting as chief (*mongwi*) of the party, another being an experienced but "common" member, and the third a novice making his first trip. The chief was Talasvuyauoma of the Coyote clan, village War Chief (*kaletaka*) at Oraibi; the "common" man was Duvenimptiwa of the Sand clan; and the novice was Don Talayesva, his twenty-two year old son, a member of the Sun clan and narrator of the events here recorded. All three men were Wuwutcim initiates, a requirement that all salt-gatherers must meet.²

The main reason for a ceremonial qualification is that the deposit visited by Oraibi men is located in the vicinity of the home of the dead (*Maski*) near the Grand Canyon, and therefore lies in dangerous territory. There is a good deal of reason to believe that one of the principal aims of the Wuwutcim is to fit men for their proper places in the after-life. Hence, only those who have passed through the Wuwutcim rites are eligible to visit the home of the dead.

The patron deities in charge of salt are the Little War Twins, Pukonghöya and Palungahöya. They are said to have established all the shrines that lie along the route to the Salt Canyon and to have inaugurated the proceedings to be performed at each sacred spot. Men who expect to go for salt in any year are required to make special prayer-feathers during the preceding Soyal and to deposit them at the shrine of the Twins east of the village. Participants must also observe a tabu on sexual relationships for four days before starting on a salt journey.

In former times, when the danger of encountering enemies made it necessary for large parties to make the trip, it is likely that each household or clan sent at least one representative annually, but my informant could give me no specific information on this point. Usually, these expeditions were made in the fall, soon after the harvest was gathered.

¹ The material on which this article is based was used by the author as part of his doctoral dissertation at Harvard University. The data were obtained on a field trip made in 1933-34, financed and sponsored by the Division of Anthropology at Harvard University.

² The Wuwutcim is the Hopi tribal initiation ceremony through which all young men must pass. It is performed jointly by four societies known as Kwan, Al, Tao, and Wuwutcim. See E. C. Parsons, *The Hopi Wovvohim Ceremony in 1920* (*American Anthropologist*, Vol. 25, pp. 156-87, 1923) for a description and bibliography.

THE NARRATIVE

In 1912 the salt journey began with Don and his father driving four burros from Oraibi to the home of Talasvuyauoma in Moenkopi. Before leaving the house Don's father made a cornmeal path towards the west where the Salt Canyon is located, and on it he placed a prayer-feather with its breath line pointing in the same direction.³ This was to give them a good journey and to prevent their growing tired.

On their arrival at Moenkopi, Don and his father went to the house of their chief where they found several men present who were busy making prayer-feathers for a yellowish clay (*pavisa*) obtainable only at the "original sipapu" near Salt Canyon. Every man in the Soyal ceremony has need of a supply of this clay and asks the men who are going for salt to fetch some, "paying" for his share with these prayer-feathers which are "for the katchinas, clouds, and the dead who live in the underworld and who own *pavisa*."

The three salt-gatherers likewise prepared a prayer-feather to be placed on the road when they were setting out. This was to give them the help of the Sun against evil spirits which might be encountered near the home of the dead. Meanwhile Talasvuyauoma's wife prepared a quantity of baked sweet-corn meal (*tosi*) which was later to be made into a dough (*qömi*) and shared among the three travelers as needed.

Early the next morning the party rose, packed their burros and prepared to start out. Talasvuyauoma, in his capacity as head of the expedition, put down a prayer-feather with the breath line extending in the direction they were to take. Then he said, "Let us travel with happy hearts," whereupon each of the three men stepped with one foot and then the other on the "road-marker" and the journey was officially under way.

In a short time the party came abreast a sacred spring, but since it lay off their line of march they did not turn from the path but deposited offerings opposite the spot. They did the same thing when they passed another spring, Monavi, and not long after they arrived at a shrine called Pan'kuku (Mountain-sheep Feet)⁴ near which the Hopi used to hunt in former times. They prayed to the mountain-sheep for "power" and for success in hunting.

From Pan'kuku the party bore slightly westward, passing through a break in a wall-like ridge, and then headed north towards the shrine known as Tutuveni (Writing). Here each man was supposed to carve his clan emblem on the rocky face of the shrine, and on each successive visit to repeat

³ A prayer-feather with a breath line attached is called a *nakwakwosi*.

⁴ This is the first of the series of Salt shrines said to have been established by the War Twins. The marks are probably fossilized footprints.

the device to the left of his original "signature." Talasvuyauoma carved a fresh coyote head at the left of a line he had started many years ago. Don's father did the same with a sand hummock which was his sign, and Don, being a novice, began a fresh row by carving a sun shield as his device. Each man pounded the string of a prayer-feather into the middle of his carving and somehow managed to secure it in such fashion that the feather dangled down against the face of the rock.

The next stopping place was at Totolospi,⁵ a shrine intimately associated with the Little War Twins, where every member of a salt party must play a game with the Twins in the hope of winning from them such benefits as "a successful journey, rain, crops, and good health." According to custom, each man, beginning with the leader, manipulates the score in such fashion that he comes out the winner. He then deposits appropriate offerings and hurries on his way, leaving the others to finish their games and to catch up as best they can.

After each man had defeated the Twins at Totolospi, the small party was soon re-united and on its way to the shrine of the Salt Woman (Öng Wuhti), commonly called luwa (vagina). "Now we are coming close to luwa," said Talasvuyauoma, "and we'll soon do a good business [sex act] there."

To help pass the time as they went along Don began to tell his companions the story of his trip to the house of the dead,⁶ describing in advance some of the scenery along the road which he was now about to traverse for the first time, as proof of the fact that he had actually visited the region while he was "dead" (unconscious). In his subconscious journey he had particularly noticed a bush called mongpivi, from which arrows were made in the old days, and his accurate description deeply impressed his companions.

Soon the leader interrupted Don's narrative by announcing that he was going to fix his burro pack. To Don's surprise, Talasvuyauoma loosened the pack and took out a full-sized wedding robe (ova). With the robe in his hands he proceeded to a slight ridge from which there protruded a white rock about ten or twelve feet long, pointing south. Over the protruding rock the leader threw the wedding robe, remarking as he did so that they were going "to do a business" there. "You watch me," he said to Don,

⁵ Totolospi was a gambling game formerly played in the kivas, although it might also be played elsewhere. It was played with reed counters on a sort of checkerboard. See S. Culin, *Games of the North American Indians* (Twenty-fourth Annual Report, Bureau of American Ethnology, pp. 3-846, 1907), pp. 795-96.

⁶ While Don was at the government school in Riverside, California, not long before 1912, he had had a serious illness during which he had "died" and visited the home of the dead. Such experiences are common among the Hopi.

"then you can do the same thing." He crawled under the robe, kneeled while he deposited meal and prayer-feathers, and then proceeded to simulate the act of copulation. When he had finished he emerged from under the robe and said, "I just had a business with my aunt (ikya'a), Talavenka."⁷

It was now the turn of Don's father, who did exactly as Talasvuyauoma had done, naming his paternal aunt Talavensi as his "mistress."

When it was Don's turn he felt like running away as it did not seem right for him to engage in a sex adventure with his own father and an older man like Talasvuyauoma, but the leader commanded Don as a *kele* (novice in the Wuwutcim and other ceremonies; literally, "chicken-hawk") not only to perform as the others had done, but actually to strip naked as this was a requirement imposed on all men who were making salt trips for the first time. "If you don't do as you are told, our journey might be pretty hard," said Don's father.

Urged in this fashion Don felt encouraged to proceed, and even made a jest about never before having done a "business" with a "private" lady. When he got under the robe he noticed for the first time a smooth, vulva-shaped opening cleft in a hard black rock which was imbedded in soft, white sandstone. The sight convinced him that his elders were correct in ascribing such a "miraculous" formation to the War Twins. Into the opening Don dropped his offerings, and then imitated the act of intercourse. As his partner, he named his paternal aunt, Pavinyesnim, at whose house in Moenkopi he and his father had spent the previous night.

Talasvuyauoma then re-packed the wedding-robe and, as it was getting late, the party made for a sheltered spot where salt expeditions generally spent the night. Here they made camp, but before sitting down to supper the leader began a formal smoke. He filled a pipe with native tobacco, lit it, smoked silently for a little while, then handed the pipe to Don's father. The latter puffed quietly for a few moments and then said to Talasvuyauoma, "My father." The response was, "My son." Then Don received the pipe from his father and in due time they exchanged father-son terms. After this Don passed the pipe to Talasvuyauoma, who smoked a little and then called Don by the term for son, Don replying with "father."⁸

⁷ Any woman in the class of the paternal aunt (ikya'a) may be named in this connection whether or not she has ever been the actual mistress of the man who names her. This may be one indication of a former practice of cross-cousin marriage, as the father's sister's daughter is classed with the paternal aunt.

⁸ All the terms used during the ritual smoke are based on customary kinship usages and are neither esoteric nor sacerdotal. Don's father called Talasvuyauoma "father" because he happened to be a man in the clan-group to which his true father belonged. Don called Talasvuyauoma "father" because he was married to an older woman of his own clan.

When the ritual smoke was finished, they ate supper and sat about the campfire while Don concluded the story of his visit to the dead. Before they went to bed they held another formal smoke and Don was instructed to get up once during the night to make sure that all was well.

The party rose early the next morning, breakfasted quickly, and resumed their journey. From this point on, the road was lined with cactus and full of loose pebbles which rolled under foot. While it was still early they arrived at a rounded slope where broken bits of pottery indicated a former village. Don had noticed this spot in his "dream," but at that time it had appeared to him with the houses still standing and inhabited by people who were dressed in the fashion of the Kwan society. Nearby there was a jutting rock where salt-gatherers always paused before beginning the perilous descent to Salt Canyon. As Don peered down the steep path he saw a haze floating upwards like smoke from a newly kindled fire. The fact that he was so close to the home of the dead gave him a queer thrill, and he also had a feeling of shame at the thought that perhaps the walls of the canyon were peopled by the dead who, although unseen by him, might be watching his actions and commenting on his appearance.

The older men did not seem to be particularly affected by their surroundings, and Talasvuyauoma calmly proceeded to make qomi (dough) from the sweet-corn meal that his wife had prepared. Meantime the others unpacked and hobbled the burros, and hid the saddles and other equipment in a safe place. Then Talasvuyauoma shared the dough, and they prayed to the War Twins and to the Kwans to look after their belongings while they were gone.

At the south entrance to the canyon there stood two jagged rocks which were said to be images of the Twins. To these they offered prayer-feathers and cornmeal, and on each they pasted a bit of the dough. All three men smeared their faces with red ochre (suta), and Talasvuyauoma called aloud to the Twins, "Itam iku!" (We've arrived!).

"Now we are ready to enter your house," continued the leader. "With good hearts and happy thoughts we go into your canyon. Help us, and protect us while we are gone. Lead us on your road so that our journey will be successful. Don't let any evil thing cross our path. We want to come out safely without sorrow."

When the leader had finished, Don's father made a short but similar prayer to which Don added his assent. Then Talasvuyauoma said, "Let's go!" and the descent began. About a quarter of a mile down they came to a huge rock in which steps were cut in such fashion that it took a good wide stride to pass from one to another. Appropriately enough, this place

was called Kurjipyakinpi (Spreading Buttocks). No shrine is located here.

From this point down there is no well defined path and loose pebbles are often displaced by the climbers. In such cases one must throw a pinch of dough in recompense to the dislodged stones. Whenever they came to an unexpected turn Don was warned to take careful note of the direction because, being the youngest and strongest of the three, it was very likely that he would be in the lead when they were coming back laden with salt. It was up to him, therefore, to remember the proper road.

Soon they passed by Putcdukwi (Broad Cliff), the special home of the dead from the Reed (Bakab) clan, but they merely sprinkled meal as they passed. They continued on to Patcip-ve'itaka (Fur Carvings), a solid red rock, carved by the War Twins to show the stitching seam that was used in ancient times for sewing wildcat skins into robes. (The skins were sewn with the fur in, and were daubed on the inside with red ochre. Robes of this kind were used by men for blankets or sleeping covers, so that if an unmarried girl showed red ochre stains on her clothing or heels, people would say, "I can see that you've been sleeping with your lover.")

After making the customary offerings at Patcip-ve'itaka, they went on until they arrived at Panktupatca (Mountain-sheep Upper Story). This was located on a ridge which goes on to form part of the upper rim of the Grand Canyon. It is said to be the home of mountain-sheep, and there were fresh tracks and droppings to indicate the truth of the belief. In addition to the customary offerings to be left here, each member of the party pasted a prayer-feather with a bit of dough so that it hung suspended from the rock.

The next point of interest was called Kwantupe (Agave Roasting-place) and, indeed, there was a flourishing growth of kwan plants to be seen all about.⁹ This place used to be frequented by Havasupai, and possibly Paiute, who were accustomed to gather the agave, pound it out and bake it in sheets somewhat similar to piki. There was no shrine here and no stop was made.

The road now led into another chasm where there was a gap that was just about the width that a man can straddle. Here one stands astride and rubs one's nose against a flat place in the wall which is conveniently located at the right height. In this way the rock has become stained with red ochre and is called, "Nose-scrapping Place" (yokajrukwanpi).

Not far from this spot they came to the home of chickens (koakumki-

⁹ Note that the Kwan society is peculiarly associated with death notions and that the plant called kwan is definitely associated in Hopi minds with this region, so close to the home of the dead. Note, too, the possibility of trade and other exchanges with foreign tribes who frequented this spot.

yungwa), formed by a slab of rock which slopes against a big solid stone. All about there are tracings of chickens said to have been drawn with red ochre by the War Twins. The customary offerings were made, and prayer-feathers affixed with dough, after which Talasvuyauoma said, "Here we'll crow like roosters," and after crowing lustily he called on Don and his father to follow suit in order that the gods might hear this prayer for chicken increase.

Soon a hollowed-out, cave-like spot came into view and Don was told that here lived the Nukpana (Wicked or Evil One), a reference to Masau'u, the dread deity who is in charge of the house of the dead. One man must venture into the cave of Nukpana each time that an expedition goes by. He must be a warrior (kaletaka) or else a member of the Coyote-Masau'u-Kokop group of clans, closely affiliated with war.

Talasvuyauoma, being of the Coyote clan and a War Chief to boot, was well qualified to perform the necessary rites in the cave of Masau'u. From each member of the party he collected feathers and corn meal, and while the others went on ahead he entered the dread place. Here there is a milling stone, exactly like those to be found in all Hopi houses, and the intruder looks it over very carefully for omens. If fresh food from recently gathered crops is seen, it is a *bad* omen, but if Masau'u seems to have been grinding old corn then it is a *favorable* sign and means good crops for the next season.¹⁰ At the same time the observer must note whether or not the occupant of the cave has stored up a large amount of fuel and whether or not there is a good supply of corn on hand, for if Masau'u seems well fortified in both respects it means that the Hopi will have a hard winter and a poor yield from their farms.

While Talasvuyauoma was in the cave, Don and his father continued on towards the home of the Koyemsi, known as Tataticktumuyi kiamu epe (Ball-headed Ones, House at). This "home" is a reddish wall whose color resembles that with which Koyemsi impersonators daub themselves. All about it too are many rounded stones resembling the knobs on the Koyemsi masks. Prayer offerings were placed within a cleft in the wall.

A sandy slope leads from the home of the Koyemsi to the banks of the Little Colorado River where the leader, followed by the others, deposits offerings at any spot he chooses along the bank. Then they follow the stream towards the juncture with the Colorado, and soon they reach Sakwa-onga (Blue Salt) where a blue-tinted salt is obtainable, and Hawi-onga

¹⁰ Note here the tendency for Masau'u to do things in opposite ways from human beings. There seems to be a definite feeling among the Hopi that as death is the antithesis of life so the god of death is the antithesis to living man.

(Going-down Salt), another deposit. Although these places are easily accessible, no parties load up here because the salt is said to be less tasty than that of the main gathering ground. Even though nothing is taken from these saltbeds, offerings are left by all parties which pass.

It was not long now before the expedition found itself approaching the Kiva, the original sipapu through which mankind emerged from the underworld. Its outlines are indicated by soft, damp earth and an outer circle of bushes called pilakho (plur., pilakhotcoki). (Stems of this tough bush serve as the male sticks, i.e. the firedrills, when new fire is kindled in the kivas during the Wuwutcim ceremony in November.)¹¹

Pushing their way through the fringe of vegetation, the party stepped into the inner ring within which the Kiva is located.¹² The sipapu is full to the brim with yellowish water, of about the same coloring as the surrounding earth, which serves as a "lid" so that ordinary humans may not see the wonderful things going on beneath the surface. The water was calm as they approached, but when Talasvuyauoma shouted, "Itam iku!" (We've arrived!) it began to "boil" as if in response.

Then the chief made a corn meal path leading to the east, on which he placed a blue prayer-stick (paho) and a prayer-feather, and six inches off he put a second set of offerings. This is the main path for clouds to travel when they emerge from the sipapu to bring rain. They rise at this spot, look towards the east, and go to the most worthy people who are summoning them.¹³

When all had prayed at this very sacred spot, they went a slight distance to the north to gather the yellow clay (pavisa) for the Soyul men. At first there was a little difficulty in locating the clay deposit, but at last Talasvuyauoma found that it had been filled in with drifting sand. At once they cleared out all foreign matter and soon disclosed a bed of dark brown, "wonderful" clay.

Then the leader called on Don, saying, "You're supposed to be the kele, so it's up to you to get it out. You're supposed to be all naked but since you have behaved good all the way, we won't make you undress. Just take off your shirt."

¹¹ During the initiations into the Wuwutcim, fire is kindled before sunrise on one of the early days of the ceremony, by the Al (Horn) society at Orabi. Cedar bark or dry dung is used for kindling and a cedar bark torch (kopiteoki) is ignited and carried to the other kivas where new fires are lighted. Note, too, that the lances carried by the Kwan society are fashioned from the same bush, pilakho, that surrounds the sipapu.

¹² The sipapu (kiva) is a circular hole about four and a half feet in diameter situated on a little hillock.

¹³ There is a close relationship between clouds, katchinas, and the dead.

Don obeyed and then knelt before the pit, extending his left hand which Talasvuyauoma held at the wrist so that Don might not lose his balance. The older men instructed him how to go about the work and advised him to keep his mind and heart full of good wishes for himself and his people. As a young man his prayer was supposed to be more intent and concentrated than that of older people who are distracted by many cares and worries.¹⁴

Talasvuyauoma picked out a single prayer-feather at a time and handed it to Don with a bit of meal on behalf of its maker. Don inserted the offering in one side of the pit and then, reaching into the soft center, he brought out a fist full of clay for the owner of the particular prayer-feather just deposited. As he brought out each handful, his companions thanked Don and spread the damp earth on a cloth to dry.

Don kept plunging his arm deeper and deeper into the pit until he struck the end of the preferred deposit and began to bring out a harder, darker sort of clay. At this the leader decided that they had taken out a sufficient quantity, but as there were still half a dozen feathers left it was decided to club them all together and to divide the last batch of the clay into six parts.

By the time they had left the vicinity of the Kiva and had reached the junction of the Colorado and the Little Colorado the men were very tired, but they stopped only for a light lunch of wafer bread (piki), washed down by draughts from the sacred river. The leader was disturbed about their condition and decided that the fatigue was due to the violation of some rule, probably because Don had not stripped naked while bringing the pavisa clay to the surface. However, there was nothing to be done about it, so they continued until they came to the bend in the canyon where the salt deposit is located. To get down from the upper ledge on which they stood to the ridge where they planned to do their gathering, was a hard and dangerous undertaking. Clothing, packs, empty sacks, and whatever other baggage might tend to encumber them were tossed down to the lower level, after which they crawled backwards on hands and knees to a breast-shaped rock which is supposed to be an image of Pukonghoya, the elder of the War Twins. About this image a rope was securely fastened, prayer offerings were made, and one after another the men slid down the rope.

¹⁴ Compare the account given in George A. Dorsey and Henry R. Voth, *The Oraibi Soyal Ceremony*, (Field Columbian Museum, Anthropological Series, Vol. 3, No. 1, 1901), p. 20, footnote.

The actual clay fetcher is held by the leader not because of the physical danger involved in stooping over a shallow excavation, but because of the spiritual danger of coming into such close contact with the realm of the dead.

The party had now arrived at the ridge where they were to load up with salt, but there were still some rites to be performed before the actual work began. There is a natural rock basin here which is so situated that it receives a constant drip of salty, "medicine" water. Into this a stone is dipped and then sucked, and some of the medicinal water (*nakuyi*) is drunk and some is rubbed all over the body to ensure health and bravery through a sort of communion with Pukonghöya who is said to be *the* salt.

Whoever likes may take whatever *qömi* dough he has left, fashion it into the shape of little animal figures, and deposit them in the medicine bowl. They are left there for a year, and on the next annual expedition, the maker finds them converted into "stone pets" called *tohopkom*.¹⁵ These animal fetishes play a large part in Hopi ceremonialism and are part of the stock in trade of every medicine man. Nevertheless, they may be made in this way by any person regardless of his clan or ritual affiliations.

Close to the medicine bowl there is a cave which is the real home (shrine) of the Kwans. It is called *Kwantupuvi*, and inside there were formerly suspended from the ceiling two huge horns of salt, fashioned like those worn by the members of the Kwan society. About the year 1910, the river washed out the salt horns and this was considered as a bad omen which foretold the lapse of the Kwan society at Oraibi a year or two later.

As soon as the proper offerings have been deposited at the shrine of the Kwans, a party is ready to start on the task of quarrying the salt. Don began by picking freshly formed deposits as high up on the ledge as he could reach, but the others grubbed about in the sand for old salt which is said to be stronger and tastier.¹⁶

A large and a small sack were loaded by each member of the party, but as too great a load could not be carried up the steep trail, care was taken not to pack the sacks too full. Then the older men clambered up to the ledge above, leaving Don to fasten the bags to the rope which they then hauled up.

Just before he joined the others at the higher level, Don, who was very much impressed with his surroundings, expressed a wish to remain a little longer that he might explore about, but the leader reprimanded him, exclaiming, "Don't say that or the spirits may get you."

Don climbed up the rope, and loading their packs on their backs, the men proceeded as far as the juncture of the two rivers, where they spent

¹⁵ Compare Parsons and Beals, *The Sacred Clowns of the Pueblo and Mayo-Yaqui Indians* (*American Anthropologist*, Vol. 36, pp. 491-514, 1934), p. 495.

¹⁶ Whether or not they are correct about the better taste of old salt, the Hopi certainly prefer it. They use it with great gusto regardless of the admixture of sand

the night. Don did the greater part of the chores, but there was not much work since supper consisted only of wafer bread, baked sweet-corn meal (tosi), and water, for in this territory it is considered improper to eat any non-Hopi foods. That night, as they sat about the campfire, the men told Don many stories of apparitions and other wonderful things that had befallen them on previous expeditions, and Don was warned not to scan the walls of the canyon too closely lest he see some fearful sight.

As they were still tired on account of having broken the rule of stripping when gathering pavisas, it was decided to get up very early the next morning in order to avoid the heat of the sun. So, even before daybreak, they loaded up and set out for the Kiva, where Don was refused permission to visit again the awe-inspiring sipapu. They paused only to gather up the clay which they had left to dry, and then began the long and difficult ascent.

It was the custom that a warrior (kaletaka), or a member of the Kwan society, or a man from the Coyote-Masau'u-Kokop clan-group should be the first going down the trail to Salt Canyon and the last of the party coming up, as these people were best qualified to protect the others against evil spirits.¹⁷ Accordingly, Talasvuyauoma brought up the rear and Don led the way as the heavily laden men toiled up towards the surface. Now, for the first time, the leader was allowed to tell what signs he had seen during his visit to the cave of Nukpana, but when they actually passed the place on the way up, they were warned not to look too closely lest they see Masau'u there in person.¹⁸

At one point, despite the instructions he had received about noting the path, Don lost his way and Talasvuyauoma teased him about it, claiming that for pointing out the right road he was entitled to make free with Don's girl.

As they passed each of the shrines at which they had deposited prayer offerings on the downward journey, the men carefully examined the condition of the feathers to see how the prayer-feathers (nakwakwosi) had been received. Cheered by the fact that most of the omens were favorable, the party gradually made its way without further incident to the spot where they had left the burros and hidden their packs.

When they had rested, they loaded their packs and made for a camping place that was within an easy day's journey of Moenkopi. Here they discussed in detail what Talasvuyauoma had seen in the cave of Masau'u. Because he had noted four old corn cobs scattered about, it was decided that their next crop would be abundant, but a large supply of wood that was stored in the cave indicated a severe winter.

¹⁷ War and death notions are associated with people of the categories given here.

¹⁸ To see the real Masau'u is a premonition of impending death.

Next morning, as they were on the last stage of the return journey, they did not fail to stop at the shrine of the Öng Wuhti (Salt Woman) to pay what was due for the pleasure they have received in "copulating" with her. Don was warned to be generous in the amount of salt that he deposited within the vulva-like opening of the shrine, and he was told of an unlucky incident that once befell a party when some of the members, having vowed generous portions to the Salt Woman, changed their minds at the last moment and tried to satisfy her with niggardly gifts. As a result, their burros later were badly frightened and took flight, scattering a great part of their precious cargo before order was restored.

Late in the afternoon they arrived at Moenkopi where the people crowded about them, eager to hear all the details of the expedition. They gave an account of the journey, ate, and retired early in anticipation of the next day's trip home to Oraibi. Next morning, Don and his father bade Talasvuyauoma goodbye and started for home. At last they reached the spot where Don's father had left the road-marker that was to give them a good path to the west. They searched about until they located it, whereupon Don's father made a fresh path (this time by sprinkling meal to the east) and placed a new road-marker pointing to the village.

Then the older man made a little speech. "Now we happily enter the houses of our fathers and mothers. Let us go!"

This form of address is always used when returning from a long or dangerous journey, and not only expresses the joy of the returned speaker but also serves as an invitation to the clouds to visit the village. Hence, when his father had finished, Don had to repeat his words before they went on.

Once home, they were eagerly greeted and thanked, especially by their female relatives. Then they rested all day, and on the morrow there was much feasting on dishes which were seasoned with the recently brought salt that had caused them so much difficulty in the gathering.

An explanation of the shrines and rites connected with salt is given in the following myth.

THE FLIGHT OF THE WAR TWINS¹⁹

Long ago, while the people of Shimopovi still lived on the lower levels of the mesa, but after all the present villages were inhabited, the War Twins were living at their shrine to the north of Oraibi. On two successive days they happened to notice an unusual amount of traffic heading towards the present site of Hotevilla.

"Where are all those people going?" they asked their grandmother. "Yes," she replied, "There is going to be a dance at Blue Canyon where the Hopi are to have

¹⁹ This myth was narrated by Don's father. Compare the version given by Henry R. Voth, *Traditions of the Hopi* (Field Columbian Museum, Anthropological Series, Vol. 8, 1905), pp. 90-92.

a matikive'e (Locust dance)."²⁰ "Well," said the Twins, "we'd like to see that. We never saw that kind of a dance in our lives. Perhaps we could all go to see it." "Yes," answered the Spider Woman, "it is a good dance, one of the best that the Hopi do. It's up to you, and if you want to see it, I'll take you there and introduce you to all the people."

As the Twins were anxious to go, they made all the necessary arrangements, their grandmother rose early to fix them a good breakfast, then all three set out, stopping at Hotevilla spring for a drink and heading for Blue Canyon. As usual, to make the time pass more quickly, the Twins played shinny all the way.

It was shortly before noon when they arrived at the village which was holding the dance. The Twins enjoyed the spectacle very much but it began to grow late and they were getting very hungry, yet no one invited them to eat because they were ugly and dirty and never washed or tidied up. At last they felt as if they were about "to swallow their throats" [ravenously hungry], and as they were still not asked to dine anywhere, they began to get very angry.

Finally, as they were passing by one of the houses a girl called out and asked them to enter. The girl set food before them, but as they ate they felt that the food lacked seasoning.

At that time salt was not known to the Hopi, but the Twins themselves were salty as they were dirty and unwashed, and the only spot about them that was not black with dirt was the back of the hand with which they were in the habit of wiping their running noses. As they ate they occasionally ran the backs of their hands across their noses and then dipped their fists into the stew to season it.

"Those ugly boys are getting too mean," remarked the girl who had invited them to eat, "putting their dirty hands into the stew doesn't look good to us. If they keep that up we'll never ask them to eat here again. Any of the stew that's left over when that family is finished we'll have to throw to the dogs as no one will want to eat it."

The Twins overheard what she whispered so they hurriedly ate what they could and then returned to the plaza to watch the dance.

Towards evening they found themselves hungry again, but word of their unclean habits had got about and no one would give them an invitation. At last the Twins grew so angry that they complained to their grandmother and told her that they planned to wreak mischief on the villagers. As she was old they advised her to start out in the direction of Moenkopi as she would be readily captured in the event of a pursuit.

Then they approached the shrine in the middle of the dance plaza called Talastcoma (Pollen Hill), and from opposite directions they fired arrows right into the middle of the sacred spot. "Why do you cause harm while we are dancing for

²⁰ Apart from the fact that this was an old-fashioned dance, the narrator could say no more about this performance. Locust, however, acts very bravely in myths. Blue Canyon is west of Oraibi, in the general direction of Moenkopi and the Grand Canyon. Several ruined places in the vicinity are said to be the remains of the villages mentioned in this story.

good things to happen?" cried the people in great anger. "You are supposed to be our protectors and you ought to know better than that. We'll teach you a lesson for that."

As the crowd rushed towards them the Twins chewed some powerful medicine which they spurted all about, turning dancers, spectators, and everything in the village into stone. Then they began to run rapidly in the direction taken by the Spider Woman.

They tracked their grandmother to Moenkopi and found that she had gone on past Mauyavi spring. So they hurried on till they came to Pan'kuku, where they paused long enough to establish a shrine. "Now we are about to make a new settlement at Salt Canyon," they said, "and we are preparing the shrines that the Hopi will use when they come this way for salt."

The next stop was at Tutuveni where they put their symbols as do the Hopi at the present day, and near here they caught up with their grandmother. "Are you getting tired?" they asked. "Yes," she replied, "I've been running all the way and I'm pretty tired." "Well," argued the Twins, "our enemies may return to human form and come after us, so we've got to hurry on."

Then they actually drove their old grandmother before them until at Totolospí they let her get ahead while they stopped to play a game, using petrified wood instead of reeds as counters.

When they again caught up with the Spider Woman she was very nearly exhausted, so the Twins put a hand each under her armpits and half lifted, half dragged her along. Finally, when she complained that her muscles ached so that she could not take another step, the Twins decided that they had gained a sufficient lead on any one who might be pursuing them, so they paused to rest. Then they dug a narrow trench and told their grandmother to lie in it on her back.

As soon as she was comfortably settled the Twins said, "Now we'll take off your dress so that your private parts will show, and when the Hopi come by here every one of the men will 'get into you' so that we can help them. In this way we'll trade with each other, because on their way back they will leave some salt for you." Then they chewed some medicine and spurring it on the Spider Woman, turned her into rock.

Freed from the burden of dragging the old woman with them, the Twins hurried on and arrived at the mouth of the canyon where Pukonghöya, the elder, said to Palungahöya, the younger, "Now I'll make you stay here so that when the Hopi come to this place for salt they will pray to you for the reward (rain) that they have won by beating us at Totolospí back there."²¹

Thus the younger Twin was turned to stone and Pukonghöya traveled on by himself past the place that spreads the rectum, past the place which he assigned as a home to the Bakab (Reed) clan, and on to Patcip-ve'itaka. Then he continued on

²¹ There is a discrepancy between the salt-gathering narrative told by Don and the flight of the War Twins as told by his father. In Don's version both Twins are represented here by rocks

to Panktpatca and to Chicken House, leaving the proper directions for the Hopi to follow at each place.

Soon the older Twin came to the home of the Nukpana (Masau'u), who is the head chief of the canyon and who dwelt there long before Pukonghoya's arrival. Of him the Twin asked as a favor that he help whatever Hopi should pass that way in the future and Masau'u agreed to do so. From the Koyemsi, too, whose home was the next spot he reached, Pukonghoya received a similar response to his request that they aid the Hopi.

Soon the traveler came to the sipapu "Now when salt gatherers come here," he announced, "they will set their offerings right by the side of the hole," and he then went through the entire procedure to be followed. He gave out further instructions regarding the proper manner of removing pavisa, and then went on to the breast-shaped rock where it is necessary to drop from one level to another. To help people in the future he decided to turn himself into a rock at this place, saying that in this way the Hopi would be able to fasten their ropes around him and so clamber down safely with the help of his power.

Then he dropped to the ridge below where he established the home of the Kwans and fixed the basin of medicine water for the manufacture of tohopkom out of qomi. As he walked along the ledge he rubbed his fists continuously against the walls of the canyon, turning everything he touched into salt. This done, he climbed back to the shelf above and turned into rock as he had promised.

In this way the manner of conducting salt expeditions properly was taught to the Hopi.

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THE SIGNIFICANCE OF THE STUDY
OF ACCULTURATION FOR
ANTHROPOLOGY

By MELVILLE J. HERSKOVITS

AN outstanding development in anthropology during recent years has been the growing interest in the study of peoples whose modes of life are undergoing, or have undergone, extensive change as a result of ascertainable historic contacts with alien cultures. Acculturation, the term current in this country for such studies, has been defined as comprehending those phenomena which result when groups of individuals having different cultures come into continuous first-hand contact, with subsequent changes in the original cultural patterns of either or both groups.¹

This definition restricts acculturation to those aspects of cultural change that have resulted from first-hand contact between peoples. It plainly does not differentiate contacts between historic and non-literate folk from those between two primitive peoples. Nor does it touch upon the motivations behind studies of these cultures, though no discussion of the importance of acculturation in the furtherance of the aims of anthropological research would be realistic in its treatment unless it took into account that here the issue is joined between scientific and "applied" anthropology.

To assess the significance of acculturation studies, the historical background of anthropological theory out of which they have arisen must first be sketched. When, at the turn of the century, the explanations offered by the evolutionary school became patently untenable, a more inductive mode of studying culture was developed, based on field investigation of primitive life. But though it was believed that from the resulting data valid generalizations about the processes of cultural dynamics might be made manifest, the great bodies of carefully controlled field reports were mainly used to make reconstructions of historic contacts between peoples—that is, in attempts to recover the processes of non-recorded history. In this country, such reconstructions were generally limited to restricted areas where a high degree of probability existed that the assumed contacts had actually taken place. In Europe, however, where history tended to become a fetish, logical deductions derived from the plotting of sweeping world-wide distributions of traits considered with all but complete disregard for their cultural contexts attained wide currency. The weakness of all such studies, as came very clearly to be understood in this country at least, was that in

¹ R. Redfield, R. Linton, and M. J. Herskovits, *Memorandum for the Study of Acculturation* (American Anthropologist, Vol. 38, pp. 149-52, 1936)

reconstructing unrecorded history, lacking the controls of the documentary history of the historians. no technique existed whereby the validity of one conclusion could be sustained against a contradictory one, too often derived from identical data.

Thus an uneasy feeling began to grow that though cultures were being studied with the greatest possible objectivity, the relationship between culture and its human carriers continued to remain obscure. In time, this methodological self-criticism brought forth an approach which sought new emphases in the study of culture. Autobiographies were obtained from members of primitive communities, mythology was analyzed to show how it reflected its cultural setting, and the place of all manner of cultural phenomena, from art to technology—"in the minds of the people" as it was sometimes phrased—was investigated. Yet not until this new trend became so sharply pointed as to clearly suggest the direction that field studies were taking was there any tendency to inveigh against the historical consideration, which, it might well have been recognized, implied much more than an insistence on recovering an unrecorded succession of events. As the formulation of this trend of thought crystallized, however, its position, especially in England, advanced to the point of maintaining that any historical consideration was merely "antiquarian," and hence of little scientific value. Therefore, all thought of history was abjured, and an understanding of culture was sought in the intensive study of the interrelationships between the facets of a single civilization, their integration with each other, and the effect of their interplay on the psychology of the individuals manifesting them.

While these latter points were not new to American anthropologists, the insistence with which this anti-historical position was phrased and rephrased served to accelerate a movement that was already in a healthy state of growth. For if the studies of the anti-historians lacked a certain breadth of background and exhibited a certain shallowness due to a failure to recognize the importance of time depth, they nonetheless often revealed values in the cultures studied that research less bent on determining the inner relationships of the elements of a given civilization ordinarily missed. Similarly, if the refusal to take the historical part of a culture into consideration made for a certain naïveté when the anti-historical school came to study peoples not living in extreme isolation, it is also true that their refusal to make historical reconstructions and to plot distributions made it possible for them to present a portrait of primitive folk that was far more lifelike than could possibly result from a search for "traits."

It was against this background that the study of acculturation came

to be projected in dimensions large enough to allow its scope and problems to be formulated. This is not to say that the significance of the study of cultures in contact had been previously disregarded by anthropologists, for anthropological investigations conducted among American Indian tribes, in Mexico, and among New World Negroes and other peoples having hybrid cultures had indicated the possibilities inherent in scientific research of this type, while in Europe, a growing recognition of the importance for European Colonial administrators of a knowledge of the traditions of the peoples under their charge, foreshadowed another place this field of interest was to make for itself. It was rather that the drive toward the study of such cultures was too diffuse to stimulate the prosecution of much active work directed primarily toward this end. Therefore, despite all this early work and its more numerous recent counterparts, concepts are still vague, methods anything but equivalent, and the ends in view diverse, so that it has become imperative that the position of acculturation studies in the anthropological repertoire be clarified and the contributions to be expected from them be made explicit.

Initially it should be emphasized that the study of acculturation bridges the gap between the two current extremes of opinion held by anthropologists as to the problems and methods of the discipline, and thus must take its place alongside other anthropological approaches to the study of culture. If the matter be considered from the point of view of cultural dynamics and the importance of culture as an historical continuum, it is to be seen that except in rare instances, of all who are concerned with primitive societies only the student of acculturation is in a position to work under the conditions of control afforded by recorded history. On the other hand, those studying acculturation are also in a strategic position to investigate the interrelationship between the elements of a given culture, and the manner in which the functioning whole influences the individuals who live under it. For how better analyze the mechanisms of culture than under those conditions where, to varying degrees, forces that are known entities rather than hypothetical reconstructions have been operative or are actually at work? How better attack the difficult problems of the inner relationships between the elements of a given culture and their influence on personality, than in those situations where the psychological stresses and strains incident upon cultural change enable the student to observe, under conditions of known historic control, the results of changes in settled habits of belief and behavior? Is it not apparent that if competent analysis is made of the baseline from which the process of change in a given society took its beginnings, and the consequences of a subsequent cultural contact as found in the life

of the people carefully recorded, a more precise determination of the manner of cultural change and the inner relationship of the elements of culture involved will be possible than can come from the employment of any other method?

The anthropologist who studies acculturation that he may understand the processes of culture has at hand, first of all, the materials of historical documentation. It is particularly to the point to stress this, since the obliviousness of students of primitive life to historical records bearing on the peoples they study is a commonplace. In studies of acculturation, however, recourse to history—actual recorded history, that is, not “historical” reconstructions—is mandatory. The study of present-day Haitian peasant culture may be taken as a case in point. Here an historical analysis based on ethnological background has made it possible to identify the African tribes that contributed to the Negro ancestry of the present Haitian population so that the African elements in the culture of Haiti today can be referred to their exact provenience, while, to no less a degree, this approach has made it possible to recover the cultural setting the French masters presented to the eyes of their slaves. As a result, the manner in which aboriginal aspects of culture persisted in Haiti in the face of terrific repression can clearly be seen, as well as how with gentler times the unceasing process of combining and revamping French and African custom brought it about that at present the resulting cultural entity exhibits, in its outer aspects at least, the characteristic forms of any unified functioning culture. Mexico furnishes other instances; aspect by aspect, the culture of the Indians living there today can be analyzed and the whole studied on the basis of a known historic past so that, as has been shown for definite elements in a single Mexican culture, early records permit an understanding otherwise unobtainable of seeming anomalies in its present-day organization.

Many specific problems in cultural dynamics can best be investigated advantageously through studies of acculturation. The relative stability of material aspects of culture as against the intangibles of tradition can be assessed on the basis of known data, not of hypothetical assumptions; in a similar manner, such problems as the relative conservatism of men and women can be analyzed. The power or impotence of the individual in the face of established tradition, the mechanisms that enable an individual to bring about cultural change or to enforce cultural stability; the educative forces which condition an individual to the patterns of behavior sanctioned by his society—problems of this order, as well as numerous others, can be studied most profitably where cultures are in a state of flux due to the shock of contact with new bodies of technology, belief, and traditional behavior.

Thus in these, and many other ways, acculturation reinforces the historical flank of anthropological investigation. What is its place at the side of those not concerned with the dynamics of culture, but with the interaction of the parts of a single culture considered on a single time plane, and with the influence of a culture in shaping the habitual reactions and personalities of those who live under it? Here, too, the study of cultures in contact is no less useful. For it is under conditions of change that the matters of primary interest to the anti-historians are most clearly shown; where traditions are in conflict, the readjustments within a culture incident to the process of effecting felicitous combinations of ancestral traits with those newly introduced, can throw much light on how the elements of culture are interrelated and how the resulting whole functions; while in those situations where traditions are in conflict, personal adjustments to the scene are more clearly discernible. If reference may again be made to Haiti, where continued contact between French and Africans resulted in a centuries-long consolidation of traditions, not only can the functioning of the entire code of Haitian culture in its present-day hybrid form be seen with the greatest clarity because of its dual nature, but the manner in which, in technology, social, and economic organization, religion, music and the dance, the ancestral components have taken new form as a result of the movement of the historical kaleidoscope, can very readily be discerned. If it be considered at the same time how the dichotomy shown by the elements of Haitian culture has affected the psychology of the Haitian—resulting in a state of “living on one’s nerves,” as a Haitian physician has phrased it out of his long observation of his people—may it not be asked whether in the case of other cultures that have undergone or are undergoing similar experiences, acculturation studies cannot be expected to further the aims of those students of culture who are more concerned with form than with process, with personalities rather than with the course of history?

A final word is to be said concerning the ends toward which studies of acculturation have been directed. Many scientific anthropologists have grown inhospitable to studies of culture-contact because of insistent claims from some quarters of their importance for what has come to be termed “applied anthropology.” Allowing wholeheartedly for the skepticism which such claims must engender—for we do not as yet know enough about culture to determine the destinies of peoples who have lost control of the direction of their own affairs because of the superior power of European nations—

² Cf. M. J. Herskovits, *Applied Anthropology and the American Anthropologist* (Science, Vol. 83, pp. 215-22, 1936)

—it is nevertheless manifest that the value of the scientific study of culture-contact cannot be held the less valid because of its possible misuse at the hand of those interested in special pleading. Fortunately there are not many who counsel the anthropologists to restrict their activities to more conventional assignments—urging that they devote themselves exclusively either to the analysis of those “uncontaminated” cultures to be found in the remote parts of the earth, or where broken cultures must be studied, that they attempt only to recover aboriginal modes of behavior and pay but scant attention to the present condition of such cultures and the effect of this condition on their present carriers. Certainly in the case of innumerable problems of anthropological significance in the Africanist field, whether studied in Africa or in the New World, such rigid delimitation would not only act as a straight-jacket but, with the factor of contact here being so continuous and decisive, would render them altogether impossible of study, especially since anthropological methods can alone cope with them. Though in the Africanist field as in the field of American Indian research there are important problems that can be solved, and solved advantageously, by a “purist” method, its dogmatic and exclusive continuation must inevitably lead to sterility. It is for this reason that the thesis is maintained that for a basic understanding of the problems which concern the nature and mechanisms of culture and the relationship between a culture and its carriers, the approach through studies in acculturation, by permitting greater control over data under analysis, and by allowing the anthropologist to employ the laboratory of history, must play a role of ever increasing importance in anthropological research.

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IN 1923 J. Alden Mason, in a consideration of the consonants to be reconstructed for original Uto-Aztecan, wrote "*tl*, on the other hand, is found only in certain Nahuatl languages. Other dialects and all other Uto-Aztecan languages replace it with normal *t*. It seems a more rational explanation, therefore, to consider *tl* as developed from *t* in Aztec under as yet unelucidated rules."¹

The alternative to this explanation is, of course, to regard *tl* or its ancestor as an original Uto-Aztecan consonant distinct from *t*. In two recent publications² I reaffirmed the thesis of Mason's, stating that it could be considered as undoubtedly correct, but gave no proof. In the present paper I wish to submit the proof; or in other words to elucidate the rules, which Mason supposed to exist, and which in point of fact do exist, for the development of original **t* under the influence of wholly local Aztec surroundings into two Aztec consonants, *tl* under one set of Aztec conditions and *t* under the complementary or alternative set.

From this point on I shall use the symbol λ in place of *tl*, as denoting a single phoneme, not a cluster of *t* and *l*. Mason's "certain Nahuatl languages" can probably be amended to "a group of specially related dialects of the Nahuatl language, forming one major dialectal division, which may be called Central Nahuatl, or Aztec." The crucial observable fact concerning the interrelation of λ and *t* in Aztec has not hitherto been pointed out, to my knowledge. It is this: with the exception of (1) the absolute noun suffixes *-λ* and *-λi*, and (2) a statistically small scattering of sporadic occurrences, λ is found only before the vowel *a* in the same word, while *t* is found only in the complementary distribution, that is before sounds, whether vowel or consonant, other than *a*, and as word-final, with the exception of (3) a statistically small scattering of occurrences before *a*. The relatively few irregular exceptions under (2) and (3) seem to stand outside the phonological system of the language, which, aside from them, works with quite mechanical regularity. Thus, in a certain class of verbs, change of final *-i* to *-a* changes the meaning from intransitive to transitive. Accordingly we find that *pa ti* 'it melts' becomes *ki-pa-λa* 'he melts it.' Or again, final *-a* is changed to *-ilia* to form an "applicative," a doubly transitive verb

¹ J. Alden Mason, *A Preliminary Sketch of the Yaqui Language* (University of California Publications in American Archaeology and Ethnology, Vol. 20, pp. 195-212, 1923).

² B. L. Whorf, review of A. L. Kroeber, *Uto-Aztecan Languages of Mexico* (American Anthropologist, Vol. 37, pp. 343-45, 1935); B. L. Whorf, *The Comparative Linguistics of Uto-Aztecan* (American Anthropologist, Vol. 37, pp. 600-608, 1935).

with both direct and indirect objects: so that $\lambda aso^{\circ} \lambda a$ 'love' and $\lambda a \lambda a$ 'burn' become $\lambda aso^{\circ} t i l i a$ and $\lambda a t i l i a$.

So much can be ascertained simply from a thorough synchronic study of present-day Aztec. Comparative Uto-Aztecan study however yields a confirmatory result—namely that original UA* t when followed by * a gave rise to Aztec λ , in other situations to Aztec t . Thus, on the one hand, UA * $tama$ 'tooth' > Tübatulabal $tama$ -, Hopi $tama$, S. Paiute $ta\eta^{\circ}a$ -, Tarahumar $\dot{d}am\dot{i}$ Aztec λan -; or UA * $k^{\circ}ita$ 'excrement' > Ho. $k^{\circ}ita$, S.P. $k^{\circ}i\dot{c}a$ -, Yaqui $b^{\circ}ita$, Tepecano $b\dot{i}t$, Cora $\dot{v}ita$, Az. $k^{\circ}i\dot{\lambda}a$ -. On the other hand UA * $t\acute{e}ka$, * $t\acute{e}ke$, * $t\acute{e}ki$ 'cut' > Ho. $t\acute{e}ki$, Tüb. $t\acute{e}ha$, S.P. $t\eta\gamma ani$ -, Papago $\acute{e}ik$ ('hole'), Az. $t\acute{e}ki$; UA * $s\acute{u}tu$ 'finger-nail' > Tüb. $\acute{s}ulu$ -, S.P. $\acute{s}i\dot{c}u$ -, Tar. $sutu$, Pap. $hu\dot{t}i$, Co. $\acute{s}ite$, Az. $-ste$ -, $iste$ -; UA * $tusi$ 'grind' > Ho. $tosi$, S.P. $tu\dot{s}u$, Tar. $du\dot{s}$; Pap. $\acute{e}uh$, Co. $ti\ \acute{s}i$, Az. $te\ \acute{s}i$; UA * $toka$ 'call, cry, name' > Ho. $totoku$, Az. $to\ \acute{k}a$ -, and so on.³

If now the method of comparative linguistics be pushed further and deeper, it will not only confirm the Aztec situation as revealed by synchronic linguistics, but will throw a further light upon that situation that no amount of synchronic study could make forthcoming. In brief, it will provide a harmonious explanation of the most troublesome of the exceptions to the Aztec complementary distribution of λ and t , and the remaining exceptions will be reduced to a number entirely permissible as stray cases due to unknown disturbing influences—special phonological situations, analogies, and loan-words.

It is first found that Uto-Aztecan * a does not invariably yield Aztec a , but in a statistically small number of cases yields Aztec e (or more rarely i that may be considered a development from e). This vagary cannot be correlated with anything in the Aztec phonetic surroundings. In the wider Uto-Aztecan purview it can be correlated, to a fair degree of probability, with the matter of the length of the Uto-Aztecan vowel, but the evidence is rather scanty, due to our lack of information on the vowel-lengths in so many Uto-Aztecan languages.

In my *Comparative Linguistics* I stated that the first vowel of the typical CVCV root might be of one, two, or three moras, * a , * a , * a a . On the basis

The phonetic orthography used is the one recommended by Sapir and used in my *Comparative Linguistics of Uto-Aztecan*. The symbols \acute{c} , \acute{t} , stand for older c , t , respectively. Accent if moras is generally not indicated, but in modern Aztec it is nearly always on the penult. As will be explained, the three vowel-lengths in Hopi are denoted differently from the usage of the cited work. I should also say that while a synchronic treatment of Hopi conventionally recognizes two main types of guttural stop, h and q , for comparative purposes it is best to cite Hopi in terms of t that takes in all complementarily distributed h and q , and k for the remaining, an especially fricated t like $\acute{k}y$.

of statistical occurrence, however, the situation does not suggest a simple build-up of length units by singles, pairs, and triplets of such units. So far as our rather scanty length evidence goes, it seems to show that the middle degree of length, **a* above, is much the most common, and the shortest length is the least common. Hence this shortest length does not seem like the elementary structural unit, as the above graphic symbolism somewhat tends to imply. Perhaps it would be better to use the symbolism **ǎ*, **a*, **ā*, and the terminology "reduced mora or ultra-short," "full mora or short (or medium)," "two-mora or long." In Hopi we have precisely this odd three-length system. In Southern Paiute, Tubatulabal, and Aztec we have information on the length of vowels, but these languages have two-length systems,⁴ and their short vowel corresponds on the whole both to the ancient short and the ancient ultra-short. "On the whole" should be emphasized, for any language may change the length of a vowel for obscure reasons. For one thing, most Uto-Aztecan languages have stress accent, and stress often alters vowel-length. However, such evidence as we have seems to show that a rather uncommon, sporadic, special type of short *a* in Uto-Aztecan yields *e* in Aztec except when preceded or followed by **k*—not to be confused with **k̄* or **k^u*. This vowel may even have had a tinge of *e*-equality in Uto-Aztecan, for it occasionally gives an *e*-reflex outside of Nahuatl. Here is the evidence:

UA **k̄āca* > Ho. *kāhcan*- 'bite,' Tub. *ha iḡi bī?*- 'chew cud,' Az. *kecoma* 'bite.' The Hopi vowel is not ultra-short, but ultra-short does not occur before pre-aspirated consonants in Hopi.

UA **k^uǎ* + determinative of water-animal = 'frog' > Ho. *pa-kā*, S.P. *pa-k^ua-n'a*, Tub. *wa-ga-iš*-, Huichol *šu-k^wa*, Az. *k^we-ya*-, all 'frog.'

UA **k^uǎsa* > Ho. *k^uǎsa* 'skirt,' Az. *k^wešan*- 'a tunic-like garment.' Note that here the Hopi vowel is ultra-short, as in three instances below.

UA **k^uǎna* > Ho. *k^wǎna* 'split, cleave,' Az. *k^wemī*-, *-k^wen* 'a furrow.' It is suggested that the *m* in the Az. full grade may be from **n* assimilated towards the preceding labial sound.

UA **lǎḡi* 'tongue' > Ho. *lǎḡi*, Tub. *lalan*-, Opatá *nene*-, Varohío *ḡeni*, Cora *nanuri*-, Huichol *neni*, Az. *nene-pil*-, all 'tongue.' The *e* that appears in other than Aztec indicates perhaps the fronting effect of the original **l*-.

UA **māta* 'stone mortar for grinding' > Ho. *māta*, S.P. *marā*-, Tub. *mana-l* (dissim. < **mala-l*), Yaqui *mata*, Tarahumar *mata*, Huichol *mate*, Az. *meXa*-, all 'stone mortar.'

⁴ I thought at first that Tubatulabal had a three length system, judging this from the orthography used by Voegelin in his grammar, but it would seem from the careful examination that has since been made by Voegelin and Swadesh that this is not the case.

UA **nā-*, reflexive and duplicative prefix > Az. *ne-*, all other languages *na-*. However, when secondarily lengthened in UA it gives Az. *na-*, e.g. UA **na-wō-y* 'four' (< *wō* 'two') > Ho. *na lo-y*, Az. *na wi*.

UA **nāk^wa* 'agave drink, pulque' > Cora *nawa*, Huichol *nawa*, Az. *nek^w-*.

UA **nāsi* 'ashes' > Cora *nasi*, Az. *neš-*.

UA **pāhi* 'three' > Ho. *pahi-w*, Tüb. *pai-i*, S.P. *pai-*, Mayo *bahi*, Tepecano *va i-*, Heve *vei-*, Cora *wai* (the *w* is anomalous for Cora), older Az. *ei*, modern Az. *yēi*.

UA **sākh^wa* > Ho. *sak^wa* 'green, green-blue, turquoise,' S.P. *saγ^wa-* 'blue, green,' Heve *sag^wa* 'leaf,' Az. *šiw-* 'green, green-blue, turquoise.' Here the Aztec change has gone beyond *e* to *i* (*e* sometimes secondarily becomes *i*, *še* is rare, syllable-closing *k^w* sometimes becomes *w*, e.g. *šik^wna wi* or *šiwna wi* 'nine').

UA **tā^wk^wa* > Cora *tak^wa* 'master, lord, god,' Az. *tek^w-* 'master, lord, god.'

UA **te^wpā* 'ground, hill, mountain' is the form indicated by several cognates, but the Aztec is *tepe-* 'mountain.' Another explanation, vowel-assimilation, is also possible here.

The non-*e* reflex when flanked by *k* is shown by e.g. **kāte* > Ho. *kāt*, 'sit,' Az. *kat-ka* 'was,' **māka* > Ho. *māka* 'give,' Az. *maka* 'give.'

There are some other evidences for the existence of ultra-short vowels in Uto-Aztec, such as the complete loss of them in Aztec under certain conditions—perhaps stress conditions—as in **sātu^w* > Az. *iste-* cited above, or **kāsi* > Ho. *kāsi* 'thigh,' Az. *ikši-*, *-kši-* 'foot, leg'—the compressed word then acquiring a prothetic vowel *i-*—perhaps borrowed from a frequent pronominal prefix *i-* and lost after other preposed elements. Again, there are the occasional doublets in Aztec like *mama* 'carry' and *meme* 'carry,' as if they derived from **mama* and a shortened variant **māmā*. However, whether or not "ultra-shortness" be the best explanation, the above evidence shows that UA **a* was here and there in a peculiar condition in which it regularly gave rise to Aztec *e*.

The greatest exception to the rule of *ā* in Aztec is provided by the hosts of words ending in the absolutive noun suffix *-ā*, *-āi*. This corresponds to the Uto-Aztec absolutive suffix **-t* for the nominative case, to which could be added the case suffixes **-e* genitive and **-a* objective (accusative). The genitive suffix may have survived only in Tübatulabal, Heve, and some of the Southern California tongues, but the objective ending has survived to a much more widespread extent, either as *-a* or as *-ta*. Where a caseless language like Aztec has descended from one with a system of nominative,

genitive, and objective, the caseless noun is perhaps especially likely to be derived from the objective form, the one of most frequent occurrence. Names other than those of persons are more likely to occur in discourse as object or in relations expressed by the objective case like to, at, in, with, etc., than as nominative actor. This is seen in the Romance languages, whose nouns are derived from the Latin accusative. Hence the pre-Nahuatl objectives ending in *-ta* would yield old Aztec nouns ending in *-ā*. The *-a* however may have been of the "ultra-short" type; indeed the Hopi evidence suggests this. In Hopi the objective *-a* is not heard at all unless it ends a sentence. Within the sentence it disappears, leaving the objective case terminating in *-t*, which distinguishes it from the nominative case, the latter using the bare stem. Now this is not a property of all final vowels in Hopi, but occurs only in certain forms, with vowels most or all of which seem to be historically the vowels of nominal case endings.

If now we assume that **t* yields *χ* in Aztec not only before Az. *a* but also before Az. *e* when that *e* corresponds to Uto-Aztecan *a*, in other words before *a* of the earliest stage of Aztec, then we explain both the occasional occurrences of *χe* in Aztec, and the absolute suffixes. The nouns in **-tā* became nouns in **-χā*, and then in **-χe*—indeed the transcription "*tle*" instead of "*ili*," is found in the oldest Spanish chroniclers such as Bernal Diaz. The further fate of this vowel when final and preceded by *χ* was determined by its position relative to the stress accent, and ties up with the fact that *e* is "weak" as a final vowel in Aztec, scarcely ever occurring in unstressed final position. The words commonly written as ending in *e*, like [cate, topille, pixque, yazque, tlacuanime],* actually end in the "saltillo," a glottal consonant, and in my orthography are *kate*°, *topille*°, *piške*°, *yaske*°, *χak'ani me*°. Immediately after a stress, which is also equivalent to after a consonant, the weak final *e* narrowed further to *i*, producing nouns of the type of *me* 'c-χi 'moon,' *nemili's-χi* 'life,' *si'°-χi* 'hare,' *to-č-χi* 'rabbit,' *χaka'°-χi* 'day,' *wi* 'c-χi 'thorn,' etc. When two syllables distant from the stress the final vowel became so weakened as to disappear, producing the nouns of the type *ko'ne-χ* 'child,' *ko'yo-χ* 'coyote,' *ma'sa-χ* 'deer,' *me'χa-χ* 'mortar,' *si'wa -χ* 'woman,' *šo'či-χ* 'flower,' etc. Hence the *χ* in the absolute suffixes is accounted for.

There remains one other small source of final *χ*. In the possessed forms *-k'īχ*, *-maχ*, *-meχ*, *-peχ* of the nouns respectively 'excrement,' 'net,' 'mortar,' 'mat,' an ordinary *a* has been lost, not by weakening but by morphological apocopation, from the absolute forms *k'wīχa-χ*, *maχa-χ*, etc. Here, of

* Brackets here indicate that the words enclosed are not phonetically transcribed, but represent quoted forms.—Editor.

course, the λ determined by the a vowel remains after secondary loss of the vowel. In the case of *maš λ a- λ* 'breechcloth,' the apocopated possessed form **-maš λ* is replaced by *-maš λ i* through analogy with the other words in *- λ i*.

This same principle explains the only occurrences of λe in stems. The syllable occurs only initially, except as a result of combining stems. The word λe - 'fire' is clearly the Aztec representative of Uto-Aztec **tā*, **tā^oi* 'fire,' whence Yaqui *tahi* 'fire,' Cora *ta* 'fire,' *taire* 'kindle,' Opata *ta* 'burn,' *tai* 'fire,' Papago *tai* 'fire,' Heve *te* 'fire,' Hopi *ta-k-* stem (in derivatives) meaning 'burn.' During the pre-Aztec period the word evidently occurred as a doublet, **ta* and **tā*, the latter yielding λe 'fire' and the former $\lambda a\lambda a$ 'burn' and $\lambda a\lambda i a$ 'burn.' The word $\lambda e\omega a$ 'species of large snake' evidently bears the suffix *- ωa* 'possessor of' and means 'possessor of fire.'

The word $\lambda e^o ko$ 'rise' may be compared with *ako* 'up' and Hopi *ʔākʷa* 'upward to, up.' If we reconstruct the root as a doublet **ʔako* **ʔāko* and assume the latter form to have been combined with the verb-prefix **ta-* 'something' (later $\lambda a-$) in a reduced form **tā-*, as a goal-object ('upward to something'), we arrive at a pre-Aztec **tāʔāko*. In Aztec the second ultra-short vowel is syncopated, leaving however the glottal consonant or "saltillo," while the first one becomes e , yielding $\lambda e^o ko$.

The words λein , λen , λei 'final n lost by weakening 'what, something' have evidently resulted from combination of the demonstrative pronoun *in* 'this' with an element **ta*, **tā* ultimately the same as the verb-prefix $\lambda a-$ 'something.' This is shown by the fact that in some dialects, such as that of Milpa Alta, D.F., λein is replaced by λaon , the parallel compound with the demonstrative *on* 'that.' While **ai* does not ordinarily yield Aztec ei , **di* would do so. The particle λe sometimes used with the imperative is very likely a similar sort of doublet with the particle λa 'if.' The word $\lambda eyo-$ 'fame, honor' is again perhaps derived from the above-mentioned **tā* 'something' with the abstract-noun suffix *-yo*, perhaps influenced by analogy of an unrelated noun *tenyo-*, *teyo-* of the same meaning; compare English 'to amount to something.'

The word $\lambda eko-$ 'line traced in middle of ball court' may be explained as a doublet of $\lambda ako-$ 'rod;' it means secondarily 'delay,' and $\lambda eko\lambda i a$ 'grant a delay or stay of time' is a derived verb no doubt alluding to the ball game. This completes the known cases of λe .

We may now consider the very few cases of λ before other vowels than a or e . The words $\lambda ila-$ 'abyss, dark depths,' $\lambda ilania$ 'make black,' $\lambda il-li$ 'black paint, ink,' $\lambda il-$ 'black,' I derive from Uto-Aztec **tu* 'black' (whence, e.g., S.P. *tu* - 'black,' Tub. *tu -l* 'charcoal'), which is the source

of the much more common root **tu·^hka* 'black, dark, night' found in most Uto-Aztecan languages. The simple root took on the noun-theme suffix **-la*,⁵ and I then assume an ultra-short first vowel in pre-Nahuatl, i.e. **tūla*, which then underwent assimilation to **tāla*. This gave in Aztec at first *ħela*, then the change went further to *ħila*. This last change may have been aided by the analogy of some neighboring dialect which had not made the vowel-assimilation and therefore showed **tila* as the straight descendant of pre-Nahuatl **tūla*.

The postposition *ħok* 'with, near' is in the Milpa Alta dialect *ħak^w*, and this is probably the primary form of the word. The spelling [tloc] of classical Aztec is either the approximation of a Spanish scribe to the unfamiliar sound *ak^w* or represents a dialect in which the *a* was actually rounded to *o* by the following lip-rounded sound *k^w*. The name of the indwelling universal god, Tloque Nauaque, is derived from this particle; the first word is in my orthography *ħak^wke*?

As for *ħo*·- 'hawk,' we may note that Aztec *o* is occasionally derived from **aw*, and compare Heve *toha'wo* 'hawk.' An original **toha'wo* would yield Nahuatl **toa'wo*, which would probably assimilate to **tawo*, whence Aztec **ħawo* and eventually *ħo*·- —for the saltillo sometimes represents a lost syllable in *w*-. The difference between this case and *to ċ*- 'rabbit' (<UA **tavu*> Hopi *ta vo*, S.P. *tavu*-, Heve *tavu*, all 'rabbit,' pre-Aztec **taw*·->**to*·-, with addition of thematic *-ċ*>*to ċ*-) is that in the latter case the stage **to* - was reached in the pre-Aztec period, while with 'hawk' the form in this period remained **taw*·- so that in the Aztec period it could become **ħaw*·-, and last of all *ħo*·-. The word *ħolololtin* 'gathering, group' is contracted from *ħa-olololtin*. This finishes the cases of initial *ħ* occurring otherwise than before *a* in the two great Aztec dictionaries.⁶

Cases of *ħ* otherwise than before *a* in the interior of words are very rare and always due to the juxtaposition of elements in a compound. One of the most interesting such cases is the name of the god Tezcatlipoca, for it tends to confirm my explanation of the reason for the *ħ* of the absolutive noun suffixes. In one of the earliest post-Conquest Aztec manuscripts, the *Historia Tolteca-Chichimeca*, the name is always written Tezcatlepoca (*teskaħepo ka*). It means "smoking mirror," from *teska*-*ħ* 'mirror,' *po·ka* 'gives off smoke.' According to my explanation, we have here a very old

⁵ It may turn out with the discovery of just a little more evidence that the formula **tu^h* should be **tul*, a contracted form, of which **tūla* is the original and primary root, and that there is no suffix involved.

⁶ Fr. Alonzo de Molina, *Vocabulario de la Lengua Mexicana* (Julio Platzmann, ed., Leipzig, 1880), Rémi Siméon, *Dictionnaire de la Langue Nahuatl* (Paris, 1885).

combination, dating from the time when the absolutive suffix was **-tā*, giving the combination **teskaā-po·ka*. This later yielded in regular fashion *teskaāe-po·ka*, the ultra-short vowel preserved because word-internal, although lost when word-final in *teskaā* 'mirror.' The vowel change then went further to *i*, giving *teskaāipo·ka*. In the few other cases of internal *āi* the *i* belongs to the second component, e.g., the name of the goddess Coatlicue and the plant *a āi·nan*, which represent the combinations *koa ā-i k^wei* 'serpent her skirt' and *a ā-i nan* 'water its mother.' Most Aztec compounds, however, are made according to a pattern that is even older than that of Tezcatlipoca, dating from a time when according to my theory the absolutive suffix and its vowel signalized *case-distinctions*, so that *ā* is not found in such compounds, because it would have to represent **-tā* of the *objective case*, which is not the case of the compounding relation. Such compounds usually show no trace of any absolutive suffix (old caseless form) internal to the word; sometimes they have an internal *-ti-* or *-t-* which must represent an old **-ti* or **-te*, perhaps of the genitive case. We have now accounted for all that statistically small scattering of cases which at first seem to be exceptions to the rule that *ā* occurs only before *a* as an alternate for *t* in all other positions.

There remain to be considered those other apparent exceptions to the rule, the scattering occurrences of *ta*. First, the rule when completely formulated does not apply to **ta* preceded by a consonant in pre-Aztec; *t* as second member of a consonant-cluster was somehow "protected" against the change. This explains the *ta*'s in *i[?]talwia* 'tell to,' *ista-* 'salt,' *ista-*, *-sta-* 'white.' *iāta* 'secretly,' *itta* 'see.' Probably also *itaka-* 'provisions' belongs here, to be regarded as simplified from **ittaka-*, just as *ita* has become *ita* in some modern dialects. Another way of stating this revision of the rule is that *ā* does not occur in clusters *within the root of a word*, that is, which were firm clusters in pre-Aztec. Where *ā* occurs in a cluster, as in *iāāwa*, *iāipāyau*, *mosā*, *cinān*, the cluster is of later origin and represents the juncture of elements. The cluster formed by the old absolutive suffix **-tā* was of course not a "firm" cluster; it represented a juncture of elements each of which was freely replaceable by others, hence the *t* was analogous to one at the beginning of a word, and was not "protected."

The fact that the syllable *ta* survived into Aztec by way of these old firm clusters resulted in that *ta* remained a possible or "pronounceable" combination in Aztec speech patterns. This made it possible for the pronominal prefixes *tī-* and *to-*, in becoming elided to *t-* before vowels, to produce the sequence *ta=t-a*, as in *tawī=t(o)-awī* 'our aunt.' In other words, since *ta* remains a possible sequence in Aztec, there is no reason why it

should not result from a juncture of elements; and since the parts of a juncture tend to conserve their typical forms as members of their two different systems, this secondarily formed *ta* undergoes no conversion to *ʃa*. In the same way Aztec is able to borrow a word containing the sequence *ta* from another language or from a *t*-dialect of Nahuatl without necessity that this *ta* be repatterned to *ʃa*.

The word *taʔ*- (*taʔ-ʃi*) 'father' however probably belonged originally to the class of "baby words," "nursery words," "Lallwörter," and as such passed through the shift from *ta* to *ʃa* unaffected. It is known that words of this type may be conservative of their original form in the face of a change that sweeps over the adult language. It is worth noting that this word is not the regular Uto-Aztecan word for father, but apparently a Uto-Aztecan word for uncle, **taha*, whence Hopi *-taha* '(one's) maternal uncle,' Yaqui *tata* 'maternal uncle,' Tarahumar *ʒate* 'uncle,' which word in this meaning has in conformity with adult language undergone the Aztec shift in the regular manner, to *ʃaʔ*- (*ʃaʔ-ʃi*) 'uncle.'

There now remain in the dictionaries the following stems containing *ta*: *taka*-, *tataka* 'dig, scratch' (from which are derived *takaliwi*, *takapiliwi*, *takaʃotia*, and perhaps *takanal*- 'root of a certain plant'), *taka*- 'species of shrub,' *tačitowia(n)* 'species of bird,' *taletek* 'small lizard,' *tamačīwa* 'measure,' *tamal*- 'tamale,' *tamasolin* 'toad,' *tamati* 'take as a patron,' *tanaʔ*- 'palm-fibre basket,' *tapa*- 'break up, blister, frizzle up' (whence *tapalka*- 'sherds, broken tile,' *tapaliwi* 'be blistered,' *tatapačoa* 'frizzle, curl, crimp, hair or cloth,' *tatapaʔ*- 'old worn garment'), *tapāč*-, 'coral,' *tapasol*- 'nest,' *tapayašin* 'small toad,' *tapayol*- 'ball,' also 'thick,' *tawī* interjection 'oh!'. The word *tačkaw* 'first, principal' = *t(o)-ačkaw*. Now most if not all of these stems are typically Nahuatl in general structure and appearance. It is quite in line with this fact that *ta* is found almost entirely as initial, including initial reduplications—entirely so if we except *ta* in clusters and *itaka*-, which may be the result of a cluster or a prefix. Similarly in Aztec *ʃa* is overwhelmingly predominant as initial and relatively uncommon in other positions; and this situation is the natural reflex of two facts: (1) that many Uto-Aztecan roots begin with *ta* while comparatively few have *ta* internally, (2) that Nahuatl builds up a vast number of derivative words from various roots with the aid of a prefix *ta*-, which in the Aztec dialect is of course *ʃa*-. Hence the above words in initial *ta*- have the appearance of being Nahuatl but not Aztec, that is, loan-words which the Aztec dialect has taken from the other or "t" dialects. In fact *tačitowia(n)* and *tamačīwa* are rather definitely earmarked as *t*-dialect words, and perhaps as much might be said of the root *tapa*-, which looks like a dialectal form of the Aztec root

ʔapa- 'break.' There are a number of these *t*-dialects of Nahuatl, which have no *ʔ* but *t* instead, outlying and adjoining the area of the Aztec dialect.⁷ Since there was more or less political affiliation, free intercourse, commerce, and a common culture over the whole Nahuatl area, we might expect such loan-words, especially for names of plants, animals, and commercial products like coral coming from outside the Aztec or *ʔ*-dialect domain.

Hence the statistically small scattering of exceptions to the complementary distribution of *ʔ* and *t* in Aztec are all or practically all proved to be due to special laws or principles operating within Aztec, and do not invalidate the general truth of a dichotomy of one elementary sound-type into *ʔ* and *t*. Even if more exceptions be found, they must needs be few in number and not disturbing to the entire case. And even if *ʔ* were discovered in another Uto-Aztecan language it could have no effect on these facts, and would have to be interpreted as a separate development. We may conclude that *ʔ* or *tl* is purely a local development in the Aztec or Central Nahuatl dialect from Uto-Aztecan **t*, and does not represent an original distinct sound of Uto-Aztecan.

One point of interest in this study to the anthropologist who is not primarily interested in linguistics as such, but is perhaps interested in it as a precise methodology that deals with a certain realm of culture, may be the fact that a combination of synchronic and historically reconstructive techniques is necessary to a solution of the problem, and that either technique alone would be helpless.

WETHERSFIELD, CONNECTICUT

⁷ Certain present-day Nahuatl dialects that do not contain *ʔ* evidently once did, as they have *-l* corresponding to final *-ʔ* but *t* corresponding to *ʔ* before vowels. An example of one is given in Kroeber, *Uto-Aztecan Languages of Mexico* (Ibero-Americana 8, 1934). Such dialects are derived from Aztec or old Aztec, and are to be distinguished from original *t*-dialects of Nahuatl.

PRIMITIVE LAW AND PROFESSOR MALINOWSKI

By WILLIAM SEAGLE

I

OF all contemporary anthropologists who have not only deplored the neglect of primitive law but have undertaken to remedy the condition, the most stir has certainly been made by Professor Bronislaw Malinowski whose *Crime and Custom in Savage Society*¹ has already become almost as celebrated as Maine's *Ancient Law*. Although Malinowski is in many respects entirely opposed to Maine's views, they really have much in common. Both have a penchant for epigram and system building. Indeed Malinowski's is the more wilfully brilliant, and has achieved an even greater triumph than Maine in deriving the general from the particular. Malinowski's work is devoted to only one Melanesian people inhabiting the Trobriand Islands of the northeast coast of New Guinea, but the Trobriand Island material is treated in such a manner as to become virtually a systematic exposition of the whole of primitive law. "Primitive law," observes Malinowski somewhat too confidently at one point, "is not a homogeneous, perfectly unified body of rules, based upon one principle developed into a consistent system."² In the hands of Malinowski, however, primitive law is shaped into a far more homogeneous system than even the most talented jurists have been able to make of any modern legal system.

Few anthropologists have understood as well as Malinowski the motivating forces of primitive social behavior. He rejects the theory of the primitive belligerency of mankind together with the myth of the primitive mind. He has a fine contempt for the collection of anthropological curiosities. He recognizes the flexibility and adaptability of primitive social institutions and the peril of attempting to describe them in terms of modern conceptions. Above all he has the merit of rejecting the idea that the whole of primitive life is dominated by religious and magical conceptions and influences so that the savage is unable to distinguish religious from other rules of conduct. In expounding his views, he often creates, to be sure, an impression of originality where it does not always exist, for some of the dogmas he attacks have often been rejected by other anthropologists. But this, after all, is only a happy knack. Far more serious is the fact that in the end he becomes the victim of his own virtues. Not only the mind but the institutions of primitive man become undistinguishable

¹ London and New York, 1926 Unless the contrary is indicated quotations from Malinowski are from this work to which hereafter only the page references will be given

² Page 100.

from those of civilized man, and the search for truly legal forms and mechanisms is virtually abandoned. The revolt against the collection of anthropological curiosities leads to the opposite extreme of system building. Although he warns against the application of modern legal conceptions to primitive institutions, he often does not heed his own warning. Not content with distinguishing between religious and secular rules of conduct in primitive society, he further differentiates this body of secular rules into "moral," "legal," and "customary" rules, and legal rules into "civil" and "criminal" rules.

The centre of Malinowski's attack is upon the sovereign reign of custom in primitive society. His thesis is that the early tendency to regard the savage as lawless has been succeeded by a contrary tendency to regard him as the automatic slave of custom. This "dogma of the automatic submission to custom" assumes that the savage never seeks to break forth from his chains. Moreover, to the savage his customs are undifferentiated, all equally obligatory and inescapable. The "cake of custom" is unbroken—an "amorphous mass of tribal usage." Thus the savage is supposed to be unable to recognize personal and individual rights but to live in a state of primitive communism. Second only to the dogma of automatic submission to custom is the dogma of the unity of the clan, the only reality of primitive society, the only bearer of rights and duties.

According to Malinowski, however, the savage is actually as much of a scofflaw as the American of the prohibition era. The slave of custom can be guilty even of the breach of exogamy—the most dread commandment of primitive society, provided, however, that the degree of relationship is not too close. He can commit adultery with civilized casualness. But to emphasize his proclivities in these directions is to give a false impression of primitive society. It has not only its prohibitions but positive obligations. It is a great mistake to emphasize "criminal law" in the life of a primitive people. Primitive society has "civil law" as well as "criminal law," the normal as well as the abnormal. This civil law consists of "a body of binding obligations, regarded as a right by one party and acknowledged as a duty by the other, kept in force by a mechanism of reciprocity and publicity inherent in the structure of their society."³ But this publicity is far less important than the reciprocity, which is indeed overshadowing. This reciprocity is established in the very "cancatenation of the obligations" in a primitive society which needs no other sanctions than this. Individual rights and duties exist even in the operation of the supposedly communal fishing canoe. Reciprocity has its base in the economic life of the com-

³ Page 58.

munity. The people of the coast catch fish while those of the interior grow vegetables, and the process of exchange involves a necessary mutuality of obligation. The criminal law is no less supported by the very processes which govern the society. The culprit openly charged with breach of exogamy is driven to a ceremonial suicide. Sorcery may also be used in enforcing the rules of tribal law. Sorcery and suicide are thus "legal influences." Such are "the factors of social cohesion" in a primitive people.

II

Putting aside for the present such problems as primitive communism and the unity of the clan, what is to be said of Malinowski's conception of primitive law? In the first place he completely misunderstands the "dogma of the automatic submission to custom." There has been a time when the savage was regarded as "lawless" but there has never been a time when he was regarded as the helpless slave of custom. Malinowski contends that the dogma has dominated Hartland, Rivers, and to some extent Hobbhouse and Lowie, and while passages may be quoted from their works which seem to support the contention, they do so only when they are read out of context. Thus Lowie does observe that "Generally speaking the unwritten laws of customary usage are obeyed far more willingly than our written codes, or rather they are obeyed spontaneously."⁴ But neither Lowie nor any other anthropologist of repute has ever taught that the savage was enslaved by his custom. For intertwined with such unguarded remarks are usually to be found examples galore of breaches of custom. The very practice of the blood feud and composition is proof thereof. All that the remarks as to the *automatic* force of custom imply is that somehow, marvellous to relate, the savage recognizes the obligatory character of his customs although they are not backed by specific judicial sanctions of a repressive character as in civilized society. In this sense the custom of the savage is certainly automatic, and while he may behave as if he were its slave, he is not a slave who never revolts. Hartland, who is one of the chief butts of Malinowski's wit, really holds much the same conception of primitive law as himself.⁵ After poking fun for many pages at the dogma of automatic submission, Malinowski finally cannot himself escape the obvious fact that

⁴ *Primitive Society*, chapter on "Justice," page 387 of the English edition, quoted by Malinowski at page 13.

⁵ *Primitive Law* (London, 1924), page 137. Hartland's definition of primitive law is: "In such communities law is not the act of a sovereign whether an individual or body of men: it is the traditional rule of the community; and it is enforced not by a sanction prescribed ad hoc by the sovereign, but one that is *involved in the beliefs and practices of the community*" (italics mine).

numerous breaches of primitive custom have been constantly recorded. But strangely enough he simply brushes them aside as proof of the "over-emphasis of criminal law in primitive communities," and remarks somewhat cryptically: "Absolutely rigid rules cannot be stretched or adapted to life. They need not be enforced—but they can be broken."⁶ In the end he rather unguardedly falls himself into the trap. Describing the consequences of the utterance of a formula of exile, he observes that "anything but *immediate compliance* with a ritual request is *unthinkable* for a Trobriand Islander."⁷

The most serious weakness of Malinowski's views of the factors of social cohesion in a primitive tribe lies precisely in his inability to grasp the automatic sway of custom. He cannot bring himself to believe that a custom may be obeyed *merely because it is the custom*. He cannot understand that custom may have a force of its own. As an anthropologist Malinowski has fashionably proclaimed himself a "functionalist," whose aim must be to eschew survivals and to study beliefs, customs, and institutions as living, working mechanisms of any given society. Since every custom must have a specific purpose, some justification must be discovered for its existence. Granted that this is true of many customs, it is not necessarily true of all. Moreover, custom is itself an institutional mechanism, and to become engrossed in individual custom is to slight the function of custom itself. The origin of many individual customs is often very obscure, but infinitely more so is the basis of custom itself. It presents to the social psychologist as difficult and mysterious a problem as that of "instinct," of which it is perhaps a phase. Yet that the purpose of custom is to promote uniformity of conduct, to establish a basis of social agreement, would seem to be clear enough. Custom must be conceded to be sovereign if it is to discharge its social function.

But seeking the *quid pro quo* of social relationships Malinowski has been inevitably led to the construction of the system of reciprocity. As it happens he has been singularly fortunate in selecting the locale for his theory. In the first place, the Trobriand Islanders have a dual organization, i.e., the tribe is divided into "moieties."⁸ Moieties are usually exogamous divisions

⁶ Page 56

⁷ Page 104 (*italics mine*)

⁸ As to the nature and function of the moiety, see R. H. Lowie, *Primitive Society* (New York, 1920), Chap. 6 and authorities there cited. At page 133 Lowie remarks, "A striking feature of moieties is the development of reciprocal services . . . It is a puzzling question how this reciprocity is to be interpreted. Is it fundamentally a matter of the moiety or merely incidentally so because either moiety includes one of the parents?" From this it would seem that Lowie

but the purpose of the moieties may have nothing to do with marriage. Malinowski's assumption is that the dual organization plays a fundamental role in the economic organization by making possible the system of reciprocal services. But the dual organization is not universal among primitive peoples. There may be three or five divisions, or none at all, as is generally the case in Africa. Hence Malinowski is compelled to assume that the dual organization was originally universal. He is thus driven to commit a cardinal sin for a critical anthropologist: he has made a generalization as to origins! Particularly fortunate, however, is Malinowski in the fact that in the Trobriand Islands the dual organization is bound up with a system of dual division of labor. The coastal people catch fish while the inland people raise vegetables, and the fish are regularly exchanged for the vegetables. Thus the system of reciprocity works with absolute precision.

The whole trouble is, of course, that not all primitive peoples regularly exchange fish for vegetables. The material culture of some primitive societies may be so rudimentary that no regular system of exchange is possible, and the economic self-interest which is presumably the most important base of reciprocity has hardly any scope. In more advanced primitive societies reciprocity is more regularly expressed in the very forms of legal transactions, and it is doubtful whether "contract" as such exists, i.e. whether obligations are recognized as binding irrespective of mutuality or preexisting obligation.⁹ However, the forms of the obligations are probably determined less by the elements of reciprocity than by the limitations of the legal system with respect to enforcement. In any event in both primitive and civilized societies reciprocity is entirely absent, at least in any economic sense, in the institution of the gift.¹⁰

Moreover reciprocity is involved only in civil obligations. In the case of "crimes" or "wrongs" it is difficult to see any element of reciprocity

had rejected the system of reciprocity a considerable number of years before it was elaborated by Malinowski.

⁹ This point is made by A. S. Diamond in his recent book, *Primitive Law* (London, 1935), a brilliant work, which unfortunately, however, swallows the theory of reciprocity hook, line, and sinker. Moreover Diamond disregards the problem of economic grade in relation to the possibilities of reciprocity. The Trobriand Islanders are a people of the second agricultural grade while Diamond discusses reciprocity in relation to even lower economic grades.

¹⁰ So obvious is it that a gift does not fit into the system of reciprocity that in *Crime and Custom* (pages 40-41) Malinowski hastens to retract a statement made by him in his earlier *Argonauts of the Western Pacific* (London, 1923). He confesses that he was there so foolish as to describe as "pure gifts" what a husband gave to his wife, or a father to a child. He now declares that the "system is based on a very complex give and take, and that in the long run the mutual services balance." All this probably amounts to is that primitive people, like most civilized people, do expect some more or less tangible return in the future in making gifts.

unless it is upon Hegel's theory that the criminal craves to be punished. The creation of an equivalence between the extent of a wrong and its reparation has been one of the objectives of the evolution of punishment, but this equivalence cannot be psychologically identified with reciprocity. Other elements than reparation enter into civil liability for tort. It is to be suspected that Malinowski himself has come to realize that he has somewhat overworked the system of reciprocity. While in *Crime and Custom* it is pushed to such an extreme that even a widow's mourning for her husband becomes a legal obligation (because she receives a ritual payment),¹¹ in a later essay he remarks apropos of the subject: "There is no doubt that from the point of view of a slave, a sacrificial victim, or a piece de resistance in a cannibal banquet, the balance is heavily topped against him."¹²

III

The central problem of "primitive law" may be said to be whether in the absence of political organization and of specific juridical institutions such as courts and codes, certain modes of conduct may be segregated from the general body of custom as at least incipiently legal. The qualifying adjective "primitive" in the term primitive law should at least suggest that it is not quite law. The test of law in the strict sense is the same for both primitive and civilized communities, i.e. the existence of courts. It is nevertheless necessary to seek the embryonic forms and mechanisms which have developed into juridical institutions. Malinowski correctly envisages the problem,¹³ but his functionalism leads him inevitably not only to slight the role which form plays in the identification of legal institutions but almost to identify the legal with the sociological. Not even all sociological

¹¹ Pages 33-34.

¹² Introduction to H. I. Hogbin, *Law and Order in Polynesia* (London, 1934), page xxxiv. The introduction is not so much an act of piety towards a disciple as an attempt by Malinowski to reply to a growing number of critics.

¹³ Malinowski has been unjustifiably taken to task in some quarters for disregarding the African material in his treatment of primitive law. Of all the conventions of cultural anthropology none has had so unfortunate a bearing upon discussions of primitive law as the indiscriminating application of the adjective "primitive" to all peoples who do not employ writing. Not all "primitive" law is really primitive. Complex legal institutions may exist although writing is unknown, as among the Ashanti of West Africa, and, conversely, legal institutions may be relatively undeveloped although writing is known and in common use, as among some of the ancient culture peoples, e.g., the Chinese, the Greeks, and the Jews. Both legislation and many complex forms of legal transactions are possible without writing. The former may be accomplished by means of proclamation while the latter may be readily consummated by means of ceremonial acts before witnesses.

mechanisms, however, are necessarily legal. The functionalist method of interpretation proves far too much. The "binding" nature of an obligation comes finally to depend not only upon social but personal sentiments of approval and disapproval. The legal explorer then becomes lost in the bogs of the psychological theory of law which is based upon the experience of a legal "emotion" with respect to certain relations.

It is the essence of Malinowski's theory to distinguish between "binding" custom and "neutral" custom. The binding customs are "law" while the neutral customs are—well, customs. But apparently the only criteria for distinguishing the two classes of customs depend upon the vitality of the interests involved. The custom is neutral if nobody much cares whether it is observed or not. Presumably the particular manner in which a loin cloth is worn is to be regarded as a neutral custom. But are there any really neutral customs in any society? Does not the distinction contradict the very conception of custom? A very trivial custom may have a tyrannical hold, which cannot be explained in terms of its functional importance. There are involved here subtle considerations of degree which can hardly be made the basis of a classificatory principle. It might be expected from the stress upon the vitality of interest that few customs would prove to be "legal." But paradoxically enough Malinowski's procedure has enabled him to discover vastly more law than custom in primitive society.

Malinowski is quite justified in supposing that even the most primitive people distinguish supernatural from secular rules of conduct. The secular and religious are not in a hopeless jumble in the primitive mind. The problem of differentiation here is not insoluble—both for the savage and the anthropologist. The sanctions for religious or supernatural rules are automatic as far as human beings are concerned. They will be executed upon them by the unseen powers. But secular rules are backed by social forces present in the mundane world. The confusion begins to arise only when human beings begin to assist the process of divine intervention. When this assistance has taken the form of definite secular penalties in this world, the rule is at least to be regarded as both secular and religious, although the general tendency is for the religious element to lose steadily in importance or even disappear altogether. The manipulation of the unseen powers through ritual or magic to bring about tangible results in this world does not constitute a secular process. Some form of manipulation is always necessary to activate supernatural forces.¹⁴

¹⁴ The widespread belief fathered by Maine that law developed from religion has been attacked with great success by Diamond. It is more correct to say perhaps that religion is one of the competitors of law. It is relatively late in his existence that God becomes the Supreme

For this reason sorcery, although it may be a very potent force in primitive society, is not properly to be regarded as a "legal influence" even when it is used as a method of retaliation for injuries in this world. Malinowski is very confusing on the subject of the role of sorcery in the maintenance of legal institutions. He regards it apparently as both a method of law enforcement and as an instrument of crime.¹⁵ Thus he observes that sorcery "being levelled at established institutions and important pursuits," it is "really an instrument of crime." But then "black magic acts as a genuine legal force: for it is used in carrying out the rules of tribal law." Again, "sorcery, in fine, is neither exclusively a method of administering justice, nor a form of criminal practice. It can be used both ways, though it is never employed in direct opposition to law." Yet Malinowski himself cites cases of magic used "to destroy crops, to thwart a fisherman, to drive pigs into the jungle," and the like. It is no wonder that he finally resorts to the traditional prefix of conceptual confusion and remarks upon "how difficult it is to draw a line between the *quasi*-legal and *quasi*-criminal applications of sorcery" (italics mine). In fact no such line can be drawn with any degree of confidence until the practice of sorcery has become a crime punishable by summary execution of the sorcerer by the social group,¹⁶ which occurs almost universally in primitive societies at an early point in their development. Until then some forms of magic may be "white" and others "black," but the judgments implied are purely moral. It is only the native's profound and terrible belief in the sorcerer's powers that tempts the anthropologist to believe that he is dealing with a secular sanction.

When, however, may secular custom be differentiated as legal? Malinowski's distinction between neutral and effective custom is the basis of his insistence that all primitive peoples have "civil" law as well as criminal law. But his rich discovery of this civil law is a tribute only to his own gifts

Lawgiver. Tabu has often been regarded as one of the origins of law, but the penalty for the infringement of a tabu is *automatic*. It produces a situation inherently dangerous to the violator but it has no external sanction, either religious or temporal. In those cultures, however, in which the breach of a tabu has come to be regarded as contagious, the tendency is for the community to support the tabu with secular penalties. But then it has really ceased to be a tabu.

¹⁵ The following quotations in the text relating to sorcery are to be found in pages 93-94.

¹⁶ In connection with sorcery Malinowski refers to certain powers of the chiefs. They apparently had armed guards who would kill anyone who insulted them. They also employed magic against certain "offenders." Presumably, however, the chiefs had no political power and sorcery was not a "crime." The power of the chief is one of the most crucial questions for legal development, it is characteristic of Malinowski's method that he barely touches upon the subject.

of psychological identification. He has only committed, indeed, what may be described as the pathetic fallacy of primitive jurisprudence. He has transferred to primitive law the legal emotions of his own culture. He has simply sought in primitive society those institutions which *in the modern world have come to be the subject matter of legal obligation*. He has selected the *customs* relating to marriage, inheritance, and property, and pronounced these to be *primitive law*. The identification of law by its subject matter constitutes really one of the tacit assumptions of the psychological theory of law. Indeed, it must be admitted that even the modern has experienced difficulty in regarding new and startling subjects of legislative regulation as entitled to be called "law," as, for instance, freakish sumptuary legislation.¹⁷ The legal historian should be grateful, to be sure, that certain subjects have been standardized as legal. Nevertheless it should not be forgotten that law is perhaps the only field of knowledge which has no subject matter of its own. Unlike the natural scientist, the poor bewildered jurist must be ready to face the whole of experience.

A far safer guide to the unfolding of law is available in primitive ceremonialism. Publicity, which is treated by Malinowski as a stepchild, deserves far more attention than even reciprocity. It is indeed the crucial test of vitality of interest and the differentiation of custom. It is only when transactions have come to be consummated according to special forms that they may be said to be on their way to legal recognition. Primitive man is already familiar with many varieties of religious ceremonial. He need only invent forms for secular use, or adapt religious forms to new purposes. Yet curiously the ceremonial transactions of the Trobriand Islands receive but scant attention from Malinowski. The few "specific legal arrangements"¹⁸ described by him, such as the ceremonial exchange of public expostulation, are accompanied, moreover, by a warning that they are not "exclusively legal," and that the "main province of law is in the social mechanism."

In one sense some ceremonialism may perhaps be described as preventive process. It is employed not only to found an obligation but to lessen the likelihood of subsequent dispute. In primitive society it takes the place of the formal written document. Even where no compulsory process is avail-

¹⁷ I have published a collection of freak legislation, *There Ought To Be A Law* (New York, 1933). Apropos of this little volume Malinowski generously remarks in his introduction to Hogbin at page lxviii "The sociologist can learn more from a serious perusal of Mr. William Seagle's humorous anthology *There Ought To Be A Law* than from whole volumes of pedantic legal philosophy." I am profoundly grateful for the compliment but I should not draw quite the same conclusion from the material.

¹⁸ At pages 60-61.

able, organized ceremonialism already bespeaks participation of the community. In primitive society there may be process although there is no court. The function of a court is fundamentally to determine an issue of fact. But in primitive society the facts are less frequently disputed. Or if they are disputed, no proof may be possible, in which case resort may be had to the procedure of the ordeal. It is in the organized forms for conducting and disposing of the dispute itself that primitive law comes to definite expression. It is in the process of retaliation that custom is shaped into law. Breach is the mother of law as necessity is the mother of invention. In terms of the modern juristic convention that distinguishes between "objective" law and "subjective" law, primitive law may be said to be subjective law par excellence, for it is never the command of a politically organized state but always a positive assertion of individual right manifested in the public assertion of a claim under traditional forms of procedure. Mere occasional personal resentment is not enough.

It is perhaps the most serious defect of Malinowski's work that he refuses to admit the overshadowing importance of breach in the formation of legal institutions. Indeed he treats the idea as absurd, and derives a great deal of innocent merriment from a statement of Radcliffe-Brown that repeated breach of tribal custom may be a source of law.¹⁹ Yet the latter is only restating one of Maine's soundest and most brilliant generalizations that in the evolution of law the course of development is from judgment to custom to legislation.²⁰ Malinowski deplores the emphasis upon "prohibi-

¹⁹ See his article in the *Encyclopaedia of the Social Sciences*. Malinowski comments in his introduction to Hogbin, page xxiv "But let us remember that the writer of the article has made a definite distinction between custom and law; he has based his definition upon law being sanctioned and custom not; and here he tells us that repeated breaches of custom result in its being sanctioned by penalties; that is becoming law. This would obviously imply that a custom if sufficiently often broken becomes a law, a *reductio ad absurdum* of the whole argument." It is difficult to see the *reductio ad absurdum* in the description of this evolutionary process. In any event the reader will look in vain in Radcliffe-Brown for any statement that the difference between law and custom is that the one is sanctioned and the other not. He clearly states that *both* are sanctioned although *in different ways*. Law is declared to be characterized by the availability of politically organized sanctions. Another basis must therefore be sought for any *reductio ad absurdum*. This may perhaps be found in Malinowski's distinction between "neutral" and "effective" custom. We might just as well distinguish between "law" and "real law" in organized legal systems, for not all legal prescriptions are sanctioned equally well. Specific performance may be superior to damages as a remedy.

²⁰ Maine, of course, overgeneralized but he is nevertheless often unjustly criticized. If his generalization as to the evolution of law is to be taken to imply that no "primitive" peoples (using the adjective in its conventional sense) have legislation, or that primitive custom is always and invariably litigious in origin, it is, needless to say, untrue. But it is by no means clear that these are its necessary implications. It is frequently forgotten that Maine was really generalizing about ancient law rather than primitive custom.

tions," on negative injunctions, the glorification of "criminal" law in primitive society; in other words the selection of the abnormal rather than the normal. Law is to him rather mysteriously "baited with inducements" which are difficult to discover. But in truth law throughout its history has been almost always concerned with the abnormal. It has had its origins in the pathology of social relationships, and flourishes only when there are frequent disturbances of the social equilibrium. It may be true, as Malinowski sarcastically observes, that "we have not yet designed a euphorimeter or dysphoriscopes"²¹ to measure the extent of the disturbances of social harmony. But neither have we yet designed a reciprocimeter to measure the forces of interest and vanity. A far better distinction than that between "effective" and "neutral" custom would be a distinction between "neutral" and "belligerent" custom.

It is the fundamental tenet of Malinowski's theory that law exists in even the simplest of primitive societies. More anthropologists will agree with him than jurists. He has lately, however, shown a tendency to call a truce, to regard the whole dispute as a squabble over words. He is willing to speak of "effective" custom rather than primitive law.²² But, alas for the prospects of peace, the dispute is not merely definitional. Only confusion can result from treating custom and law as interchangeable phenomena. If custom, as it has been contended, is in the truest sense of the term automatic, law is the product of organized force. A phenomenon partaking of the order of the latter differs in a profound *functional* sense. The state is an alien and oppressive power. Its courts, as they develop, destroy the intimacy and naturalness of customary behavior patterns. They tend to substitute for them a series of gross fictions, and artificial and stereotyped forms and processes. Often the courts of politically organized society find themselves powerless to recognize obligations which presented no difficulties as components of primitive custom.²³ We are confronted here with Dean Pound's "inherent limits of legal action."

²¹ Introduction to Hogbin, pages xxiv-v

²² Introduction to Hogbin.

²³ Thus early courts refuse their compulsory process for the enforcement of incorporeal rights, and legal historians have assumed therefrom that the distinction between corporeal and incorporeal rights is a characteristic of mature law. In the attempt to explode this idea by showing that many primitive peoples recognize property rights in songs, legends, magical formulas, and names (see in particular Professor Lowie's *Incorporeal Property in Primitive Society*, Yale Law Journal, Vol. 37, 1928, p. 551) there has, however, been overlooked the fact that these examples of "incorporeal" rights exist only in societies in which political and judicial differentiation is embryonic. Such rights are thus customary rights, and it remains true to say that early law deals harshly with them. It might also, however, be asked whether a recognition of a distinction between "corporeal" and "incorporeal" rights is psychologically demonstrable

IV

The confusion of law and custom is only the cardinal sin of the muddled conceptualism which prevails in the treatment of all phases of primitive institutions. A great part of Malinowski's argument depends upon his conception of criminal law, but, like all anthropologists and even many jurists, he makes no effort to grapple with the distinction between "civil" and "criminal" law. He falls victim again to the pathetic fallacy of primitive jurisprudence, and even while he recognizes that crime in the Trobriand Islands can be but "vaguely defined," he proceeds to include murder and theft in the list of primitive crimes. Now while breach of exogamy and sorcery have come to be recognized as "crimes" in many primitive societies, ordinary murder and theft are acts that never constitute crimes in any truly primitive society. They are acts for the individual kin to settle as best they may. Hence if the distinction between civil and criminal law is to be accepted at all with reference to primitive society, murder and theft must be pronounced purely "civil" wrongs.

Naturally Malinowski is as vague in his treatment of "punishment" as of crime. "The most important methods," we are told, "in fact, are a bye-product of *non-legal* institutions, customs, arrangements and events such as sorcery and suicide, the power of the chief, magic, the supernatural consequences of taboo and *personal acts of vindictiveness*" (italics mine).²⁴ Not only is the legal here not separated from the non-legal but not even the institutional from the purely personal. Due respect has already been paid to Malinowski's treatment of the supernatural agencies of punishment. He is equally confusing in his comments on suicide. Conceivably a ceremonial suicide may constitute an organized although self-inflicted form of punishment, and as such has not been unknown in the legal institutions of some civilized nations. But Malinowski reports not only a case of suicide following a breach of the serious crime of exogamy, but also cases of suicide consequent upon adultery and connubial insult. The result is that one begins to be sceptical about the institutional nature of any of the cases.

Sir Henry Maine noted the strange preponderance of criminal over civil law in ancient jurisprudence which has so distressed Malinowski, but he at least qualified the observation by remarking "the penal law of ancient communities is not the law of Crimes, it is the law of Wrongs, or, to use the English technical word, of Torts." In the interest of removing the under-

in these societies. Is it necessary to transfer these rights because they are recognized as "property," or because, being magical, a ceremonial payment is required to make the magic effective? Moreover must not a spiritual rapport exist between transferor and transferee?

²⁴ Page 98.

lying confusion, Radcliffe-Brown has suggested the abandonment of the classic distinction between civil and criminal law and the substitution of a distinction between "public delict" and "private delict."²⁵ His definition of these terms show, however, that he has not improved matters very much. "Public delict" turns out to be only a synonym for "crime" as it is conventionally defined in treatises on criminal law! As a matter of fact, however, it is extremely difficult to distinguish civil from criminal law even in mature legal systems. It can be done neither in terms of procedure nor of sanctions. Criminal law is a breach of public law but there is no absolute formal distinction between public and private law. The classic dualism is founded primarily in economic individualism. The "civil law" becomes associated with private property rights, and there is really no civil law until there is a market in which things are exchanged as abstract "commodities" rather than as concrete objects of use. The criminal law betokens a state monopoly of force. Actually in both the civil and criminal law the will of the state is objectified. The only difference is that in the civil law the state acts as umpire in a dispute between two litigating integers who are "private" persons, while in the criminal law it may be necessary for the state itself to supply one of the integers in the form of a public prosecutor. The stake differs, too, in that while in one case it is a "commodity," in the other it is a "penalty." In the absence of both the state and the market the distinction between civil and criminal law is altogether without fundamental basis.

In dealing with "ownership" Malinowski sins even more grievously. Even in the law of the culture peoples such an abstract conception as ownership is a relatively late development. If modern legal categories are to be applied to primitive institutions it would be better to consider control over chattels as a phase of the "law of persons," for the chattel is considered only as an extension of the person. Malinowski, to be sure, recognizes the inappropriateness of applying the conception of ownership to primitive relationships, but he then proceeds to employ the conception for the purpose of demonstrating the recognition of "individual rights among savages," and to repel the insidious notion that "primitive communism" obtains in his beloved Trobriand Islands. He thus has his cake and eats it! In the endless debate as to the existence of an aboriginal communism, the disputants have often revealed very little understanding of the nature of the institution. Many anthropologists have triumphantly proceeded to demonstrate the irrelevant truth that there is no people so primitive but

²⁵ In his article on "Primitive Law" in the *Encyclopaedia of the Social Sciences*, *supra*, note 19.

that it does not at least recognize the right to private possession of personal chattels. Yet even Morgan, one of the crassest of the evolutionists, contended that primitives did not recognize individual rights to things.²⁶ No more does communism imply any absence of individual rights and obligations. It implies only the communal ownership of the means of production. Since the means of production are relatively slight in a primitive society, apart from the utilization of land, the emphasis should perhaps also be placed upon the communal disposition of the means of consumption.²⁷ A degree of "communism," in the sense in which most anthropologists seem to use the term, exists in every society, even in modern capitalism. In no legal system have rights of property ever been absolute. There are always the rights of escheat and eminent domain, and restraints on alienation. These are particularly common in primitive societies, and it is very much to be doubted whether private property in one sense of the term may be said to exist where the right of alienation is denied or seriously restricted. In terms of any legitimate conception of communism Malinowski's Trobriand Islanders are very much under its sway. The fact that among them the ownership of the fishing canoe involves a complex of rights, claims, and privileges does not in any way affect its communal character. For one thing the "owner" of the canoe cannot refuse its use, which makes him a strange owner indeed! Many anthropologists have attacked "primitive communism" by garnering evidence from individualist peoples. It has remained for Malinowski to attempt to do so with evidence from an authentic communistic people! The reason is easy to understand. It is difficult to trace the exact workings of reciprocity in a collectivist society. The *quid pro quo* is the ideal of an individualist society.

Malinowski is both at his best and at his worst in attacking the dogma of the unity of the clan, which is closely related to that of primitive communism. The dogma does not necessarily imply that there are no individual relationships *within* the clan. It is only in its external relations that the fiction is at all maintained that the clan is a unity. It might just as well be said that the modern dogma of the unity of the State implies an absence of individual rights and duties. Conceivably also there may be conflict

²⁶ See Bernhard J. Stern *Lewis Henry Morgan: Social Evolutionist* (Chicago, 1931), page 179. "But the fact is often overlooked by his critics that he recognized that individual rights to personal property prevailed even in the simplest cultures."

²⁷ Thus there is a widespread sharing of food in very primitive societies. But even where individual claims to hunting grounds are recognized, the arrangement may really constitute no more than a division of labor. The primitive conception of the situation may run less in terms of the land than the products of the land. The primitive attitude is very important and even competent observers may disagree.

within the clan but this is not necessarily a universal condition in primitive society. As in dealing with reciprocity, Malinowski is again fortunate in his selection of the Trobriand Islanders who, according to his own account, are in a state of transition from mother-right to father-right. He attempts to show that in the evasion of claims of property and inheritance, as well as in the obligation of the blood-feud, the father desires to align himself with his son rather than his nephew, who is his successor according to the principle of mother-right. But this source of conflict would not exist in all clans, and in any event it is not clear why the acceptance of composition in lieu of the blood-feud should be treated as a form of evasion. It is true that a conflict of lineal loyalties might strengthen the inclination to composition.²⁵ But this means of appeasement has its own attractions and is primarily to be associated with a sufficient growth of material goods to regularize the practice. Nevertheless, Malinowski's emphasis upon the factor of conflict is all to the good. The role of breach is at least displayed with reference to the primitive "criminal" law. It should be noted, however, that Malinowski speaks here throughout of "systems of *law* in conflict," when it would be more accurate to speak of a conflict of law and contrary custom. Generally speaking Malinowski, like most laymen, takes legal dogmas too seriously. Discovering that the legal fiction does not correspond with the sociological reality, he is so shocked that he loses all respect for the legal fiction.

The dogma of the unity of the clan derives from another of Maine's soundest distinctions: that between kinship society and political society. It is easy, however, to understand why Malinowski is disposed to minimize the unity of the kinship group. The kinship principle as a bond of social cohesion is the most powerful one in primitive societies which have not achieved political organization. Hence it is a dangerous rival of the principle of reciprocity. The solidarity of the kinship group, in the absence of judicial institutions, is essential to the maintenance of peace. Now if the traditional view that primitive law deals primarily with reparation for wrongs is justifiable, the overwhelming importance of the kinship principle becomes apparent. Law is a needless luxury in gentile society: it knows the golden age before law begins. Before the peace of the King is the peace of the Kindred.

Malinowski, of course is not the only anthropologist who is a peril to

²⁵ It is interesting that Hobhouse also suggests that among the factors mitigating the blood-feud is the existence of "marriage classes cutting across local groups and building up complex relations between group and group" (*Morals in Evolution*, London, 1915, page 107). So, too, Steinmetz in his *Ethnologische Studien zur ersten Entwicklung der Strafe* (2 vols., Leyden, 1894).

the jurist. But his views of primitive law in particular have challenged the most fundamental conceptions of jurisprudence. The eternal question, "What is Law?" appears in anthropology in only a slightly different guise: "Is there law among primitive peoples?" The answers of Malinowski have done as much as the most ardent legal realists to all but remove the boundaries between law and jurisprudence. His functionalism, indeed, has exercised a pervasive influence upon modern jurisprudence, and he is really entitled to be regarded as the sire of the realists, neo-realists and ultra-realists. The *reductio ad absurdum* of his method would be to disregard entirely the formal structure of law in modern society in favor of the study of the social processes from which they derive. Reciprocity is in force in civilized communities, too, but at least until recent years nobody confused purely social with formal legal relationships.²⁹ The value of law to society has undoubtedly been greatly exaggerated, but the way to judge of its importance is not to misunderstand its essential nature. Forms have a function, too. In neglecting them the functionalist betrays his own cause.

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²⁹ The most recent and ardent disciple is Professor William A. Robson, who in his *Civilization and the Growth of Law* (New York, 1935) virtually achieves the *reductio ad absurdum* of Malinowski's functionalism. He becomes so befuddled that he can hardly tell the difference between scientific laws and human laws, and is led to denounce poor John Austin's analytical jurisprudence as "an unpleasant brew" prepared from "characteristically Victorian ingredients" such as compulsion and the fear of punishment. Professor Robson is anxious to establish his thesis that there is a close relationship between the increasing rationality of men's ideas of the universe and the nature of legal ideas and institutions. He naturally seizes upon Malinowski's functionalism as triumphant proof of this progressive process. But to balance his antitheses he is compelled to dwell for a long time upon the irrationality of primitive law and to engage in the very assemblage of anthropological curiosities so deplored by Malinowski. But primitive custom is at least as rational as archaic and even mature law. Thus, as not infrequently, the disciple betrays the master.

STRUCTURAL ANALYSIS OF KINSHIP

By KINGSLEY DAVIS
AND W. LLOYD WARNER

A SURVEY of the history of kinship theory reveals that its main tradition, beginning with the monumental work of Morgan and extending to the present-day contributions of men like Professor Radcliffe-Brown, has been interested not so much in kinship *per se* as in kinship as an index of something else. Morgan's interest in evolution has given place to an interest in social organization, so that the attention in kinship is now focused upon relations between kinship and other aspects of social structure, but the fact remains that until today the central stream of thought on the subject has essayed an internal analysis only when forced to it by insistent criticism of alleged external correlations.¹

The one outstanding exception to this tradition is Professor Kroeber's article on the eight categories.² This article, published nearly thirty years ago, is undoubtedly a significant contribution to kinship theory, yet in all the time since its publication neither its author nor anyone else has built upon it. Perhaps the reason lies partly in the fact that Dr Kroeber included in the article a statement of his peculiar theory of psychological as opposed to sociological determinism in kinship. No connection seems to exist between the two parts of the paper, except that Kroeber's disbelief in external correlations probably put him in the frame of mind necessary for making a logistic internal analysis. At any rate the critics, notably Rivers,³ have roundly condemned the psychologism but ignored the categorical analysis. The latter, though widely cited, has never, so far as we know, been developed, and has been seldom utilized in classifying systems.

So far as can be judged, the central tradition in kinship theory has reached a point where it must devote some attention to a purely internal analysis. It is therefore our intention, if we can, to advance a step farther along the path indicated by Kroeber in his categorical study. We are interested in formulating the principles upon which kinship rests and upon

¹ For a brief résumé and interpretation of the history of kinship theory in anthropology see Kingsley Davis, *A Structural Analysis of Kinship* (Ms., Doctoral dissertation, Harvard University, 1936).

² *Classificatory Systems of Relationship* (Journal, Royal Anthropological Institute, Vol. 39, pp. 77-84, 1909). Recently, B. W. Aginsky has attempted an internal analysis, evidently headed in the same direction as the present paper. See his *Mechanics of Kinship* (American Anthropologist, Vol. 37, pp. 450-57, 1935).

³ W. H. R. Rivers, *Kinship and Social Organisation* (London, 1914). Kroeber himself has made a half-retraction in *California Kinship Systems* (University of California Publications in American Archaeology and Ethnology, Vol. 12, No. 9, 1917), pp. 388 ff.

which any intrinsic classification of concrete systems must accordingly be based.

Kinship is a concept that touches two levels of phenomena. On the one hand it refers to a relatively fixed biological structure, on the other to a relatively variable social pattern based on this biological structure. Hence the place to look for universal elements is on the biological plane. Then an endeavor can be made to find out how these elements are used to build the different concrete sociological structures represented by terminologies. For this purpose it will prove necessary to describe the nature of kinship terms and to invent a symbolic apparatus for the convenient representation of categories. It is hoped that this discussion may illuminate a few controversial topics, among them the question of classificatory and descriptive systems; but we are not for the present concerned with societal evolution, geographical distribution, or group behavior. We are concerned rather with the groundwork for a comparative sociology of kinship.

BIOLOGICAL STRUCTURE⁴

1. *Birth and the Birth-Cycle.* Reproduction seemingly offers the key to the biological structure, and indeed to all kinship; for kinship may be defined as social relations based on connection through birth. This holds for relationships by affinity as well as for those by consanguinity—for although husband and wife may have no recognized common ancestry, they are nonetheless related by blood through their common offspring. Even relatives by adoption are relatives only in so far as they are treated *as if* they belong to the family by reproduction. Wherever one finds two relatives, no matter which two, there are one or more births (real or fictitious) connecting them. It makes no difference what conception of reproduction the particular culture may have.⁵

Birth is not an instantaneous thing, but a process. Copulation, gestation, and care after parturition are all definite parts of it. Birth is also a different thing to different persons concerned in the process. Every normal individual, for example, participates in at least two species of birth: first when he himself is born, second when he procreates another individual. The essential point to note is that between participating in the first kind and participating in the second, there is a cycle of time. This cycle, ranging

⁴ This section purports to deal only with biological elements, but since the distinction is purely theoretical it will be impossible at all times to exclude the cultural.

⁵ Malinowski has shown the universal presence of some idea of reproduction which, no matter how fantastic, nevertheless serves as the core of kinship. See *The Family Among the Australian Aborigines* (London, 1913), pp. 168-207.

normally from fifteen to forty years, may be conveniently called the birth-cycle. It is the most important constituent, and the relationship to which it gives rise—parent-child—the most fundamental connection of kinship structure.

2. *Siblings and Sibling-Link*. Siblings are individuals born from the same mother, same father, or both. The relationship between them constitutes a structural unit of great importance.⁶

3. *Siblings and Birth-Order*. The interval of months or years that, except with twins and triplets, normally intervenes between the birth of siblings we shall call birth-order, as distinct from birth-cycle. It is not so significant as the other elements.

4. *Two Sexes*. Another element in kinship, and a highly important one, is the ubiquitous presence of sex similarity or dissimilarity between individuals. The manner of its use, however, is different from that of the birth-cycle and birth-order.⁷

5. *Procreative Union*. Indispensable to the birth process, union between the sexes implies more than mere copulation. It implies cooperation in nourishing the offspring after gestation and parturition, thus indicating a relationship between the partners cemented by other organic ties than sheer sexual satisfaction.⁸

6. *Five Categories*. We have now described five structural units of the biological level, four of major, one of minor importance. Obvious as they seem, they provide the skeleton around which to build a systematic conception of kinship. Every terminological system will be found to embody

⁶ Compare with Radcliffe-Brown: "In Australia the whole social structure is built up on the recognition that the most significant and permanent social relations between individuals are those between parents and children and between children of the same parents" (*Social Organization of Australian Tribes*, Oceania, Vol. 1, pp. 34-63, 206-46, 322-41, 426-56, 1930-31), p. 437.

⁷ The difference lies in the fact that the other categories are more integrally related to the birth process and the family constellation, whereas sex cuts across all these and extends even to non-relatives. A person is of the same or different sex from *anybody* he meets, but he is not a sibling, a parent, or a mate of anybody he meets. It is true, also, that the other categories make it possible to radiate out from Ego to distant relatives. Thus one can speak of the number of birth-cycles and sibling-links separating Ego from his grandfather's brother. But sex similarity or difference does not in and of itself constitute such an organically related chain. Sex is a *trait* which individuals have, the other categories are more easily regarded as *relationships* between specified relatives. However, with the other categories of relationship given, sex similarity and difference can and does become an additional factor in relationship, and can itself be regarded as a relation.

⁸ The role of so-called economic bonds in marriage has been insisted upon by Malinowski, Lowie, Radcliffe-Brown, and Westermarck, in contradistinction to Morgan.

them, and yet each system will give recognition to them in its own characteristic way; just as each type of gasoline motor combines, but combines uniquely, the same principles of combustion engines. They thus furnish an intrinsic basis for distinguishing between kinship *types*.

7. *Space and Position*. The biological structure apparently has two axes, vertical and horizontal. The vertical axis is constituted, first, by the chain of birth-cycles. Ego looks up to his ancestors and down to his descendents. The birth-cycle is the measuring unit: he looks up one birth-cycle to his father and all members of his father's generation, and down one birth-cycle to his son and all member's of his son's generation. (Generation, in fact, is a vertical concept, for the difference between contiguous generations is one birth-cycle.) The vertical axis is constituted, secondly and less importantly, by birth-order. Ego looking up to his elder and down to his younger brothers. It will thus be seen that the common ingredient of the vertical axis is time—time as measured in the genetic process.

Constituting the horizontal axis are three relationships, viz the sibling-link, sexual union, and sex similarity and difference. Ego looks *across* to his brothers and cousins, *across* to his mate, and *across* to individuals of the same and opposite sex.

(a) All collateral lines are connected with the central line at some point by a *sibling-link*. To know how many collateral lines out a relative is, it is essential to know where this link occurs. We do not know that a person is a "second cousin" until we know that his line is connected with Ego's by a sibling-link at a point two birth-cycles above Ego, namely in the grandfather generation. It is therefore not enough to know *that* a sibling-link occurs in the line of connection, but also one must know *where* it occurs. In so far as this is true we see that the horizontal character of the sibling-link is not unconnected with the vertical axis; for the same "time" that is used in measuring the vertical distance is also used in measuring the horizontal distance.

(b) *Procreative union* gives a horizontal character to kinship structure in two ways. First, it creates a bilateral separation between one's own relatives and one's mate's relatives. Ego looks out upon his wife's relatives, and upon relatives of his children through sexual union. Second, the horizontal nature of procreative union lies in the nature of the case. If it be disputed that people of the same generation (i.e. occupying the same niche on the vertical axis) can generally unite to procreate more suitably than people of different generations, and that it is easier to keep the mechanics of kinship straight when both mates are of the same generation, it is nevertheless true (and this is the more vital consideration) that in the

very nature of procreation *husband and wife are equated for all descending generations, since they are father and mother (i.e. parents) to their children*. Even though they originated in two different generations, they are both one birth-cycle removed from their children, two birth-cycles removed from their grandchildren, etc. Also for ascending generations, once two people unite in procreation they tend to be regarded as on a level.

(c) *Sex similarity and difference* serves as a universal, variegated basis for the building of kinship patterns. At every point of articulation the relative or connecting relative is either man or woman, and it makes a difference which. As you move away from Ego in the structure, points where sex makes a difference accumulate in geometric ratio. Sex differentiation, like procreative union, has a horizontal rather than a vertical connotation. Reason for this is not hard to find. No time concept is involved. The sexes co-exist; there is in their differentiation no before and after. This is not true of the birth-cycle difference between generations or the birth-order difference between siblings. The difference between male and female is not a spread up and down but a spread to left and right. The sex difference between mother and father, for example, acts as a spread in the horizontal field. Ego looks up to his father's kin on the left, to his mother's kin on the right, much as a man looking at a "Y" from the middle up.

8. *Summary and Outline*. The five categories constitute the foundation principles of kinship structure. Each is a relationship based upon both a common element and a difference. Each can be used as a unit of structural measurement. All of them relate to the birth process and are found in the biological constellation of the immediate family. Their outstanding characteristics can be outlined as follows:

VERTICAL AXIS

- 1) Unit = *Birth-Cycle* (symbol "C"). Time between a person's own birth and his procreation of another.
 - (a) Relationship involved is that between procreators and procreated, which is generalized to include the relation of one generation to the next.
 - (b) Distance up or down is measured in terms of the birth-cycle, beginning with Ego's (zero) generation.
- 2) Unit = *Birth-Order* (symbol "O"). Time between birth of one sibling and birth of another.
 - (a) Relationship is that between successive offspring of the same parents, which is generalized to include all members of zero generation older or younger than Ego, all members of first ascending generation older or younger than Ego's father, etc.

- (b) Distance up or down measured in terms of the birth interval, which almost universally means simply the order of older and younger.

HORIZONTAL AXIS

- (3) Unit = *Sibling-Link* (symbol "L"). Space between two persons born from same parents.
- (a) Relationship is that of two people with common progenitors.
 - (b) All collateral lines branch off from central line at some point by a sibling-link.
- (4) Unit = *Procreative-Union* (symbol "U"). Connection between two persons cooperating to bear children.
- (a) Relationship is that of two persons with common descendants.
 - (b) Tends to double the area of relationship in the ascending hemisphere.
- (5) *Sex-Difference and Similarity*. Unit = either difference or similarity (symbol "S"). Space between two persons of opposite or like sex.
- (a). Relationship is in one case that of two persons of same sex, and in second case that of two persons of opposite sex.
 - (b) Significant at every articulation point of the structure. It thus includes, as subcategories, the sex of Ego (S^*), the sex of intermediate relatives (S^i), and the sex of the relative (S^r).
 - (c) This category has a different character from the others. It does not serve, like the others, to set one family group off from another.

TERMINOLOGICAL STRUCTURE

1. *Social Space*. In the preceding discussion of biological space, it was implied that a relative could be located and the distance to him measured in relation to Ego. Such location and measurement is simple. It is done, like measurement on any other plane, by locating the point with regard to both the vertical and horizontal axes. By ascribing one unit to our four major kinship relations (birth-cycle, sibling-link, procreative-union, and sex-difference), the position of any relative in the total kinship structure may be plotted. (See Figure 1.)

But no one could pretend that such measurement correctly gauges *social* distance. Every concrete system of sociologically related kin diverges from the biological distances, sometimes moderately and sometimes radically. Among most cultures possessing the avunculate, the mother's brother is closer to male Ego socially than the father's brother,⁹ yet the

⁹ This is true among such tribes as the Omaha, the Hopi, the aborigines of British Columbia, the Torres Straits Islanders, the Anglo-Luo of Upper Guinea, and many peoples of

distance in terms of biological units is greater. In societies where descent and inheritance are patrilineal, the social distance between Ego and his mother's relatives is usually greater than between him and his father's relatives, though for given relatives on each side the biological distance is the same.

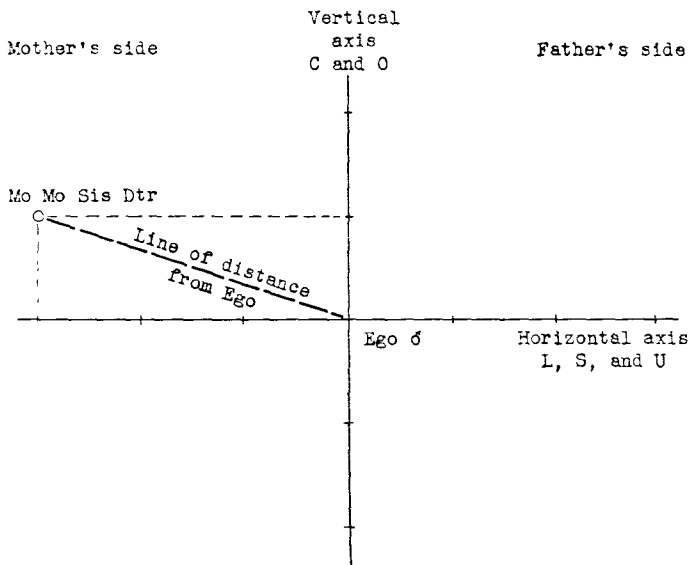


FIG. 1. Location of a relative in biological space. On the vertical axis the mother's mother's sister's daughter is located at the first ascending generation, because in order to trace the connection to her one must go up two birth-cycles and down one. On the horizontal axis, she is three units out. This figure is obtained by giving a numerical value of two to a sibling-link when it occurs in the second generation removed from Ego, and a numerical value of one to an alternation of sex. In this case Ego is assumed to be a male, the relative a female. This figure is a rough illustration of how kinship distance may be measured in the biological structure.

It is true, however, that taken statistically a rough approximation exists between biological and sociological kinship distance. The reason is clear. In so far as social distance is based upon kinship (and this is the only kind under consideration), it has to be based upon the underlying biological relationships implied in the birth process. It may select certain of these relationships and lay emphasis upon them while ignoring others, yet the chances are that those who are most closely related through the birth process are also those most closely related socially. This is especially true

Oceania. Consult Lowie, *Primitive Society* (New York, 1920), pp. 82 *et seq.*, and Rivers, *History of Melanesian Society* (Cambridge, 1914), Vol. 2, pp. 17 *et seq.*

for the immediate family. It grows less true in the outer reaches of kinship space, where greater variety of combination and emphasis is possible and actual contact is not biologically necessary.

Limitations do not permit in this place a treatment of the complicated problem of social space and social distance. It is doubtful that a systematic statement of this subject can be given unless behind it lies a sociological system of thought. Ultimately, in fact, kinship as a social phenomenon must itself be interpreted in the light of a full-blown theory of society. In the present tentative stage of our kinship theory, we prefer to confine our efforts to *terminological* space. It is assumed that kinship terminology, being the instrument in which people *think* about kinship, is closely related to social organization.¹⁰

2. *The Terminological Translation of Biological into Social.* How does terminology relate to the biological structure of kinship? This is for the present our basic question. Its answer must hold the key to the classification and comparison of systems.

Language symbolizes only a part of reality, selecting various elements and putting them together to form an experiential world. In studying terminological systems it is therefore important to know what elements are left out and what elements included, and on what principle. We know that every kinship nomenclature embodies only a part of the actually existing biological structure, that it seizes upon certain elements of the biological organization and ignores others in constructing a socially experienced world. In this way terminology bridges the gap between the biological and the sociological levels.

Not every terminology, however, selects the same elements. Different nomenclatures employ different principles of selection. Here, then, is the basis for a comparative typology of kinship systems. It is our belief, along with Professor Kroeber, that the elements selected are precisely the categories of relationship. The manner in which a terminology selects and combines these categories into a coherent conceptual whole is the thing that differentiates that system from other systems.

¹⁰ This close relationship, one may truthfully say, has been proved beyond a doubt by Rivers, Lowie, Radcliffe-Brown, and others. See also W. L. Warner, *Morphology and Functions of the Australian Murngin Type of Kinship* (American Anthropologist, Vol. 32, pp. 207-56, 1930; Vol. 33, pp. 172-98, 1931).

Worthy of mention is the fact that Ego thinks of kinship in terms of vertical and horizontal axes. He thinks of time as vertical and of the other relations as horizontal. Why this should be is not altogether clear, but the fact remains. He (his culture) sees the birth series as some sort of line—a succession of fathers and sons as a road, a ladder, a rope, the trunk of a tree, whereas the collaterals are branches, arms, etc. Often Ego thinks in terms of analogy with his own body—the trunk being the line of descent, and the limbs the collaterals.

3. "*Classificatory and Descriptive.*" A controversy has long endured over the traditional distinction between so-called classificatory and so-called descriptive systems. Morgan, who first elaborated the distinction, was diffuse and vague, and subsequent authors have only partially dispelled that vagueness. It is alleged, on the one hand, that the descriptive nomenclature designates particular relatives, while the classificatory classifies a number of relatives under one term. Rivers, for example, maintains that in the most complete form of the classificatory system there is not one single term of relationship used to refer to one person and to one person only, whereas in our own system there are six such terms—husband, wife, father, mother, father-in-law, and mother-in-law.¹¹ In extreme classificatory systems every term applies to a *class* of persons.

Kroeber, on the other hand, considers the dichotomy between descriptive and classificatory systems to be fallacious.¹² Every language, he maintains, groups together under single designations many distinct degrees and kinds of relationship. Our word *cousin* for example denotes thirty-two different relationships; our word *brother* classifies four. The distinction between the two kinds of systems, he holds, has its origin in the point of view of investigators who, on approaching foreign languages, have been impressed by their failure to distinguish relationships which European languages do discriminate. They forget that their own languages have "entirely analogous groupings which custom has made so familiar and natural that they are not felt as such."

The striking thing about this controversy is that both sides are apparently right. It is true that what have been called classificatory systems habitually group more persons under one term than what have been called descriptive systems. But it is equally true that systems of the latter kind have very few terms that designate only one individual, and the number is so small as scarcely to justify characterizing a system by their presence. On the other hand, Dr Kroeber seems correct in his contention that most terms in any kinship system include not one but several relationships, and that this is even truer of the alleged descriptive systems than the so-called classificatory ones.

This controversy therefore seems obviously based on different conceptions of the point at issue. It is one thing to say that a term designates one and only one person, and another thing to say that it designates one and only one relationship. Any two kinsmen have more than one relationship subsisting between them. Father and son, for example, are related to each other by a single birth, separated by only one birth-cycle. They are also of

¹¹ *Kinship and Social Organisation*, p. 2.

¹² *Classificatory Systems of Relationship*, p. 77

the same sex; they are, on the negative side, not related through procreative union or through a sibling-link. A term may express all these facts or relationships, and yet designate only one individual. Dr Kroeber was thinking of relationships when he wrote his article; Rivers was thinking of persons when he wrote his book. Both were following enlightening leads, but neither seemed to gain anything from the other. By further analysis it may prove possible to reconcile both points of view and emerge with a more exact solution of the issue.

4. *Isolating Terms.* When a term designates one person and only one person, it can be called "isolating." It is possible, in any terminology, to count the isolating terms. In English there are six. Now the characteristic of an isolating term is certainly not that it designates one relationship. Just the contrary, it must designate enough relationships to place a person precisely on the kinship map. The principle is the same as the location of a point in three-dimensional space. One coordinate will not locate a point, nor two. It takes three to do it. In kinship we found that there were two axes constituted by five categories. In order to locate a person in the structure so that he alone, and nobody else, is the individual meant, it takes a knowledge of all five categories. Our word *father*, for instance, tells us at once that the designated person is one birth-cycle up from Ego, that he is on the vertical axis (no sibling-link), that he is masculine rather than feminine, and that he is not married or directly connected to Ego by procreative union. (Monogamous marriage makes irrelevant the birth-order category in this instance.) So specifically located is he by terminological recognition of these biological facts that there cannot be two persons filling the bill. There can be only one. With our term *uncle*, however, the case is quite different. We know that he is one birth-cycle up from Ego, that he is one sibling-link over from Ego's line in the horizontal plane, and that he is of the masculine sex; but we do not know the sex of the relatives connecting him with Ego or if he is connected by marriage. Thus he may be either the father's or the mother's brother; or indeed he may be the husband of one of their sisters. Nor, assuming that he is a parental sibling, do we know what his birth-order is among the other parental siblings. The term is not specific for one person, but may designate a great many.

It stands to reason that if we are to use the term "classificatory" to cover all terms which are not specific for one person, then every kinship nomenclature in the world is preponderantly classificatory. On the other hand, if we choose to call any term classificatory which names more than one category of relationship between persons, then nearly all kinship terms again turn out to be classificatory, because most of them signify more

than one category of relationship between Ego and the relative. It is only a very general term like child or grandparent which narrows down to only one category of relationship between Ego and the relative; and obviously these general terms embrace a great number of persons—purely because they do not specify all relationships.

The two conceptions of "classificatory" are incompatible. The more different relationships one groups under one term, the fewer individuals will be covered by that term; while the more individuals covered by a single term, the fewer must be the relationships implied in its meaning. If you include enough relationships in the meaning of one term, one individual will be designated; while if you designate only one relationship, the number of individuals indicated will be tremendous. "Classificatory" cannot logically mean both the grouping under a single term of several relationships *and* the grouping under a single term of many relatives. The first conception goes contrary to the whole tradition of thought associated with the subject. If it were accepted, we would have to transfer all the former classificatory systems to the descriptive camp, and all the descriptive systems to the classificatory group. Furthermore, the second conception (though traditional) still leaves most of the terms in any system classificatory.

In any language the number of terms that designate only one individual—isolating terms—is extremely limited. In the extended kinship structure the awkwardness of trying to give the specific set of relationships applying to each individual is appalling (though some systems, such as the Erse, approach this ideal). One of my second cousins would have to be called by some such combination of terms as "my father's father's second youngest sister's son's second but first female child." Or else each specific relative would have a unique name which was understood to express each and every one of his relationships to Ego. This would require, by conservative estimate, six hundred separate terms.

That this is never done is shown by looking at the birth-order category. Practically no system gives full expression to birth-order. At best it is usually merely a classification of siblings into older and younger. Consequently, most relatives are not distinguished from each and every one of their siblings.

5. *Summary: Isolating, Descriptive, and Classificatory Terms.* In view of the controversy over so-called classificatory and descriptive nomenclatures, and in the light of the preceding discussion, the following concepts are proposed:

Isolating—those terms which designate one person only. Specific in regard to all five categories.

Descriptive—those terms which are specific for all categories except birth-order, so that the only other persons that could be meant are the designatee's siblings of the same sex. Example: son in English. Such terms are definite with regard to birth-cycles, sibling-links, sex of relative and intermediate relatives, and procreative union occurring anywhere in the line of connection.

Classificatory—those terms which are not specific for one or more of the four major categories. Inevitably if this is the case several persons (not merely siblings) will come under the term. Example: uncle, which fails in definiteness concerning sex of intermediate relatives and procreative union.

Using these definitions it is possible to decide whether any given kinship term is isolating, descriptive, or classificatory. One can therefore count the terms in any system that fall under each heading. If one system has more descriptive and fewer classificatory terms than another, it is more descriptive, and *vice versa*. The concepts "descriptive" and "classificatory," as applied to an entire system mean simply the proportion of terms, in comparison to other systems, which give definite expression or fail to give definite expression to the four major categories of kinship.

LINGUISTIC DEVICES, PRIMARY, CATEGORICAL, AND COMBINED TERMS

Frequently in the history of kinship theory, descriptive systems have been defined as those in which a set of primary terms is used to designate members of the immediate family, these terms then being combined to describe relatives in the extended system (e.g., *fodur-brodir*, father's brother, in Icelandic).¹⁰ But is this common criterion of descriptiveness invariably correlated with the other major conception—namely that a descriptive system classifies fewer relatives under one designation? The answer is, no. The Chinese system, for example, is descriptive to a greater degree than the Indo-European descriptive systems; yet it uses an entirely different linguistic technique, reminiscent of the Hawaiian style. The use of primary and combined terms is probably a convenient method for descriptive systems, but in the last analysis it is merely a linguistic device which almost any kind of relationship system may employ. It is extrinsic to kinship logics, a question of linguistics or word structure rather than of semasiology or word-content.

Not every kinship word used in combination, however, is a primary term.¹¹ One frequently finds kinship terms made up of a combination of

¹⁰ Morgan, *System of Consanguinity and Affinity of the Human Family* (Smithsonian Contributions to Knowledge, Vol. 17, 1871), p. 12; Rivers, *Kinship and Social Organisation*, pp. 77-79; Lévi-Strauss, *Culture and Ethnology* (New York, 1917), p. 104.

words, without the words ever being used independently to designate kin.¹⁴ It is necessary then to make the following distinctions.

1. *Primary Terms*. These are single kinship terms that can be used alone to designate a relative and also combined with other primary or categorical terms. Almost invariably applied singly to members of the immediate family, they are combined for most relatives outside the family. For instance, ahar (father) and drihar (brother) are single terms in Erse. They are combined to designate uncle in the following way, drihar m'ahar (brother of my father), which means, logically: this person is to my father as my brother is to me. In other words the relationships of the immediate family are combined to describe the precise set of biological relationships connecting Ego to the extended relative. Hence the connection with descriptive systems; but note that it is merely a *convenient* method, not a *necessary* one for this type of system. It must be remembered that *some classificatory systems*, such as the Kariera, *have nothing but single terms*.

2. *Categorical Words*. These are not usually, by themselves, kinship terms. They are generally combined when used. They are, as a matter of fact, fundamentally different from primary terms because, instead of symbolizing one concrete *set* of relationships between Ego and a given relative, they symbolize merely one category or unit of relationship. For this reason we choose to call them *categorical* words. In Hawaiian, for instance, the word makua means simply "one birth-cycle up from Ego." It can be combined with either kana (another categorical word meaning male) or waheena (female) to mean "father" or "mother." This linguistic device is certainly not limited to so-called classificatory systems. All that is required is enough categorical words in combination to denote any relative specifically. As a matter of fact many of our Indo-European systems use categorical words to some extent (e.g., English "grand" and "in-law").

3. *Combined Terms* are those formed by combination of other elements. Examples: mac drihar m'ahar ("son of my father's brother") in Erse; wai tsu fu ("grandfather on mother's side") in Chinese; and grandmother and father-in-law in English.

A SYSTEM OF KINSHIP SYMBOLS

In analyzing terminologies with reference to the way they represent the biological categories, the really important questions are: How many cate-

¹⁴ By "kinship *term*," as distinct from "kinship *word*," we understand any word or combination of words employed as a unit to designate a relative

¹⁵ In Hawaiian such linguistic elements as moo, moopuna, kupuna, kolu are never used separately to designate relatives, and most of the other elements are not so used.

gories of relationship does each term in the nomenclature express? How many categories does the system as a whole rely upon? How does the system combine the categories and subcategories on which it does rely?

An indispensable aid in gaining this knowledge is some sort of symbolical apparatus by which the categories and subcategories may be represented. Once this is achieved it is possible to write the formula of any kinship term, to sum up the characteristics of an entire system. With this in view the following set of symbols has been adopted.

(1) *Birth-Cycle* = C

Birth-cycle ascending = C^a (above Ego)

Birth-cycle descending = C^d (below Ego)

Total number of birth-cycles, ascending and descending = C^{a+d}

Generation = C^{a-d} (Number of descending birth-cycles subtracted from number of ascending. The resulting difference can be known without the exact numbers involved in the calculation being known.)

Generation ascending = $C^{a-d=+}$

Generation descending = $C^{a-d=-}$

Certain number of generations from Ego in either direction = $C^{a-d=\pm}$

(2) *Birth-Order* = O

Exact number in a series of siblings = O^n

Older sibling = O^o

Younger sibling = O^y

(Note: Where O is used in a sibling series of which Ego is not a member, it will prove useful to designate in what generation the series occurs, as O^y^a , or to put by the O the number of the relative with whom comparison is made, as O^y^{16} .)

(3) *Sibling-Link* = L

Brother-brother = $L^{\sigma^{\sigma}}$

Sister-sister = $L^{\varphi^{\varphi}}$

Brother-sister = $L^{\sigma^{\varphi}}$

Sex of siblings identical = L^u

Sex of siblings different = L^a

(Note: It may seem that the sex category is here being confused with the sibling-link category. But it has been pointed out that the sex category is not quite the same type of relationship as the others. It is sometimes more convenient to represent it as part of the symbol of another category.)

Point where sibling-link occurs = L^w

Point where sibling-link occurs unknown = L^x

(4) $Sex = S$ Sex of Ego = S^e Sex of Ego male = $S\sigma^e$ Sex of Ego female = $S\varphi^e$ Sex of relative = S^r Sex of relative male = $S\sigma^r$ Sex of relative female = $S\varphi^r$ Sex of intermediate relatives = S^i Uniform = S^{iu} Alternating = S^{ia} All male = $S\sigma^{iu}$ All female = $S\varphi^{iu}$ Male, female, female, etc. = $S\sigma^r\varphi^r\varphi^i$ Uniform with sex of Ego = $S^{i(ue)}$ (5) *Procreative-Union* = U (a) When in the line of connection = U

(Note: When it stands in the line of kin connection, it is symbolized simply as U and should be placed in the formula at the point where the union actually occurs in the line. When it indicates whom the relative marries, potentially or *de facto*, this general fact is symbolized by U^w and it should come at the end and be always modified by a small symbol.)

(b) When indicating whom the relative marries = U^w With Ego = U^e With kinsman = U^k With non-kinsman = U^x Potential = U^p Potential with Ego, etc. = U^{pe}

These symbols enable one to describe in convenient formulæ exactly which categories a term recognizes or expresses, and which ones it ignores or fails to express. Our term father is definite with regard to all four of our major categories. We know that the person designated by this term is one birth-cycle above Ego (i.e. that he is C^a), that he is of the male sex ($S\sigma^r$), and that he is married not to Ego but to Ego's mother. Stated in symbolical form the term is definite with regard to $CLSU$, merely leaving out O .

The first thing we can say about kinship terms is that they either express categories or fail to express them. If they express a category, they tell us that this particular kind of relationship prevails or does not prevail between Ego and the given relative. If they fail to express a category, they

fail to tell us whether this relationship subsists or not. In the one case the meaning of the terms with regard to a given category is definite, in the other case indefinite.

But note that a term can be definite in two ways. It may state that a person has a certain kind of relation to Ego, on the other hand it may imply just as definitely that he does not have this kind of connection with Ego. In other words a term may be definite in a positive way or in a negative way. Both the positive and negative meanings are important. The term *father* is of course negative with regard to *L* and *U*. It asserts merely that the relative is not connected to Ego by these units. Yet this negative meaning contained within the term serves to place *father* with regard to these categories just as definitely as the positive meaning places him with regard to the other two categories, *C* and *S*. In other words in locating a person in the kinship structure, negative and positive meanings are of equal importance.¹⁶ It is only when a term leaves us in ignorance of whether a certain category of relationship obtains or does not obtain between Ego and the designated individual that it (the term) can be called to that extent indefinite or non-expressive.

It often happens that the positive meaning contained in a term is exactly the same as the positive meaning contained in a homologous term in another system; yet due to a difference in the negative meaning, the two terms are totally different. Thus our term *father* is definite in a positive manner, as shown by the following formula: $C \cdot S\phi^{\pm}$. The term for "father" in *Kariera*, *mama*, expresses exactly the same positive formula: $C \cdot S\phi^{\pm}$. Yet the two terms are utterly different. The English *father* is a descriptive term according to our definition, while the *Kariera mama* is a classificatory term. The difference thus lies in the negative meanings. *Father* is definitely not related by a sibling-link, while *mama* in *Kariera* may or may not be. The latter term is indefinite with regard to the category *L*.

A SUGGESTED METHOD OF ANALYZING KINSHIP SYSTEMS

By going through a relationship system and writing down the formula for each of its terms, the student of kinship will find it easy to observe the outstanding characteristics of the system and state them in explicit, even quantitative form. He can specify how many terms in the system recognize which categories, define the average term in the system, and compare the specific features of this system with the features of other systems. Since

¹⁶ This requires modification. The negative meanings of terms are ultimately determined by the positive meanings of other terms in the system. Thus in a certain sense the positive meanings are the fundamental ones.

nomenclatures vary minutely and multifariously, it seems that this method of detecting small details and slight variations will make it possible to construct a classification that will embrace not only broad general types but an entire range of shaded subtypes. This should enable the student to correlate with greater precision types of nomenclature with types of societies, and particular features of kinship with particular features of social organization.

But the preliminary spade-work of symbolic analysis is not easy. Before writing the formula of a kinship term one must already know something about the entire terminology. Kinship terms, being parts of systems, have scarcely any meaning when considered apart from other terms.

In practice it has proved necessary to go through several preliminary steps before actually making use of the symbols. It will be noticed in the accompanying sample table that there are ten columns. The first and fourth columns contain (except for birth-order) the biological positions—these being numbered in the first column and given an English equivalent in the fourth column. The first step in analyzing a system accordingly consists in listing the native terms in Column 3, beginning with relatives in the immediate family and next those in the second and third degrees. Whenever a native term appears for the first time in the list, it should be given (in Column 2) the number to which it corresponds in Column 1. When the same term reappears, it should not be listed, but should be given its original number. In this way the analyst can soon dispense with the native terms and work only with numbers. These numbers (in Column 2) can be jotted down in their proper places on a chart like Table 1. This chart is merely a graphic outline of the biological facts in consanguinity; when the numbers are filled in at their proper places on it, it is easy to look at the chart and write the formulæ for the various terms.

Not one but three formulæ are necessary for each term. First it is advisable to write (in Column 6) the exact and full meaning a term conveys, down to the minute details. (See sample, Table 1.) Much use of subcategories is naturally required. This formula describes how accurately the kinship term places the person or persons it represents. In the next column are written the general categories in regard to which the term is definite. The third formula column (Column 8) states in a general way which categories are left indefinite; it is obtained by subtracting the definite categories from the total number of categories.

When this procedure has been followed the stage is prepared for the final analytical work. It is easy to glance down the formulæ columns and note which categories are consistently definite, which consistently in-

definite. It is often possible to write the formula for the average term in a system, and inevitably basic differences between systems reveal themselves.

TABLE 1. SELECTED TERMS FROM ARUNTA

1 Fixed number	2 Term number	3 Native term	4 English equivalent	5 Female speaking	6 Complete formula	7 Categories definite	8 Categories indefinite	9 Distance from Ego	10 Unit distance
<u>1</u>	1	Kata	<u>Father</u>		$C^A-d=1g(1u)(rd)U^2$	$C^A-dg1rU$	C^Ad1S^a	C+	1+
<u>2</u>	2	Maia	<u>Mother</u>		$C^A-d=1g(1u)(rq)U^1$	$C^A-dg1rU$	C^Ad1S^a	CS^a+	2+
<u>3a</u>	3	Itia	<u>Elder brother</u>		$C^A-d=01S(1u)U^6/18$	$C^A-d1S1U$	C^Ad1W^aer	C+	1+
<u>3b</u>			<u>Younger brother</u>						
<u>4a</u>	5		<u>Elder sister</u>						
<u>4b</u>			<u>Younger sister</u>						
<u>5</u>	5	Pala	<u>Husband</u>		$C^{2a}/2d/0S1aU^9/Ego$	$C^{2a}/2d/0S1U$	C^Ad1S^aer	$2CS^a+$	3+
<u>6</u>	6	Noa	<u>Wife</u>		$C^A-d=0S1aU^9/3/Ego$	$C^A-d1S1rU$	C^Ad1W^a	$4CS^a+$	5+
<u>7</u>	7	Alira	<u>Son</u>	21	$C^A-d=-1S1uU^15/71$	C^A-dg1U	C^Ad1S^aer	C+	1+
<u>8</u>	7		<u>Daughter</u>	21					
<u>9</u>	9	Aranga	<u>Father's father</u>		$C^A-d=*2S1uU^5$	$C^A-d=*2S1U$	C^Ad1S^aer	$2C+$	2+
<u>10</u>	5		<u>Father's mother</u>						
<u>11</u>	11	Tjima	<u>Mother's father</u>		$C^A-d=*2S1uU^12$	$C^A-d=*2S1U$	C^Ad1S^aer	$2CS^a+$	3+
<u>12</u>	12	Ebmata	<u>Mother's mother</u>		$C^{2a}+2d+2dS1aU^49$	$C^{2a}+*2S1U$	C^Ad1S^aer	$2C+$	2+

REMARKS CONCERNING THE CLASSIFICATION OF KINSHIP SYSTEMS

1. *Ideal-Types and the Theoretical Range of Kinship Variability.* At one pole of the kinship range are those systems in which nearly all terms are descriptive, at the other those in which nearly all terms are in some way classificatory.¹⁷ But at the absolute extreme on each end of the scale there are no actual systems. Perfect polar types exist only in the imagination.

If we tax our imagination and ask what a perfect classificatory system would be like, there is apparently a clear answer. A perfect classificatory

¹⁷ The authors regret that they have not yet been able to undertake the laborious work of classification except for a limited number of systems. While they feel that the number of samples is sufficient to give an empirical basis to this section, they think that a full treatment of the subject must await an actual classification on a world-wide scale.

system would consist of but a single term, this term being used to designate all relatives indiscriminately. This would mean complete terminological identity of all kinsmen, complete failure to recognize any of the categories of relationship, complete failure to express degrees of distance. Obviously no actual terminology approaches such classificatory perfection. Every system draws some distinction between individual kinsmen, recognizes some categories of relationship, and expresses some degree of distance between relatives. Yet there are nomenclatures that approach nearer the ideal-type than others, and it is this differential approximation that gives an opportunity for finely-drawn classification.

If we draw upon our imagination to picture the pure kinship system at the opposite pole, we again get a clear vision. The perfect type at this end would be, not merely a descriptive system, but a completely isolating system. Such a terminology would contain a unique term for every related individual, the number running into hundreds. This ideal-type is as far removed from actuality as the perfect classificatory terminology.

In between the two extremes there is still a third ideal-type. This is the perfect descriptive system. With the exception of the birth-order category, it would describe accurately the biological position of every relative. Some systems remotely approach this ideal,¹⁸ but most of them fall far short of it. Inevitably any descriptive system must break down in the outer reaches of the kinship structure. The main question concerns the nearness of this breaking point to Ego—that is to say, the diameter of the circle within which the nomenclature is descriptive.

The difference between the pure (fictitious) classificatory system and the pure (equally fictitious) isolating system, however absolute such a sharp difference may seem, is ultimately one of degree. They are both opposite ends of the same continuum, the fundamental principle of gradation being the degree and accuracy with which biological space is terminologically recognized. Any dividing line between classificatory and isolating terminologies, or between classificatory and descriptive, will necessarily be arbitrary. Indeed there is no particular point in drawing it, because the aim should be to distinguish not two types but many—observing differences all along the line.

2. *Sub-Types and the Criteria of Classification.* If the main types are distinguishable on the basis of the degree of recognition given biological kinship space, subtypes may be distinguished according to which category or categories are left indefinite. All systems, for example, that leave the sibling relationship indefinite (and thus emphasize the identity of brothers)

¹⁸ Erse, Roman, Chinese

can be put into one class; all that leave the vertical connection indefinite can be put in another, etc. Types may be further distinguished on the basis of how categories are combined—in the sense that a partial indefiniteness of one category may be combined with a partial indefiniteness in another. In this way a large number of variant types may presumably be detected and their differences precisely stated.

For purposes of illustration a comparative table is given (see Table 2) in which the outstanding traits of seven different kinship systems, chosen for their heuristic value, are juxtaposed. This table contains eleven points of comparison, some of them more important than others. In order to give something by way of a summary of what has been said and at the same time an explanation of the comparative table, these eleven features are separately discussed below.

(a) *Total Number of Terms.* The number of terms in a system is not always easy to compute. In most nomenclatures in which terms for extended relatives are combinations of primary terms, Ego can go on naming relatives as long as he can see the biological line of relationship. The number of terms for Erse and English, for example, is theoretically infinite. What the number actually used by the group may be is another question. In systems tending towards the classificatory side, however, where the terminological space does not correspond at all closely to the biological, a limited number of terms will be found to cover all possible relatives. The total number of terms therefore indicates something concerning the nature of the correspondence or lack of correspondence between biological and terminological space.

(b) *Number of Isolating Terms.* The greater the number of isolating terms found in a system, the greater the recognition given by that system to biological structure; because the presence of several isolating terms indicates immediately that at least for some relatives all biological categories are definitely expressed. The total absence of such terms indicates that the system brings outside relatives terminologically into (i.e. merges them with) the immediate family and is consequently to some extent classificatory.

Isolating terms are apt to be primary, but primary terms are not necessarily isolating. The linguistic device of primary terms is used almost invariably where there are isolating terms, but it is also used for terms not isolating. This can be seen by comparing items 2 and 4 of the *Kariera*, where it appears that in *Kariera* and *Arunta* there are many primary terms but no isolating ones at all.

TABLE 2. COMPARISON OF VARIOUS KINSHIP SYSTEMS

	ERSE	ENGLISH	KARERA	HAWAIIAN	CHINESE	ROMAN	ARUMTA
1. Total number of terms	Infinite	Infinite	21	31 (approximately)	350 (estimated)	112	21
2. Number of terms isolating	6	6	0	0	8	8	0
3. Number of descriptive terms	All except those in second ascending generation and up	12	0	2 (possibly)	Practically all	All except those connected through relatives in second ascending generation	None
4. Number of primary terms	9	12	21	2 primary 16 categorical	Same category not represented by same term throughout system. One categorical word often represents more than one category	24 primary 7 categorical words and prefixes	All
5. Vertical axis	Unlimited extension	Unlimited extension	No extension beyond second generation	Extended at least to fourth generation	Extended 5C in either direction	Extended 6C in either direction	No extension beyond second ascending generation
Category C	Completely definite	Completely definite	Ca-d (generation is definite with regard to odd or even)	Ca-d (generation definite)	Completely definite	Completely definite	Ca-d (generation definite)
Category O	Indefinite	Indefinite	O ^o and O ^y (definite in zero generation)	O ^o and O ^y (definite for members of zero generation for relatives of same sex as Ego)	Definite for zero and ascending generations for relatives through males	Completely indefinite	Indefinite
6. Horizontal axis							
Category L	Definite	Definite (except "cousin")	Indefinite (equivalence of brothers)	Indefinite (equivalence of siblings)	Definite	Definite	Indefinite (equivalence of siblings)
Category U	Definite	Definite except for two terms	Definite	Indefinite in eleven terms	Definite in most terms	Indefinite except U ^x	Definite
Category U ^w	Indefinite except U ^x	Indefinite except U ^x	Definite	Definite	Indefinite	Indefinite except U ^x	Unexpressed.
Category S ^e	Indefinite except in two terms	Indefinite except in two terms	Indefinite except in five terms	Indefinite in twenty terms	Indefinite except for a few affinal terms	Indefinite except in affinal terms	Brother and sister identified for descending as well as ascending relatives
Category S ⁱ	Definite except in second ascending generation and up	Indefinite for most terms	Definite (system depends on it)	Completely indefinite	Definite	Definite except for relatives related through second ascending generation or above	Definite (system depends on it)
Category S ^r	Definite	Definite (except cousin)	Definite except in five terms	Definite	Definite	Definite	Indefinite in terms
7. Main categories expressed. Formula describing the system	CLUS ⁱ U ^x	CLUS ^{U^x}	Ca-dS ^r U ^{C^oL^oX^oC^d}	Ca ^a -dS ^r	CLUS ⁱ r	CLUS ⁱ U ^x	Ca-dS ⁱ U ^{C^{2a}-2dL^oS^{ia}}
8. Main categories	OS ^e	OS ^e i	Ca ^a -dL	Ca ^a -dL ^s i	S ^a	OS ^e	LOS ^r
9. Distance	Definite in most terms	Definite in majority of terms	Indefinite	Indefinite	Definite in nearly all terms	Definite	Indefinite
10. Relation of immediate family to extended kin	Sharply distinguished	Distinguished	Merged	Merged	Sharply distinguished	Sharply distinguished	Merged
11. Linguistic devices	Primary terms of immediate family combined to describe biological position of extended kin	Same as Erse, except there are more terms that are primary outside immediate family	All terms primary designating merged kin	Categorical words combined to describe kindred	Categorical words combined to describe kin descriptively. Words sometimes used alone as primary terms	Primary and combined terms, but tendency to distort primary terms into categorical words in combination. Also a number of categorical words and prefixes	All terms primary designating merged kindred

(c) *Number of Descriptive Terms.* A count of the descriptive terms in a system gives some indication of how near it approaches to being a perfect descriptive system, and consequently how much of it (i.e. the remainder) is classificatory. The amount of descriptiveness of a terminology is in direct ratio to the number of descriptive terms it contains. But note that this tells one nothing about the *kind* of classifieriness exhibited by the non-descriptive terms; this fact is given only by items 5 and 6.

(d) *Number of Primary Terms.* The Erse and English nomenclatures use primary terms for immediate relatives, combine them to designate extended kin. This means that in each primary term several categories must be included, because all four major categories are required if the term is to express the concrete total relationship between two persons (e.g., in the immediate family). On the other hand, Kariera and Arunta have nothing but single kinship terms; they are never combined. But these terms each designate something less than four categories; they are therefore classificatory and approach what we have called categorical terms. In Chinese and Hawaiian we get single terms which come nearer to being pure categorical terms. The Chinese system is peculiar in that its kinship words do not represent the same category all over the area—at one spot the same word may represent one or more categories, at another place a different category. The Hawaiian has many words that are never used alone, that represent only one or two categories, and hence are pure examples of this type of linguistic arrangement.

It should be noted that though the Chinese is a descriptive system, it does not use the same linguistic technique as Erse, English, and Roman (which itself is a variant); and neither do Arunta and Kariera, which are both classificatory systems, use techniques identical with the Hawaiian.

(e) *Vertical Axis, C and O.* This axis may be theoretically of unlimited length, as in Erse and English, where the use of combined terms makes it possible to describe the vertical position of any relative; or it may be chopped off, as in Chinese, Roman, and Hawaiian, after several generations; or, as in Kariera and Arunta, after the second generation.

Erse and Roman not only place a relative definitely in his generation, but they trace the line of relationship and reveal the number of birth-cycles connecting him to Ego. They thus indicate C^{+d} . Chinese does the same in a different manner and not quite so consistently. Kariera, Arunta, and Hawaiian on the other hand do not reveal the number of birth-cycles in the line of relationship. They reveal only generation, C^{+d} . That is, they show what the result is when the ascending birth-cycles are subtracted

from the descending, but not what the actual number of ascending and descending cycles is. This is due to a failure to express definitely the sibling-link category, as appears shortly.

(i) *Horizontal Axis, L, U, U^w, S^a, and S^r.* This axis has numerous ingredients, of which the sibling-link, L, is one of the most important. When the L category is left indefinite by a terminology, siblings of the same sex are not differentiated. This leads to a narrowing of socio-kinship space. Extended relatives are "brought into" the immediate family by giving them the same terms. So far as this is true the horizontal dimension of biological space is not accurately represented by socio-kinship space, the latter being narrower. The omission of L is tantamount to the omission of C^{+d}; because in addition to not knowing *that* a sibling-link connects the relative, one does not know *where* it comes in the line of relationship even if it is there. One does not know, in other words, how far up the vertical line the sibling-link occurs which connects the central line to the collateral line. At the same time that this omission of L and of C^{+d} narrows the kinship space, it makes practicable the naming of a greater number of relatives. If all collaterals connected by a sibling-link are called by the same terms that members of the central line are called, then any number of collateral lines may easily be given kinship names. This accounts for the fact that classificatory systems are peculiarly suited to those societies where everybody is a kinsman. Omission of L is called "equivalence of brothers" because a system which fails to distinguish between siblings of the same sex emphasizes their social identity.

Sex of Ego is important from the point of view of what two persons are identified as Ego. Our English system is hybrid on this point: for all persons connected below Ego's generation husband and wife are identical as Ego, while for all persons above this generation brother and sister are identical. This is true of Erse and other descriptive systems because it is biologically true. It is also true of many classificatory systems; but occasionally, as in Arunta where the sister calls her brother's children by the same terms that he calls them by and where he calls hers by the same term that she calls them, brother and sister are identified as Ego for connections below their generation as well as above.

Sex of intermediate relatives serves to distinguish subtypes. Systems that fall into the same broad class may differ with regard to this category. Thus the Kariera and Hawaiian systems, both called classificatory, are precisely opposite in their treatment of this category.

(g) *Main Categories Definitely Expressed.* This constitutes in reality a

summary of items 5 and 6. It gives a short, symbolic statement of the nature of the system, and is valuable for that reason.

(h) *Main Categories Indefinite*. This formula is obtained by subtracting the categories definitely expressed—item 7—from the total number of possible categories.

(i) *Distance*. This is taken to mean: Do the terms in the system express definitely the distance, in terms of categories, of the relative from Ego? In so far as a term is descriptive, it expresses definitely the biological distance from Ego; in so far as it is classificatory, it fails to express anything but a minimum distance.

(j) *Relation of Immediate Family to Extended Kin*. Members of the immediate family may or may not be set off from other relatives. If they are, the feat is accomplished by descriptive terms. If they are not, the failure is due to the use of classificatory terms which merge immediate with extended kin. The terminological separation or non-separation of the immediate family is therefore a resultant of the manner in which the categories of relationship are handled. When Rivers classified kinship nomenclatures into "classificatory," "family," and "kindred" systems,¹⁹ he gave a rough differentiation on the basis of resultants; he did not classify on the basis of the internal mechanics on which the resultants rest.

(k) *Linguistic Devices*. In so far as primary terms are used outside the immediate family, it is a safe guess that these terms are classificatory. If there are many of them, it is probable that immediate relatives are merged with extended kin. Finally, if a system is composed almost totally of combined terms made up of primary elements, it is in all likelihood a descriptive system, but if it is composed of combined terms made up of categorical elements, it is likely to be classificatory.

CLARK UNIVERSITY
WORCESTER, MASSACHUSETTS
UNIVERSITY OF CHICAGO
CHICAGO, ILLINOIS

¹⁹ *Kinship and Social Organisation*, pp. 72-81.

WALDEMAR BOGORAS, known best to American anthropologists through his work on the tribes of eastern Siberia, passed away on May 11, 1936. Exiled to Siberia in his youth he devoted his time to the study of his environment, particularly of the tribes among whom he lived. His remarkable powers of observation were such that the Russian Geographical Society charged him with a scientific mission. On recommendation of the Petersburg Academy of Sciences he and his friend Waldemar Jochelson were invited by the American Museum of Natural History to join the Jesup Expedition whose task was the study of the tribes of the North Pacific coast. Still later another exile, Leo Sternberg, joined in this work. Thus a close contact between Bogoras and American anthropologists was established which has continued until the time of his death. His work on the Chukchee, partly published by the Jesup Expedition, partly by the Petersburg Academy of Sciences, is proof of his deep insight into the life of the people among whom he was compelled to live. The clarity of his description is due to his scientific insight; but no less to his artistic gifts. His work as a novelist—he wrote under the name Tan—is also characterized by remarkable powers of observation and psychological analysis. After the close of the field work of the Expedition he stayed for several years in America. After his return to Russia he took up work in the Museum of the Academy of Sciences and, after the Revolution, he became identified with the work among the Northern Tribes of Siberia. In this capacity he organized the educational work intended to give to the tribes greater economic security and to help in their unavoidable assimilation. During the last years of his life, his interest was centered in what he liked to call the grand generalization of anthropology in which he liked to give freer rein to his imagination than he could do in the narrower field of faithful presentation and careful analysis of observed facts. I think it was the artist rather than the scientist who spoke when he dwelled on these problems. He was filled with these ideas when we saw him here last as Delegate of the Academy of Science in Leningrad at the Congress of Americanists held in 1928 in New York. Those who knew him personally could not help admiring his knowledge as well as his enthusiasm; those nearer to him, like the writer of these lines, valued his staunch friendship, and feel keenly the loss they sustained in his death.

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COLUMBIA UNIVERSITY
NEW YORK CITY

REPORT

PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION FOR THE YEAR ENDING DECEMBER, 1936

The American Anthropological Association held its thirty-fifth annual meeting at the Carlton Hotel and United States National Museum, Washington, D.C., on December 27-29, 1936, in conjunction with the American Folk-Lore Society and the Society for American Archaeology.

COUNCIL MEETING, DEC. 27, 4:30 P.M.

President Herbert J. Spinden in the chair. The minutes of the Andover meeting, 1935, were not read, but were approved as printed in the *AMERICAN ANTHROPOLOGIST*, Vol. 38, pp. 301-13, 1936.

REPORT OF SECRETARY

The President appointed the following committees and representatives during the year: Nominating Committee C. Wissler (chairman), C. E. Guthe, J. A. Mason.

Program Committee: J. R. Swanton (chairman), F. H. H. Roberts, Jr., J. M. Cooper.

Committee on Reprinting Out-of-print Numbers of *AMERICAN ANTHROPOLOGIST*. Officers of the AAA, with F. H. H. Roberts, Jr. as chairman.

Representative of AAA on Governing Body of the International Institute of African Languages and Cultures M. J. Herskovits.

Representative of AAA to India Science Congress in Hyderabad. D. Mandelbaum.

Representative of AAA to ACLS annual meeting alternating for A. V. Kidder. T. Michelson.

In January, 1936, the AAA was notified by the AAAS that the place of the latter's 1936 meeting had been changed from Washington, D.C. to Atlantic City, N. J. After informal consultations with various groups of our membership, and in view of the seeming preference of the members against meeting at Atlantic City, the Executive Committee voted to hold our 1936 meeting at Washington, D. C.

The membership of the Association as of December 1, 1936, is as follows:

Life				8
Regular				
AAA members in good standing	573			
" in arrears for 1936	18			
" deceased or in arrears for 1935	39	630		
CSB " in good standing	70			
" in arrears for 1936	13			
" deceased or in arrears for 1935	4	87		
AES " in good standing	216			
" in arrears for 1936	5	221		
ASW " in good standing	35	35		
PAS " in good standing	15	15	988	
Total membership				996

There are 16 exchanges.

The Association has lost by death during the year nine members (AAA) William L.

Abbott, Edward Reynolds, H. W. Frink, Samuel J. Guernsey, T. J. Maxwell, Panchanan Mitra; (AES) George C. Fraser, Albert B. Reagan; (CBS) James H. Breasted.

In closing his six years of secretaryship the Secretary wishes to express to the members of the Association his cordial appreciation of their unfailing courtesy and cooperation in what has been a very congenial task, and in particular to Miss Regina Flannery his deep thanks for her continual help in giving unstintingly of her time and counsel throughout these years.

Respectfully submitted,

JOHN M. COOPER, *Secretary*

It was voted that the Secretary's report be accepted.

REPORT OF TREASURER

The present bank balances of the four funds of the Association stand as follows:

Regular Fund	\$4,068.30
Permanent Fund	3,658.93
Index Fund	1,211.56
Memoirs Fund	1,220.21

This makes a total of \$10,159.00, of which \$9,868.98 is drawing interest in four savings accounts (New Haven Savings Bank, Connecticut Savings Bank of New Haven, Second National Bank of New Haven, and First National Bank and Trust Co., New Haven). The \$4,068.30 of the Regular Fund is divided between a checking account with a balance of \$290.02 and a savings account of \$3,778.28.

The Association is clearly in excellent financial condition. With no outstanding bills the annual budget has been balanced with an increase in the total funds of the Association from \$9,165.76 to \$10,354.74. Although the annual surplus reflects the increase in membership which may be expected to be a continuing source of income, it should be pointed out that last year's income from such sources as unpaid dues and from the Reprint Series must be considered extraordinary. Also the amount of dues received for 1937 is somewhat larger than comparable sums in the past two years. Despite these factors, the financial condition of the Association gives every reason to warrant that the causes for curtailments in our expenditures enforced by the depression have now disappeared.

REGULAR FUND

Gross Receipts

Balance on hand December 1, 1935 \$4,285.05

Membership dues:

American Anthropological Association.

1933-34	\$ 36.00	
1935	195.60	
1936	3,156.74	
1937	356.40	\$3,744.74

American Ethnological Society	1,100.75	
Anthropological Society of Washington	180.00	
Central States Branch	389.50	
Philadelphia Anthropological Society	90.00	\$5,504.99

Sale of publications	375.24	
Reimbursements	378.66	
Reprint Series	140.24	
Interest (Regular Fund only)	104.00	
Transferred from Memoir Fund	118.10	6,621.23
		<hr/>
		\$10,906.28

Gross Disbursements

American Anthropologist.

George Banta Publishing Company:

Printing and illustrating	\$2,921.05	
Distribution	198.91	
Storage, insurance	60.00	
Reprints	283.48	\$3,463.44
		<hr/>
Reprint Series	185.00	
Editor's expenses	1,088.76	
Treasurer's expenses	613.20	
Secretary's expenses	77.50	
Paid for Memoir No. 46	118.10*	
Transferred to Memoir Fund	1,291.98	\$ 6,837.98
		<hr/>
Cash on hand, November 30, 1936		4,068.30
		<hr/>
		\$10,906.28

* The amount of \$118.10 for Memoir No. 46 appears as a disbursement of the Regular Fund only since it was paid by check from that fund, the amount then being reimbursed from the Memoir Fund.

Resources

Cash on hand, November 30, 1936		\$4,068.30
Due from dues.		
1936 American Anthropological Association	\$108.00	
American Ethnological Society	29.25	
Central States Branch	65.00	\$ 202.25
		<hr/>
Due from sales	69.04	
Due from reimbursements (reprints, etc.)	49.67	320.90
		<hr/>
		\$4,389.26

Liabilities

Membership dues for 1937 already paid	\$ 356.40	
Net excess resources over liabilities	4,032.86	\$4,389.26
		<hr/>

PERMANENT FUND

Receipts

Balance (savings account and bonds), Dec. 1, 1935		\$3,672 85*	
Interest on savings	\$87 82		
Interest on two bonds	—†		
Life Membership	94 00‡	181.82	\$3,854.67

Investments

Liberty Bonds (two)	\$ 195.74		
Cash in savings account, November 30, 1936.	3,658 93		\$3,854 67

* Increase of \$2 52 over balance of Permanent Fund shown as of Nov. 30, 1935 represents revaluation of three bonds

† By oversight, interest on two bonds was not deposited in time to show on this year's statement

‡ The amount of \$6 00 had already been credited to 1936 membership dues

INDEX FUND

Receipts

Balance, December 1, 1935	\$1,187.70		
Interest on savings	23.86		\$1,211.56

Investments

Cash in savings account, November 30, 1936			\$1,211.56
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MEMOIRS FUNDS

Receipts

Balance, December 1, 1935	\$ 22 68		
Accumulated surplus from Regular Fund transferred Feb. 28, 1936 to Memoirs Fund	\$1,291.87		
Interest on savings	14 65		
Royalty from book "American Indian Life"	9.00	1,315.63	\$1,338.31

Disbursements

Memoir Number 46 (Underhill).	\$ 118.10*		
Cash in savings account, November 30, 1936	1,220.21		\$1,338.31

* The remainder of the cost of Memoir No. 46, an equal amount, was paid by Dr Ruth Underhill

NET EXPENDITURES AGAINST 1936 BUDGET

	<i>Allowed</i>	<i>Spent</i>	<i>Balance</i>
Secretary's expenses*.	\$ 100.00	\$ 64 60	35.40
Editor's expenses:			
Editor's assistant	960 00	960 00	—
Office expenses	150 00	128 76	21.24
	1,110.00	1,088.76	21.24

Treasurer's expenses:

Treasurer's assistant	480.00	480.00	—
Office expenses	100.00	76.40	23.60
Membership charges	25.00	13.80	11.20
	<hr/> 605.00	<hr/> 570.20	<hr/> 34.80

American Anthropologist:

Printing and illustrating	2,900.00	2,753.43	146.57
Reprints . .	200.00	103.34	96.66
Distribution (includes distribution of Memoir No. 46)	250.00	198.91	51.09
Storage, insurance	60.00	60.00	—
	<hr/> 3,410.00	<hr/> 3,115.68	<hr/> 294.32

American Council of Learned Societies

Anthropological Reprint Series

25.00	25.00	—
150.00	185.00	-35.00

Totals	<hr/> \$5,400.00	<hr/> \$5,049.24	<hr/> \$ 350.76
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* Budgeted to include the President's expenses, if any

REGULAR RECURRENT INCOME AND EXPENDITURES

Net Income

	1933	1934	1935	1936
Memb. dues collected directly at \$6 (less subscription commissions) (AAA)	\$3,263.16	\$3,273.73	\$3,550.64	\$3,744.74
Memb. dues from Central States Branch and affiliated societies	1,166.50	1,415.25	1,509.00	1,760.25
	<hr/> \$4,429.66	<hr/> \$4,833.98	<hr/> \$5,059.64	<hr/> \$5,504.99
Sale of Anthropologist and Memoirs	239.56	339.24	366.54	375.24
Sale of Reprint Series	—	—	43.86	140.24
Interest (Regular Fund only)	164.27	144.81	122.05	104.00
	<hr/> \$4,833.49	<hr/> \$5,163.03	<hr/> \$5,592.09	<hr/> \$6,124.47

Net Expenditures

American Anthropologist, printing and illustrations:

No. 4 of preceding year . .		\$ 874.26	\$ 745.87	
Nos. 1-3 of year	\$2,522.40	1,836.75		
Nos. 1-4 of year . .			2,613.89	2,753.43
	<hr/> \$2,522.40	<hr/> \$2,711.01	<hr/> \$3,359.76	<hr/> \$2,753.43

Anthropologist and Memoirs (distrib., storage, insur., net cost gratis reprints):

1933	199.66			
1934	291.20	58.88		
1935		559.01		
1936		7.08	362.25	
1937		7.08		

Totals . . .	\$ 475.38	\$ 490.86	\$ 632.05	\$ 362.25
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Memoirs: printing and illus. paid by Association (Regular Fund).

67.02	232.32
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Total account publications	\$3,064.80	\$3,201.87	\$4,224.13	\$3,115.68
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Sec'y, Treas., and Ed.'s offices	1,102.85	1,222.77	1,598.89	1,723.56
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American Council of Learned Societies		25.00	25.00	25.00
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Anthropological Reprint Series			122.55	185.00
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Reprinting and purchase out-of-print publ's	165.82			
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Totals .	\$4,316.47	\$4,449.64	\$5,970.57	\$5,049.24
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Surplus carried over or deficit	517.02	713.39	-378.48	1,075.23
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Annual surplus or deficit . .	-573.90	982.56	440.43	1,068.15
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Respectfully submitted,

CORNELIUS OSGOOD, *Treasurer*

It was voted that the Treasurer's report be accepted, subject to the findings of the Auditing Committee.

The President appointed the following Auditing Committee: Edward Sapir (chairman), G. P. Murdock, G. C. Vaillant. The President further appointed the Executive Committee of the Association to serve as the Budget Committee and to report at the annual meeting, December 28, 1936.

REPORT OF AUDITING COMMITTEE

The undersigned, appointed as Auditing Committee, report that they have examined the Treasurer's accounts as submitted for the fiscal year 1936, and find them correct.

EDWARD SAPIR, GEORGE P. MURDOCK, GEORGE C. VAILLANT

REPORT OF EDITOR

The publication record of the year is the *AMERICAN ANTHROPOLOGIST* in a volume of 692 pages and Memoir No. 46—"The Autobiography of a Papago Woman" by Ruth Underhill. Under our plan for cooperative publishing half the cost of the Memoir was borne by the author, to whom go the thanks of the Association.

The Reprint Series seems to have filled a need to judge from the sale of 755 reprints since its inception two years ago. The net return has been nearly two-thirds of the outlay for reprinting, with several hundred copies still available.

As in earlier years, I would draw your attention to the need for papers of wider scope in the *AMERICAN ANTHROPOLOGIST*. It would be desirable if we could have, for example, a series

of brief surveys assessing various fields of interest with suggestions of problems remaining unsolved. Or if we could have the anthropological philosophies of a number of representative men, thus finally putting into print much of that unwritten body of principles and tenets we share. It is my notion that such professions of faith would be of value.

We need to emphasize again that the Association has some funds for publishing Memoirs. The fact that only a few manuscripts were offered during the year suggests that it is not generally known that the Association has this item in its budget. The Association's plan for co-operative financing is quite flexible, but until we are more affluent we should reasonably expect the authors or their sponsors to meet some part of the cost. To give a rough estimate, if the amount were shared equally, a contribution of \$1.25 per manuscript page would take care of most papers. We urge the submission of your papers.

There are at least two other points on which the Editor would like guidance. How far shall we go in printing the reports of the several units of our Association? It is the opinion of some that the reports are largely dead matter, of others that they are worthwhile as records of actions taken and allow members who cannot be present at annual meetings to learn what is going on. While we are probably obligated to publish the reports of the two original constituent societies in Washington and New York, shall we publish the reports of the Central States Branch, for example?

The question has also arisen how far we should go in printing material on the culture of natives who participate in civilized life. I have reference here to the so-called acculturation studies. It is maintained on the one hand that studies of such hybrid cultures are best left to sociological or other journals concerned with aspects of modern life, on the other, that they belong in the *AMERICAN ANTHROPOLOGIST*. Since your wishes should be followed, I would like an expression of your opinion of what we should include.

Respectfully submitted,

LESLIE SPIER, *Editor*

It was voted that the Editor's report be accepted.

It was moved and seconded. It is the sense of the American Anthropological Association that papers in the field of acculturation lie within the interests of anthropology, and that, at the Editor's discretion, they be not discriminated against in the *AMERICAN ANTHROPOLOGIST*. It was voted that the motion be tabled without prejudice.

Dr Spinden, speaking for the Committee on Honorary Memberships, reported verbally that the Committee is not in favor of honorary memberships.

Dr Kroeber reported that the Committee on Abbreviations was unable to come to an agreement and that it asked to be discharged. It was voted that the report be accepted and that the Committee be discharged.

The Committee on Reprinting Out-of-print Numbers of the *AMERICAN ANTHROPOLOGIST* reported in substance as follows. In order to recover the estimated cost (\$12,375) of reprinting by photolithographic process about fifty-five numbers, out of print or nearly so, of the *AMERICAN ANTHROPOLOGIST*, it would be necessary to sell about sixty-five complete sets. There seems to be no reasonable probability of such extensive sale. It was voted that the report be accepted and that the Council at the same time record its opinion that the out-of print issues of the *AMERICAN ANTHROPOLOGIST* should not be reprinted.

It was voted by the Council that the forty-nine new applicants be accepted for membership in the Association.

It was voted that the report of the Nominating Committee (cf. *infra* minutes of Annual Meeting) be approved for recommendation by the Council to the Association.

The President appointed the following Committee on Resolutions to report at the annual meeting, December 28, 1936: A. L. Kroeber (chairman), E. C. Parsons, A. M. Tozzer

The Council was requested to express its views on how specifically the relationship between the Division of Anthropology and Psychology of the National Research Council and working anthropologists could be made more fruitful. After discussion it was voted that the representatives of the American Anthropological Association to the National Research Council be empowered to act in the matter.

The Chairman of the Division of Anthropology and Psychology of the National Research Council, by letter of Dec. 7, 1936, recommends that the Association be asked to take over the responsibility for the publication and distribution of the International Directory of Anthropologists. After discussion it was voted that the matter be referred to the Executive Committee of the Association with power.

It was voted that the following members of the Association constitute the new Executive Committee of the Pacific Coast Section of the American Anthropological Association: A. L. Kroeber, R. L. Beals, L. S. Cressman, E. Gunther, F. W. Hodge, and that the Committee be empowered to add to its membership as other institutions develop work in anthropology.

It was voted that the matter of the permanent storage of the Association's records and archives, now in the Museum of American Archaeology, Phillips Academy, Andover, be referred to the Executive Committee with power.

It was voted that the procedure followed by the Program Committee this year (cf. *American Anthropologist*, Vol. 38, p. 311, 1936), including omission from printed program of titles unless accompanied by paper in full, be continued for the coming year.

It was voted that the President appoint a Committee on American Linguistics with power to act coordinately with the American Council of Learned Societies and the Linguistic Society of America.

ANNUAL MEETING, DECEMBER 28, 2:00 P. M.

President Herbert J. Spinden in the chair. The Nominating Committee (Wissler, chairman, Guthe, Mason) presented its report. After presentation thereof the following officers, Council members, and representatives to councils and associations were elected:

President, Nels C. Nelson

First Vice-President, Matthew W. Stirling (1937)

Second Vice-President, Edward Sapir (1937-38)

Third Vice-President, Diamond Jenness (1937-39)

Fourth Vice-President, John M. Cooper (1937-40)

Secretary, Frank M. Setzler

Treasurer, Cornelius Osgood

Editor, Leslie Spier

Associate Editors, M. J. Herskovits, Cornelius Osgood, F. H. H. Roberts, Jr., F. G. Speck

Executive Committee, Herbert J. Spinden, A. M. Tozzer, J. M. Cooper

Council

Members for term to 1940 inclusive: S. A. Barrett, R. L. Beals, M. V. Beckwith, R. F. Benedict, C. W. Bishop, D. D. Brand, M. Butler, C. S. Coon, C. A. DuBois, M. M. Edell, W. N. Fenton, G. R. Fox, E. F. Greenman, G. B. Grinnell, E. Gunther, B. Haile, A. I. Hallowsell, D. G. Haring, E. Hauray, J. Henry, M. J. Herskovits, G. Herzog, E. B. Howard, W. W. Howells, A. E. Jenks, N. M. Judd, R. Kennedy, E. Kopta, T. de Laguna, R. Linton, D. Mandelbaum, J. C. McGregor, P. A. Means, H. P. Mera, N. Morss, J. L. Nusbaum, B. Oct-

teking, L. M. O'Neale, L. Outhwaite, W. Z. Park, A. C. Parker, H. Pollock, R. H. Post, J. H. Provinse, A. R. Radcliffe-Brown, F. G. Rainey, G. Reichard, H. H. Roberts, M. J. Rogers, L. Satterthwaite, Jr., C. C. Seltzer, L. Sharp, J. H. Steward, M. W. Stirling, R. J. Terry, M. Titiev, R. Underhill, B. L. Whorf, A. Woodward.

Members for term to 1939 inclusive: H. M. Allyn, M. Andrade, W. C. Bennett, Glenn A. Black, L. Bloomfield, P. H. Buck, R. Bunzel, D. Byers, W. B. Cline, C. H. Danforth, D. S. Davidson, H. Field, R. Fortune, L. Foster, E. W. Gifford, E. A. Golomshtok, W. D. Hambly, M. R. Harrington, L. Havemeyer, J. N. B. Hewitt, H. Hoijer, E. B. Howard, M. Jacobs, D. Jenness, F. Johnson, C. R. Keyes, C. Kluckhohn, T. F. McIlwraith, R. McKennan, P. S. Martin, J. A. Mason, M. Mead, W. K. Moorehead, G. P. Murdock, N. C. Nelson, P. H. Nesbitt, E. C. Parsons, O. Ricketson, H. B. Roberts, E. Sapir, A. H. Schultz, D. Scott, F. M. Setzler, W. D. Strong, G. C. Vaillant, C. F. Voegelin, H. Webster, W. S. Webb, L. A. White, C. C. Willoughby, W. J. Wintemberg, G. Woodbury.

Members for term to 1938 inclusive: C. Amsden, H. Beyer, P. A. Brannon, K. M. Chapman, F. E. Clements, H. S. Colton, J. M. Cooper, T. Deuel, K. P. Emory, G. Engerrand, R. Flannery, C. D. Forde, A. H. Gayton, J. Gillin, M. R. Gilmore, H. S. Gladwin, C. D. Gower, J. B. Griffin, C. E. Guthe, H. U. Hall, E. S. Handy, A. T. Hansen, J. P. Harrington, F. Hawley, C. L. Hay, W. W. Hill, L. W. Jenkins, I. T. Kelley, A. V. Kidder, O. LaFarge, A. Lesser, J. Lips, E. M. Loeb, T. Michelson, E. H. Morris, M. E. Opler, C. Osgood, H. Powdermaker, V. F. Ray, W. A. Ritchie, F. H. H. Roberts, Jr., H. C. Shetrone, J. B. Stetson, Jr., S. Tax, T. W. Todd, H. Turney-High, W. L. Warner, G. Weltrish, G. D. Williams, F. R. Wulsin.

Members for term to 1937: R. B. Bean, H. B. Collins, Jr., B. Cummings, F. Densmore, A. C. L. Donohugh, G. G. Heye, E. A. Hooton, H. Kelley, H. W. Krieger, W. M. Krogman, S. K. Lothrop, R. Mackaye, W. C. McKern, R. L. Olson, J. E. Pearce, R. Redfield, H. L. Shapiro, F. G. Speck, L. Spier, E. Spinden, W. D. Wallis, H. N. Wardle.

Past Presidents (*ipso facto* members of the Council): F. Boas, F.-C. Cole, F. W. Hodge, A. Hrdlička, A. L. Kroeber, R. H. Lowie, G. G. MacCurdy, H. J. Spinden, J. R. Swanton, A. M. Tozzer, C. Wissler.

Representative to Social Science Research Council: R. Linton.

Representatives to National Research Council: F. G. Speck, J. R. Swanton, G. C. Vaillant.

Representatives to Section H, A.A.A.S.: T. Michelson, H. L. Shapiro.

Representative to American Council of Learned Societies: A. V. Kidder.

The Budget Committee presented the following budget recommendations for 1937:

1. Secretary's expenses		\$ 200.00
2. Editor's expenses		
Editor's assistant	\$ 960.00	
Office expenses	150.00	1,110.00
3. Treasurer's expenses:		
Treasurer's assistant	480.00	
Office expenses	100.00	
Membership charges	25.00	605.00

4 American Anthropologist:		
Printing and illustrating	\$3,300.00	
Reprints. . . .	200.00	
Distribution . .	250.00	
Storage, insurance. . .	60 00	3,810.00
		<hr/>
5 Anthropological Reprint Series		50 00
6 American Council of Learned Societies		25 00
		<hr/>
		\$5,800.00

The Budget Committee further recommended that the sum of \$800.00 be budgeted from the Memoir Fund for the payment, in part or in full, for the publication of Memoirs during the year 1937.

It was voted that the budget as recommended be approved.

It was voted that the following resolutions presented by the Committee on Resolutions (Kroeber, chairman, Parsons, Tozzer) be adopted:

1. *Be it resolved*, that the deep loss to American anthropological science through the deaths of James H. Breasted, C. B. Cosgrove, Samuel J. Guernsey, Panchanan Mitra, Clarence B. Moore, Albert B. Reagan, and Edward Reynolds be recorded in the minutes of this annual meeting and that copies of this resolution be sent to the close relatives of the persons here memorialized.

2. *Be it resolved*, that the American Anthropological Association tender to the officials of the United States National Museum its cordial thanks and appreciation for their courtesy in granting the use of the Auditorium of the Museum.

The President appointed the following Committee on American Linguistics: E. Sapir, chairman, M. Andrade, L. Bloomfield, F. Boas, G. Herzog, M. Jacobs, A. L. Kroeber, T. Michelson, G. Reichard, J. R. Swanton

PROGRAM

SUNDAY, DECEMBER 27TH

9:30 A.M.

A. IRVING HALLOWELL, Sin, Sex and Sickness in Saulteaux Belief
 WILLIAM N. FINTON, Tonawanda Longhouse Ceremonies. Ninety Years After Lewis Henry Morgan
 LUSIE CHILDS PARSONS, Human Sacrifice among Pueblos and Aztecs
 BERNICE M. KING, Form in American Indian Music, as Exemplified in the Songs of Jemez Pueblo, New Mexico (Records)
 MISCHA TITIEV, The Use of Kinship Terms in Hopi Ritual

2:00 P.M.

MELVILLE JACOBS, Ancient Language Boundary Movements in the Northwest United States
 VERN E. RAY, The Lower Chinook in Northwest Culture

MARIAN W. SMITH, Distinctions between a Religious Life and Psychic Abnormality among the Coast Salish.

JULIAN H. STEWARD, Shoshonean Bands and Social Groups

FRID EGGAN, Respect and Joking Relationships among the Cheyenne and Arapaho

DOUGLAS G. HARING, Cultural Phenomena: a Redefinition

8:00 P.M.

Distribution Surveys

A. L. KROEBER, Culture Element Distribution Surveys

JOHN M. COOPER, Ethnological Stock-taking

MONDAY, DECEMBER 28TH

9:00 A.M.

MARTHA BECKWITH, New Light on the Meaning of the Hawaiian Kumulipo, the So-called Song of Creation

MAY EDEL, Bachiga Law

EDWARD WARD, Factors Making for Polygyny among the Yoruba

HEINZ WIESCHHOFF, Remarks on Kingship among the Babudja in Southern Rhodesia

ALFRED E. HUDSON, Remarks on Kazak Social Organization

GEORGE DYVREUX, Functioning Units in Ha(rh)ndea(ng) Society

RAYMOND KENNEDY, Marriage Classes and Totemism in Eastern Indonesia

2:00 P.M.

Auditorium, United States National Museum

MELVILLE J. HERSKOVITS, The Significance of the Study of Acculturation for Anthropology
TRUMAN MICHLSON, Report on Two Expeditions to James and Hudson's Bays Region (Lantern)

SOL TAX, The Concept of Culture and Civilization Considered in the Light of Guatemalan Societies

C. F. VOEGELIN, Paradigmatic Difference as an Index of Dialectic Difference

B. L. WHORF, A Comparative Decipherment of Forty-one Ancient Maya Written Words

WILTON MARION KROGMAN, Racial Types Represented at Tepe Hissar, Iran, from the Mid-Fourth to the Mid-Second Millennium B.C. (By Title)

M. F. ASHLEY-MONTAGU, The Infecundity and Infertility of the Unmarried in Primitive Societies

6:15 P.M.

Annual Dinner of the AAA, AFLS and SAA

HERBERT J. SPINDEN, World Chronology and the Peopling of America: Address of the Retiring President

HENRY B. COLLINS, JR., Culture Migrations and Contacts in the Bering Sea Region

GEORGE C. VAILLANT, Archaeology of the Valley of Mexico (Lantern)

TUESDAY, DECEMBER 29TH

9:00 A.M.

GOPHERY J. OLSEN, Certain Archaeological Investigations in the Lake Champlain Valley, including the Orwell (Vermont) Burial Site (By Title)

- FREDERICK JOHNSON, Problems Surrounding the Classification of Certain Culture Complexes in New England
DOUGLAS S. BYERS, The Excavation of the Nevin Shellheap, Bluehill Falls, Maine (Lantern)
WILLIAM A. RITCHIE, The Hopewellian Phase in New York (Lantern)
FLORENCE M. HAWLFY, The Present Status of Tree Ring Dating in the Middle West
EARL H. BELL, The Archaeological Problems of the Lower Niobrara Valley, Northeast Nebraska (Lantern)
E. B. REINAUD, Chronology of the Indian Petroglyphs of the Western Plains (By Title)
JOHN LAMBERT CUTTER, Some Aspects of the Folsom and Yuma Problem (Lantern)
JOHN P. GILLIN, Preliminary Report on Archaeological Investigations in Nine Mile Canyon (Lantern)

2:00 P.M.

- MARY BUTLER, The Monongahela Woodland Culture
ISABEL GARRARD PATTERSON, Archaeological Survey of the Chattahoochee Valley in Georgia (Lantern)
LINTON SATTERTHWAITE, JR., Peten Influence on Usumacinta Maya Architecture (Lantern)
FRANK M. CRESSON, JR., Maya and Mexican Sweat Houses (Lantern)
WILLIAM DUNCAN STRONG, Historic and Prehistoric Horizons in Honduras (Lantern)
H. NEWELL WARDLE, A Forgotten Exploration in Chira Valley, Peru (Lantern)
ERNESTINE W. SINGER, A Fishnet and some Netted Bags from the Chira Valley, Peru
ROBERT McCORMICK ADAMS, Significance of the Archaeology of Southeastern Europe

BOOK REVIEWS

NORTH AMERICA

Lowry Ruin in Southwestern Colorado. PAUL S. MARTIN. (Anthropological Series, Field Museum of Natural History, Vol. 23, No. 1, 1936. 216 pp., 112 pls., 54 figs., 4 maps.)

The Lowry Ruin report is divided into three sections, the report on the ruin by Martin, a discussion of masonry by Lawrence Roys, and a study of skeletal material by Gerhardt von Bonin. The last section has already been reviewed in the *AMERICAN ANTHROPOLOGIST*.

Lowry Ruin is a large site of late Pueblo II and early Pueblo III in southwestern Colorado, which appears to have been occupied by only fifty or sixty people at a time, but which shows frequent rebuilding and reoccupation. Dates on the excavated portion run from 1086 to 1106 A.D. The pueblo was chosen for excavation because of its location in a little known district showing both Mesa Verde and Chaco culture traits, because the refuse heaps were large enough to allow stratigraphic studies, and because it was suspected that this area might be the place of development of pueblo architecture and kivas.

Maps of the successive additions to the nucleus of Lowry Ruin are tied to Roys' detailed descriptions of wall types. All of the walls found were made with rubble cores, faced on both sides with surface stones, but emphasis is put upon dissection of the walls and their consequent division into two main types, the Chaco-like and the non-Chaco. The former is defined as characterized by thick mud cushions separating the stones and spalls, the consistent use of flake-like stop spalls to retain the mud mortar, and the consequent tendency toward coursing. The second type is described as based on the block-type technique of dry masonry, in which the load is transmitted downward through stone to stone contact of blocks and true-bearing spalls, with clay mortar merely filling the spaces left by the irregular surfaces of the stones. An intermediate type merges characteristics of each. The Chaco-like masonry is divided into three numbered types.

Both Roys and Martin voiced shocked comments that archaeologists of the Southwest have never heretofore provided the reader with descriptions of dissected walls but have, instead, classified them more or less inadequately on the basis of their surface finish. I believe there is no question of the value of the type of classification they suggest and which has proven its worth at Lowry Ruin, but, on the other hand, if wall surfaces are found to vary consistently in a district and these variations are found to come within certain dates of construction, that type of wall classification proves its value in chronological implications for related undated ruins. While the proposed classification by dissection might certainly provide new data for relationship studies, the names used by Roys leave something to be desired. The negative term of "non-Chaco" is scarcely appropriate as a name for the most prevalent type of wall throughout the known prehistoric Southwestern Pueblo area. It is likewise disconcerting to find the three types of Chaco-like masonry

numbered rather than named, at a period when those of us who have previously used numbers for wall and pottery types are regretfully discovering that what we called number I should have been number IV or VI or X, and that there is nothing to do but to go back and name the whole group, old and new. The ever-present bugaboo of classification brings the wall nomenclature to the status of pottery nomenclature: the large divisions by dissection might be compared to wares, while the sub-type by surface or other characteristics would be comparable to types in pottery.

In setting up the two main divisions of Southwestern walls, Roys is interested in their ancestry as well as in their distribution. The Chaco-like walls are thought to be derived from Gladwin's Red Mesa sequence dating between 850⁺ and 950 A.D. in which the earliest walls show a minimum of stone and a maximum of mortar. The non-Chaco Roys suggests to have been derived from the Basketmaker cists and pit houses with slabs laid up to line the sides of the pits. Because the slabs were laid against clay, no clay was placed between them. In the 1936 Chaco excavations of the University of New Mexico, however, the pit house walls appeared as ancestors of the walls characterized by much clay and few stones, for an intermediate type was found in which the pit house slabs were used as base for Pueblo I surface walls, the upper part being a typical representative of the type of small stones set in much clay. Roy's other suggestion of the mortarless watch towers and wind breaks of the Four Corners region as ancestors for his dry masonry appears more reasonable, except for the paucity of these remains. His suggestion is that the loose stones might very well have been taken by later builders for their own use, but this explanation sounds a little weak. Other solutions to the problem of individual ancestry or of the two divisions breaking from the main stem may later present themselves.

In view of the recent interest in kiva murals in the Southwest, Martin's photographs of the Lowry kiva murals make a welcome addition to distribution studies of the art. His suggestion that the black paint was probably that used on pottery sounds somewhat improbable. The black paint on Lowry pottery gives the same tests as that used on the modern Pueblo pottery. This modern paint, and hence probably that of the ancients, does not turn black until after it has been fired. No evidence of burning the decorated walls is given; the hypothesis that the black paint was some mixture of soot and grease seems more tenable until a test is made.

Detailed descriptions and measurements of artifacts and pottery are supplemented by 54 figures of photographic illustrations and 102 pages of clear, well produced, and artistic illustrations of walls, ruins, pottery, and skeletal remains.

A list of the types of pottery believed to be local and another of the types believed to have been imported prefaces a statement that percentages given in pottery descriptions are based on the total amount of each collected, but technicians would be interested in knowing whether that total was comprised of all the sherds collected from all the rooms excavated, of all the whole pieces, or of the sherds from one or from all of the refuse mound trenches. In passing, also, a question regarding

the name of Wingate Black-on-white, attributed to Mr Gladwin, might be raised. Gladwin states orally that he has never given a name to the black-on-white type characteristic of his Wingate Phase, a type described in 1934 for the Chaco as Semi-polished Black-on-white and since listed as Gallup Black-on-white, with Mr Gladwin's approval. The photographs of pottery and potsherds are up to the usual excellence of Martin's illustrations, but the question mark which follows the classification of many vessels may well puzzle the reader, and the five plates showing sherds with no suggestion of their classification but only a note on their provenience are wasted, for the reader is at loss to classify by surface design alone.

Martin has presented, in the main, a clear description of careful scientific investigation of a ruin in an important, but little known, part of the Southwest. His theory of the northward diffusion of southern complexes from the Little Colorado-Puerco focus, with special influence from the Chaco in walls, pottery, and the great kiva, checks with previous suppositions which lack of evidence have hitherto left as hypotheses. I am inclined to agree with Roberts that recent excavations suggest the focus as possibly toward the Chaco rather than farther south, but spotting a focus is impossible until the area is thoroughly known. Martin's attempt at inference of the psychology and culture pattern characteristics of the region of Lowry Ruin is based upon clever interpretation of material culture and is one of the original contributions of this study. Archaeologists could well do more of this—if they did it with requisite caution. The report is well indexed and a brief table of location, by state and county, of the major ruins mentioned in the text is an item which might well be copied in consideration of readers unfamiliar with the Southwest.

FLORENCE M. HAWLEY

UNIVERSITY OF NEW MEXICO

Pictographs and Petroglyphs of the High Western Plains. E. B. RENAUD (Archaeological Survey of the High Western Plains, Eighth Report. 47 pp., 46 pls. \$75. Denver: University of Denver, 1936.)

The greater part of this monograph is devoted to descriptions of pictographs and petroglyphs in Wyoming, South Dakota, Colorado, and New Mexico recorded during seven years of archaeological survey of the high western plains. Sites were visited by the author or his field assistants or were known through sketches and photographs from various sources.

In synthesizing his material, Dr Renaud postulates five major periods which are established by patination (meaning discoloration of the inside of pecked or incised lines), technique of line work, style, and superimposition. Although the last three are unimpeachable criteria if cautiously used, I cannot agree that "patination is a very significant criterion of age" unless the high plains area be shown to possess unique properties of climate or rock which eliminate insuperable difficulties in using this criterion elsewhere. Shallow carving appears old if it has not cut through the discolored outer shell of the rock; often a single design appears to have very different degrees of patination when it is unevenly exposed to weathering. The impres-

sion is conveyed that patination is usually the primary age criterion and that the other criteria are only auxiliary to it.

It is not surprising that the petroglyphs of this area should fall into several periods. But, though there is no reason to suppose that the periods named by the author are not sound, the reader may feel entitled to more than the author's word as evidence of them. Differences in linework, superimposition of classes of figures and patination can best and often only be detected on the original stone. Good photographs, however, would greatly help those who cannot visit the originals. Unfortunately, this monograph is illustrated only with pen and ink sketches in which even the style of the original figures necessarily succumbed to the style of the draftsman who reproduced them.

Dr Renaud has, on the whole, shown restraint in the extraordinarily tempting and dangerous business of interpreting petroglyphs. I am mystified however that he can know that a spiral represents a whirlwind, a circle with radiating lines a sun, etc.

On page 5, Dr Renaud utters a plea for systematic, comparative, distributional studies of petroglyphs. With this I emphatically agree and regret that Dr Renaud has, for a purpose which is by no means obvious, devoted considerable space to a comparison of his material with that of Palaeolithic Europe and ignored entirely its relationship to the half-dozen states west of Colorado which are now fairly well known through the publication of many papers and several comprehensive monographs. That the relationship with neighboring areas is vastly greater than with Europe is evident from the illustrations. It is to be hoped that Dr Renaud will some day illuminate his own area by relating it to these neighboring areas.

But, after all, it is difficult to deal with petroglyphs and retain complete invulnerability. Dr Renaud deserves commendation for adopting this unwelcome step-child of archaeology.

JULIAN H. STEWARD

BUREAU OF AMERICAN ETHNOLOGY

Northwest Sahaptin Texts. MELVILLE JACOBS. (Columbia University Contributions to Anthropology, Vol. 19, Part 1. xi, 291 pp. \$4.00 New York. Columbia University Press, 1934.)

Further evidence of the long-recognized similarities in Shoshonean and Sahaptin tales is furnished by this valuable group of text translations recorded by Dr Jacobs. They comprise myths from the Klikitat (36), Cowlitz (5), Upper Cowlitz (Taidnapam) (9), and Kittitas (2). Two groups of tales which could not be clearly differentiated as either Cowlitz or Upper Cowlitz are combined (21).

Coyote is transformer, place-namer, teacher, hero, and charlatan. Moon, too, is a transformer, Wild Cat, Cougar, and Blue Jay adventurers. Dangerous beings abound: almost any person or object may suffer a personality change and become "dangerous" during the course of a story. Soft Basket Woman, Swallowing Monster, and Grizzly Bear Woman are invariably villains. Familiar devices are: hot rock

missile, spider rope, stepping-over revival, log substitution, wounded adviser, faces advisers, five as ritual number.

Tales or incidents familiar from the Plateau, Basin, or northern California are Controversy over length of day; Lewd story-teller, Skunk loses his musk; Coyote visits afterworld, Wild Cat and Coyote trick each other, Skunk and Coyote dupe animals; Coyote and swimming girls; Coyote as doctor; Coyote takes son's wives, floats as baby, releases salmon; Coyote loses eyes, recovers them by clothes-shifting; Arrow-chain theft of fire, Theft of hoop; Bear and deer.

Possibly expectable but absent are: Deluge or flood; Theft of sun, summer, Death controversy; Dog husband; Star husband; Any constellation myths, Twin heroes. Two tales with Mink as a character are seemingly of Coast Salish source, as, no doubt, are others.

The native literary style has been delightfully maintained. The tales are long, pointed in plot, and pleasantly ramified with significant detail. As a check on narrative style Dr Jacobs obtained several ethnologic texts which, while appended, he advises are not offered as ethnographic material. Enlightening comments are given on the cultural and linguistic affiliations of each informant.

A. H. GAYTON

NEW HAVEN, CONNECTICUT

Archaeological Survey of the Guano Valley Region in Southeastern Oregon. L. S. CRESSMAN (University of Oregon Monographs, Studies in Anthropology, No. 1. 48 pp., map \$ 50. Eugene: University of Oregon, 1936.)

Work of the Cressman party in the Guano Valley region had as its object not only the definition of cultures and of their sequences in this district, but also the larger problems of the relationship of the culture of the Guano camp sites to the caves farther to the east and the identification of the northern periphery of the Southwestern culture area. The initial work on this area, one of the major depressions in southern Oregon, formed by late Pliocene or early Pleistocene movements of the fault blocks of the northern continuation of the Basin-Range faults, was hampered by complete lack of adequate maps, which the party consequently constructed for themselves. The shallow valley, surrounded by highlands of barren irregular blocks of basalt, supports only the flora of the arid Transitional and Upper Sonoran, but the fauna of animals, birds, and fish attracted an aboriginal population whose culture is reminiscent of that of Lovelock Cave, 100 miles to the south.

On the beach of dry Guano Lake, camp sites are indicated by mullers, metates, points, knives, and ash beds. Around Desert Lake, scrapers, knives, drills, axes, and petroglyphs are common. The petroglyphs, to be reported in detail in a later monograph, showed predominance of Great Basin type curvilinear designs, but two photographs of interesting modifications of the Pueblo rain cloud design are illustrated. The superposition and weathering of the petroglyphs may offer data on their relative chronology.

The only cave found in Guano Valley was small, 18 by 12 feet, and the floor

covered with a 10 inch layer of packed manure. It was trenched from front to back with a cut three feet wide, the rise of the side walls made a cross trench impractical. Beneath the manure was fine dry soil, which was shoveled into a hand sieve, carried outside, and sifted. Twined basketry, with twisted two rod warp, and bone and stone artifacts were found centered around the fireplace and just inside the cave opening. It is stated that there was no stratification in this cave, and in view of the detailed descriptions of their careful work and in the interest in technique, we miss a note on what test for stratification was used—arbitrary levels, observation, or artifact typology.

The monograph concludes with the formulation of problems in culture sequences in the region and of the possible interpretations. The closest tie-up appears to be that of the Guano Lake cave with the pre-Paiute middle stratum of Catlow Cave No. 1, where excavations suggest a long occupation, with three cultures extending, possibly, from late Pleistocene or early Recent to the present. Paiute basketry was found on the cave floor above another type which checks with the fragments from the Guano Valley cave. This basketry has been identified by Weltfish as more similar to that of the modern Klamath-Modoc than to any other, a fact which fits with the Paiute legend that they drove the Klamath from the valley and took it over as their own.

Dr Cressman and his associates have put out a paper which makes the fashionable attitude of scathing criticism impossible. Caution and lack of dogmatism are everywhere evident. Working with surface material which offers but few leads to definite culture relationships, and lacking all skeletal material, he suggests usages of artifacts and judiciously lists various geological and cultural theories of his own and of other men, with the evidence and probabilities for each. The data of artifact types by region is organized into tables. Excellent drawings showing top and side views of artifacts and techniques of basket weaving, and photographs of artifact groups all carry a measuring line conveniently marked for comparison. Typographical errors are lacking, except for the bibliographic citation of Kidder and Guernsey's famous bulletin (correctly *Archaeological Explorations in Northeastern Arizona*). The two appendices give summaries of the Gifford-Schenck classification of points, used by Dr Cressman, and of the technique of mapping used in constructing the map folded at the back of the volume.

In a period when poor and inadequate photographs and lack of caution and of outside consultants on the technical aspects of questions touching sciences outside one's own have damned the publication of work which otherwise carried some merit, the authors of this monograph can be congratulated on their carefully selected illustrations and on their perspicacity in quoting the opinions of the numerous experts who have examined their artifacts, basketry, and fossilized wood, and have commented on the geology, tree rings, and climatology which outline the Guano Valley problems. Future aid from various studies and from identification of fossil pollen is planned. The criticism of geographers that anthropologists usually neglect studies slightly apart from their field of main interest was avoided by half

the party and half the monograph being devoted to geographers and geography. If this type of well rounded work can come from a region where problems are difficult and cultural material scanty, workers in richer fields will have to look to their laurels if they would compete.

FLORENCE M. HAWLEY

UNIVERSITY OF NEW MEXICO

The Relation of North American Prehistory to Post-Glacial Climatic Fluctuations

REGINALD G. FISHER (Monograph, University of New Mexico and School of American Research, No. 3, x, 91 pp., 28 figs., 3 maps, Albuquerque, University of New Mexico Press, 1935.)

The author states the purposes of his monograph as follows.

The first purpose of this work shall be to summarize the behavior of climate in order to interpret it as a force conditioning events of prehistory. The second purpose shall be to investigate the relation of the climatic factor to North American prehistory. The third purpose shall be to determine the relation of prehistoric culture in America to culture in Eurasia through a combined analysis of climatic fluctuations and world culture history.

Since there is yet so little convincing evidence of human population in North America antedating the Wurm-Wisconsin glaciation, the dimension of this work will be limited to the Post-Glacial epoch (p. 6).

In summarizing the material bearing on climatic changes since the last glaciation, the material is idealized or schematicized, and the author rightly says that it must be recognized that climate does not behave in any such simple manner as the formal presentation would suggest—a statement with which we heartily agree. It should be recognized that if one attempts to "interpret it [climate] as a force conditioning events of prehistory" that it is real, not theoretical climatic conditions which must be described.

The suggestion of a migration route into the New World by way of Bear Island-Spitzbergen, Novaya Zemlya-Spitzbergen, and Faeroes-Iceland courses (p. 56) is offered, although it is not clear whether the author is thinking of it with reference to the total aboriginal population or only the Eskimo, presumably the latter. He asserts that climatic conditions show this is possible, but neglects to offer any evidence from archaeology in support of his theory.

He divides the culture of North America into two culture-horizons, the Osteic and the Ceramic. Eight traits are chosen as the basis of classification, the presence or absence of which characterize the two horizons. The more significant elements which differentiate the two horizons are the "ceramic process" and "the full agricultural process." "As a matter of fact, culture in North America at the time of Columbus, for the most part, was in the Ceramic horizon" (p. 91). We submit that any such attempt to classify the cultures of North America according to this plan is useless for any interpretation or explanation. It fails to recognize the great variations which are the really striking features of the cultures. Furthermore, so little is known about the prehistoric cultures outside of the Southwest that any effort to

build up a classification based on the scattered archaeological evidence is quite useless.

Attention should be called to the author's theories concerning the origin of the New World Ceramic Process and the Agricultural Process. He believes or at least states that

an independent origin of the American Ceramic complex seems out of reason (p. 79). . . . Although, as Merrill has pointed out, not a single one of the great Eurasian agricultural products ever reached America in Pre-Columbian times, the basic fact of agriculture must have been common knowledge of the Ceramic populations which filtered into North America during the non-glacial period (p. 81)

Space does not permit either theoretical or factual criticisms of this position.

The legibility of some of the maps could have been improved. The figures showing the mean monthly precipitation are poorly reproduced, so that special effort is required to decipher the legends in many instances. Poor editorial work shows in the variation of spelling of proper names: for example, on page 85 Novaya and Navaya occur within five lines of each other; Faeroes (map A-38) and Foeraes (p. 56) appear; while Birket-Smith is spelt both Birket (*passim*) and Birkete (p. 85).

The long bibliography conspicuously lacks any reference to Nordenskiöld's article, *The American Indian as an Inventor*¹ and an important paper by Boas, *America and the Old World*,² both having vital bearing on the author's subject.

L. S. CRESSMAN

UNIVERSITY OF OREGON

Taos Pueblo. ELSIE CLEWS PARSONS. (General Series in Anthropology, No. 2. 121 pp., 14 pls., 6 figs., map. \$1.75. Menasha, Wis.: George Banta Publishing Co., 1936.)

The village of Taos, due to its marginal position in the Pueblo area and to its relationship with the Plains, has long been of great interest to anthropologists, but the close secrecy of its inhabitants has hitherto prevented investigators from securing more than a fragmentary outline of its culture. Over a period of about a decade, starting in 1922, Dr Parsons has from time to time gathered information which she now presents in *Taos Pueblo*. Her monograph, while far from complete, makes a welcome contribution to our knowledge of this distinctive pueblo.

Dr Parsons divides her data into six main sections. Somewhat unexpectedly, the part called "Material Culture" is devoted primarily to accounts of such pursuits as agriculture, hunting, and war rather than to technology and art. There is a fairly detailed treatment of "The Personal Life," but the section on "Ceremonialism" is often sketchy, owing to the reticence of informants. The rest of the book comprises a short but very satisfactory account of "Secular Government and Catholic Church;"

¹ Journal, Royal Anthropological Institute, Vol. 59, 1929, pp. 273-309.

² Comptes-rendu, XXI^e Congrès International des Américanistes, Göteborg, 1925, pp. 21-28.

a brief historical sketch called "Place," and a concluding chapter, "Attitudes and Trends," in which the author comments on the psychology of the Taos Indians and speculates on their future.

As is to be expected, Taos departs in several respects from customary Pueblo forms. We find that it lacks the clan principle and has neither a masked Kachina cult nor a curing society—traits generally regarded as more or less typical of the Pueblos—and, in addition, we find that it had less than the customary dependence on agriculture, that it had no weaving, and that it suffered an early degeneration in basketry and pottery manufacture. On the positive side, Taos is unique in its partial acceptance of Peyote and in the possession of a political organization strong enough to enforce its edicts by the imposition of heavy fines and by such non-Pueblo punishments as jailing and the public whipping of offenders. Despite all variants, the culture of Taos does conform to Pueblo patterns in most respects, but there are so many traits that seem to be of Spanish-Mexican provenience or that may well have been borrowed from the Plains, that Taos presents more than the usual number of ethnological problems.

In view of the difficulties under which Dr Parsons was forced to work, and in the light of her frank admission of uncertainty regarding the accuracy of her data (p. 6), it would ill become a reviewer to complain about gaps in her material. There are, however, a few faults in Dr Parsons' presentation which may properly be pointed out. Surely, there is no good reason why a mere list of officers should be made as hard to read as the following sentence from page 71:

As in other eastern towns, there are annual officers—the Governor (t'aabuna), his lieutenant and three others, a group of five; two war captains called humlauwa t'unena with ten assistants called the chiefs, lauwena (these men are of varying age, young, middle-aged and old, "even as old as the present Governor," a man, say, of sixty-five, all twelve war captains or *capitans de guerra* may be called humlauwena); and a tiskal or pikale t'unena (Sp. *fiscal*) with four assistants or lieutenants (*tiniente*), another group of five, church officers who are considered part of the Governor's staff.

Far more important than the matter of style is Dr Parsons' handling of her kinship material. She notes (p. 38) that the Taos Indians have no conception "of any matronymic or patronymic exogamous group," and that they tend to regard the word "clan" as referring to ceremonial groups or societies. It seems as though we have here a problem of kinship and social organization that well merits close investigation and which might have been approached through the medium of a thorough compilation of kinship terms and usages. Instead of making out a conventional schematic kinship chart, however, Dr Parsons chooses to present her data in a fashion that makes analysis extremely difficult. A list of kinship terms is followed by five genealogies, each of which contains from 22 to 46 named and numbered individuals. She then devotes nearly three solid pages to the application of kinship terms by the various people listed in the genealogies. Thus the reader is faced with the dreary prospect of turning back and forth interminably through eight pages of raw data in the attempt to puzzle out the Taos system.

Under such circumstances it is not surprising that Dr Parsons should observe (p. 38) that "Obviously, the kinship terminology is used, as always in Pueblo circles, loosely." But in the absence of an exhaustive and systematic analysis of their kinship patterns, it seems unwarranted to ascribe "looseness" to the Pueblo Indians.

On the whole, whatever its deficiencies may be, it must be granted that Dr Parsons' monograph has made available a good deal of important information. It is to be hoped that a time will soon come when Taos will be less hostile to investigators and will permit further study of some of the interesting ethnological problems which it presents.

MISCHA TITIEV

UNIVERSITY OF MICHIGAN

Zuñi Mythology. RUTH BENEDICT. (Columbia University Contributions to Anthropology, Vol. 21. 2 vols.; I: xliii, 342 pp.; II: vii, 345 pp. \$8.00. New York: Columbia University Press, 1935.)

Dr Benedict has not only issued a large collection of new tales from Zuñi, but edited them with such thoroughness that these two volumes constitute a definitive treatise on Zuñi mythology and its relation to the tribal culture. She has brought together for comparison with her texts all the tales of earlier collectors and has carried out comparisons with clarity and consistency.

In her introductory remarks she contrasts the extensive folklore studies where the interest is in the wide distribution of parallels with her own intensive work on a single tribe.

For the most profitable study of single bodies of mythology, folk tales should hold an important place in the tribal life, not being relegated, for example, to children's amusement or used solely as word-perfect recitations of magic formulae; a large body of tales should have been recorded, and over as long a period as possible; the culture of the people who tell the tales should be well known; and folklore among that people should be a living and functioning culture trait.

In the Zuñi she finds that these conditions are fulfilled to an extraordinary degree.

The book, aside from the excellent collection of tales, concerns itself with two problems: (1) the themes which their folklore elaborates and the relation of these to their culture, and (2) the literary problems of the Zuñi narrator. This detailed study constitutes a very great contribution to the understanding of folklore. It is clearly conceived, and copiously illustrated with incidents from the tales. This part of the study must be read by every folklorist who hopes to understand the material he is handling.

Although the author has denied herself frequent comparison of the themes with those of other tribes and cultures, it is a matter of regret that more of the most obvious of these have not been pointed out. The outside connection is not only of general interest, it frequently enters into the question of the literary problem of the narrator. It would, for example, be interesting to know something of the provenience of the tale of "Who is the Strongest?" (Vol. II, p. 225). The author,

of course, knew that this is a version of the world-wide tale "Stronger and Strongest," but the unwary reader might well take it for a product of native invention.

It is perhaps too much to hope that many other American Indian tribes, not to speak of other native peoples, may be studied with the thoroughness and understanding which Dr Benedict has here devoted to the Zuñi.

STITH THOMPSON

INDIANA UNIVERSITY

MEXICO, CENTRAL AND SOUTH AMERICA

Excavations at Zacatenco. GEORGE C. VAILLANT. (Anthropological Papers, American Museum of Natural History, Vol. 32, Part 1, 1930.)

Excavations at Ticoman. GEORGE C. VAILLANT. (Same series, Vol. 32, Part 2, 1931.)

Excavations at Gualupita. SUZANNAH B. AND GEORGE C. VAILLANT. (Same series, Vol. 35, Part 1, 1934.)

Excavations at El Arbolillo. GEORGE C. VAILLANT. (Same series, Vol. 35, Part 2, 1935.)

Early Cultures in the Valley of Mexico: Results of the Stratigraphical Project of the American Museum of Natural History in the Valley of Mexico, 1928-1933. GEORGE C. VAILLANT. (Same series, Vol. 35, Part 3, 1935.)

Dr and Mrs Vaillant's work in the Valley of Mexico is one of the most striking examples yet presented of archaeology as an exact science. Documentation of the objects encountered in the excavations could not have been carried further. This extreme carefulness of detail has been carried over into the published results so that every specimen can be placed in its provenience in the ground. The plans, sections, pottery silhouettes, and other illustrations furnish a thesaurus of the "Early" cultures of the Valley. In addition, there are tabulated statistics and distribution and "trait" tables. These all mean an indefatigable zeal, countless hours of unromantic cleansing, classifying, and counting of sherds.

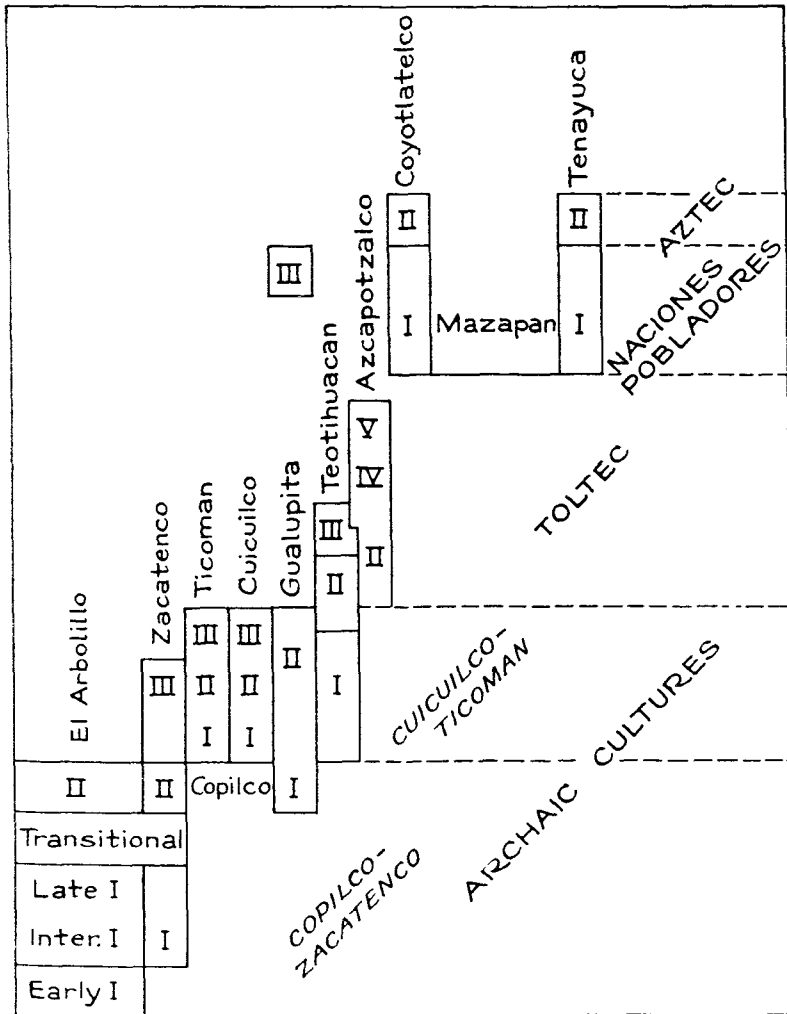
Beginning in 1928, the work continued through 1932. During the first season there was a reconnaissance of the hill sites north of the Valley. Work began in earnest at Zacatenco in 1928-29, followed by a season each at Ticoman and El Arbolillo. The final year of the campaign was spent at Gualupita, a site in the neighboring State of Morelos.

One who has not followed from year to year this brilliant piece of work can only with difficulty comprehend the diversity of the specimens from these sites. Apart from the complexity and variety of the figurines, there are more than twenty different varieties of pottery; utensils and implements of clay, stone, and obsidian; stone, clay, horn, shell, and bone ornaments; ear plugs, spindle whorls, whistles, and even a part of a stone sculpture (1931, Pl. 79), and one bit of textile.

The solid foundation and substantial structure of the "Early" cultures in the Valley of Mexico have been built. Future work in this region and on this horizon can only be extensions to the building with, I think, no fundamental changes in the

plan. The sites of Copilco or the Pedregal and the elusive Cuicuilco find a home in this dwelling.

The author calls for "long range stratigraphic horizons" which will include the Southwest, the Maya, and other southern cultures stretching into South America.



Fortunately the trend in American archaeology seems to me to be in this direction. The triangulations coming from Vaillant's works ought to furnish an important base line for this extension

The accompanying diagram¹ shows the equations among the "Archaic" cultures of the Valley and Gualupita in Morelos, together with those of the Toltec-Teotihuacan sites, the post-Toltec or pre-Aztec settlements of the properly called "Naciones Pobladores," and the Aztec sites of Tenochtiltan, Texcoco, and others. Certain pottery styles are tentatively correlated with these migrant tribes (1934, Table 4, p. 122). A conjectural alignment of these various cultures with history and "semi-history" starting with the foundation of Tollan and the death of Quetzalcoatl, and ending with Montezuma II is attempted (1935, Table 23, p. 259). Both of these tables are full of suggestions and are of the greatest help to the student who wishes a picture of the whole sequence of culture, even if the details may vary as the result of future investigations of the later and more diversified cultures.

The author speaks in one place of the "permutations of the different objects made by human hands" and again he writes,

The minute differentiations in technique and style, that are indicative of changes of fashion and, therefore, time, were stressed to the exclusion of broader resemblances such as would be considered in a philosophic or artistic evaluation of the material culture of these people.

I think that Dr Vaillant has been too permutational-minded in his classifications, that he has assumed that "the minute differentiations in technique and style" do mean "a change of fashion, and, therefore, time" when we can more naturally explain them as personal and chance variations. I should like, in principle, to see a coalescence of some of his sub-sub classes (C3a, C3b, and C3c, for example). I cannot believe that these most minute differentiations all have a meaning either as regards style, time, or place. This has a practical side, however, as I know from experience that only Dr Vaillant can apply his classifications to a collection of archaic figurines. I have tried and he had to come to my rescue.

This all too brief review, in consideration of the importance of the works listed above, should not be closed without a word of grateful thanks from all American archaeologists to the Mexican government and its officials, especially to Ignacio Marquina, Eduardo Noguera, and José Reygadas Vértiz for their great aid and cooperation in allowing Dr Vaillant to carry on these investigations. The rigorous rectitude of the Vaillants while carrying on their archaeological work in a foreign country has amply justified the courteous and generous attitude of the Mexican government.

ALFRED M. TOZZER

HARVARD UNIVERSITY

Chan Kom: a Maya Village. ROBERT REDFIELD AND ALFONSO VILLA R. (Carnegie Institution of Washington, Publ. No. 448. viii, 387 pp., 16 pls., 15 figs., 6 tables. Washington, 1934.)

This impressive volume is the first published ethnological study done under the auspices of the Carnegie Institution in their ambitious project for the investigation

¹ In the preparation of this diagram, I have had the benefit of Dr Vaillant's advice. He has suggested minor changes on the Aztec end of the scale, which I have included.

all aspects of life in Yucatan. Never before has it been possible to assemble specially trained persons, one to take the physical measurements, one to draw the maps, doctors to obtain medical data, nurses to investigate midwifery, and linguists to translate and check the texts. It is a form of cooperation of which anthropologists must all have dreamed, and it is encouraging that it has been achieved in this one instance.

The Maya village which was chosen for this investigation is twenty kilometers south of Chichen Itza. It was settled twenty years ago and did not become independent of its mother town and acquire its own communal land until ten years ago. It was founded by émigrés with touching secular enthusiasm, in the words of the village leader, "for a clean and civilized pueblo," and they have steadfastly achieved modern schoolhouses, playgrounds, masonry houses, and good roads, and now deep-paved village streets. It has been no part of their objective to secure for themselves freedom to continue undisturbed the ways of their ancestors. Calendric ritual in charge of village officials is not found, simple *novenarios* in honor of the village patron have been substituted for the extended fiestas held in neighboring towns, the communal rain ceremonies are held not as part of the safe routine of life but in the emergency of drought, and the communal exorcising in the emergency is apotropaic; the rites of the field and of bee-keeping are private affairs not organized by the village. Even the ceremony according to which the annual saints' officer, called the *majordomo*, passes on the decorated pole to his successor as a sign of transferring his ceremonial charge is absent in Chan Kom though it is the core of the annual fiesta in neighboring towns.

On the other hand the wide spread institution of *fagina*, compulsory work on public projects, has been basic in the development of this newly established village. All public services are regarded as *fagina*, though possibly the chief office of *comisario* is an exception. Whether a man is serving as *guardia* (the village administrator), or working at the behest of the local Agrarian Committee (organized under the laws of the revolutionary government), or building a road or a school house, he is discharging the obligation of labor which he owes to his village. *Fagina* is heavy in Chan Kom as contrasted with neighboring villages, but there is social approval of its exactions and no man may leave Chan Kom with unfulfilled *fagina* obligations.

In Redfield in his introduction stresses the fact that *Chan Kom* is presented as a scene on a series of studies showing the range of cultural life from city to hinterland in this region. Against such a background *Chan Kom* seems most interesting as describing an émigré group bent on social betterment under native leadership and the developments that have taken place in such a situation. The *comisario* has taken over a paternalistic role suggestive of that of the old *butab*, the *fagina* has provided workmen for village improvements, beer and spirits have been put under control and are hardly drunk, fireworks, the almost inevitable Middle American accompaniment of a fiesta, were tabued as a useless waste, and the most eminent village leader characterizes the native shaman-priests who conduct the non-Catholic fiestas as "exploiters of the working man and the worst enemies of all the workers

of the world." It is not surprising to learn that the shaman-priests of the old religion have now all departed from Chan Kom. Yet even the village leader quoted above has recourse to these "exploiters" in trouble, and fireworks have crept in again.

Against the larger background of the project as planned, the selection of Chan Kom for intensive observation will undoubtedly show itself justified, but in the present state of our knowledge the account of a village not made up of émigrés, and preserving traditional village forms would cast much light on the special case of Chan Kom. The account of this village is limited not only by the absence of traditional social forms due to the single-minded secular enthusiasm of the village founders, but also by what seems to be a methodological commitment to report only on what took place during the period of observation. Thus a hundred-page diary of the junior author, the native schoolteacher, is printed *in extenso*, though its relevant material could more enlightenly have been summarized in a few pages; and on the other hand there are no data on even such questions of cultural background as older customs of service for a bride or of extended family households—whether or not persons now living in individual households grew up in extended households like the one aberrant group in Chan Kom—or broken marriages transcending the two years of the study. Methodology above all things can be pragmatically judged by its full and accurate results, and it does not seem desirable to place a tabu on knowledge of background easily obtainable, in favor of exclusive presentation of happenings under observation.

There is not space within the limits of this review to discuss the religious rites and beliefs in Chan Kom. Calendric village ritual has gone, but as individual agriculturalists and bee-keepers they carry out rites to the old gods of the fields and the bees. There is a long list of illnesses named from the "evil winds" that have entered the patient, and illness is regarded as due to accident or non-observance of tabus, rather than to malicious activity of a witch or sorcerer. The native distinction between "cold" and "hot" plants is well described. The folkways that have survived in Chan Kom appear to be those which have to do with the individual's conduct of his own life, as against those which depend on the traditional organization of community life.

RUTH BENEDICT

COLUMBIA UNIVERSITY

Archaeology of Santa Marta, Colombia. The Tairona Culture, Part II, Section 1: Objects of Stone, Shell, Bone and Metal J. ALDEN MASON. (Anthropological Series, Field Museum of Natural History, Vol. 20, No. 2, pp. 133-273, Chicago, 1936.)

Archaeology of South America. J. ERIC THOMPSON. (Anthropology Leaflet, Field Museum of Natural History, No. 33. 160 pp., 18 figs., 12 pls., map. \$0.75. Chicago, 1936.)

Two recent papers on South American archaeology represent a specific field account and a general survey. Mason's report is a continuation of his description

of Santa Marta material. His first account described the ruins visited and the excavation practiced on the 1922-23 field trip. Part II, Section 1 is a description of the objects of stone, shell, bone, and metal, including not only the material encountered in the field, but also objects of the same locality in many museum collections. The pottery and perhaps the skeletal material will be described in Section 2, yet to be published. Comparisons, distributions, and conclusions are reserved for a Part III. As a consequence of this program the present paper is of greatest importance to archaeologists working in northern South America who can utilize the careful description and graphic presentation of Santa Marta artifacts for comparative purposes. A worker in Venezuela, for example, will note many similarities between his specimens and those illustrated from Santa Marta. Students and scholars who desire familiarity with type material of Santa Marta will also find the report invaluable. Without doubt Mason is contributing one of the most detailed accounts of a South American archaeological region. We look forward to the description of the pottery, and above all to the important comparisons and conclusions which should take Colombia out of the class of archaeologically unknown countries.

Thompson's paper is intended as a non-technical review of South American archaeology and a guide to the Field Museum's collections. His review of the South American continent is competently handled and major problems are outlined, although, quite logically, not presented in great detail. The resumé of Peruvian coast archaeology is particularly well treated, incorporating the major accomplishments in that field since the time of Joyce's general work. Many suggestions are inserted for the specialist, and a good perspective is presented for the general student. The Andean highland archaeology is more sparingly reviewed, probably because the latest reports were not available to the author at the time of writing. Brief accounts of Peruvian religion, social organization, and arts are included.

The mass of literature on the northwest Argentine area, and the great variation in the authors' opinions make chronological conclusions difficult. Thompson follows Eric Boman's conclusions which virtually state that all northwest Argentine archaeological divisions are roughly contemporaneous. It is my feeling that more thorough analysis will show that the Diaguite culture is pre-Incaic in spite of a carry over into Inca times, and that the Atacameña culture is still older. Thompson does suggest that the Atacama civilization of north Chile and northwest Argentine may be derived from a pre-Diaguite culture (which would thereby account for some of the obvious Tiahuanaco influences).

The sections on Ecuador and Colombia are good in spite of the fact that here, as elsewhere in the accounts, the combination of archaeological, ethnological, and historical materials is confusing, although perhaps necessary for the museum-visitor reader.

A valuable lead is presented in Thompson's resumé of the arguments for a South American center of distribution to Central America in reference to certain traits. This argument is initiated in the introductory chapter and reiterated in the sections on Ecuador and Colombia. Thus in all probability Central America received from South America manioc, the seedless pineapple, the tomato, metal-working, wax-

casting, wax-painting of pottery, the hammock, a religious ball game, the blow-gun, urn burial, datura, and the custom of chewing lime with tobacco; and possibly it received cups with a flaring base, pot-stands, mammiform tetrapod bowls, horizontal figure vases with the hollow stomach forming the bowl, and a bowl with a relief fish detail as a rim design. Since some of these traits have heretofore been considered as Central American distributions, the importance of Thompson's suggestions is manifest

WENDELL C. BENNETT

AMERICAN MUSEUM OF NATURAL HISTORY

AFRICA

Stone Age Africa L. S. B. LEAKEY. (xii, 218 pp., 13 pls., 28 figs., map. \$2.75. London [New York]: Oxford University Press, 1936.)

Stone Age Africa is a pocket-sized handbook containing a complete outline of the prehistoric archaeology of each major section of Africa, preceded by chapters on Pleistocene climate and fauna, and followed by others on rupestrine art, the fossil men responsible for early African cultures, and the relationship of African prehistory to that of Europe. Dr Leakey discusses the pluvial periods of the African Pleistocene without attempting to correlate them to the glaciers, although it is clear from the cultural connections that the second pluvial and third glacial periods must have been at least roughly contemporaneous. He considers the African fauna south of the Sahara to be particularly archaic, and states that no new animals other than domestic varieties have evolved anywhere since the beginning of the Middle Pleistocene. This would make *Homo Sapiens* a Middle Pleistocene species along with the others, so that he would have been contemporaneous with Neanderthal if not with even earlier hominids.

East Africa was apparently the scene of active cultural evolution from the earliest Pre-Chellean pebble culture down to the Mesolithic, but lost its initiative before the development of a full Neolithic, which it derived tardily from the north. South Africa, similarly, proceeded from a pebble culture to evolved late Paleolithic techniques, but retained some of the latter until the beginning of European settlement. The forested part of Africa has revealed nothing earlier than the Neolithic, which is hardly surprising since the Paleolithic manner of living seems to have favored open country. North Africa differs from the South and East in lacking a Pre-Chellean, but Dr Leakey presumes that such evidence will eventually be discovered. In both East Africa and Europe, the Levalloisian flake industry overlaps the later stages of the hand-axe culture, but in North Africa the two are separated by a definite time gap.

The Upper Paleolithic in Africa is peculiarly complicated and suggests the presence of several contemporary ethnic groups, each with a wholly or partially separate industry. Men of the Upper Paleolithic, all *sapiens*, were nevertheless sharply divided into races, long-headed and narrow-faced ancestral Hamites in East Africa;

full-sized and big-brained Bushman progenitors in the South, a negroid at Asselar, east north of Timbuctoo, tall, rugged-boned Crô-Magnon-like individuals in Algeria; and partially negroid Mediterraneans in Palestine and Egypt.

Dr Leakey derives the Upper Paleolithic industries from contacts between pre-existing complexes, through the agency of parallel evolution. Thus Aurignacian develops in several places from Levalloisian influenced by Acheulean, while the pressure-flaking technique, be it associated with Stillbay in East and South Africa, Aterian in North Africa, or Solutrean in Europe, represents evolved Levalloisian in contact with Aurignacian. Microlithic cultures similarly have several separate origins. Although this scheme would lead a number of parallel technological lines into Neolithic stone industries, Dr Leakey balks at ascribing a multiple origin to the Neolithic economy.

A superficial analysis of this book reveals two fundamental assumptions which underlie most of its reasoning. These are: (1) *Homo sapiens*, being fully evolved in the Middle Pleistocene, was responsible for the hand-axe cultures of Africa and Europe, while the Levalloisian sequence was the property of Neanderthal man, who perhaps shared it with Rhodesian. (2) Parallel evolution played as great a role in Pleistocene technical development as did diffusion.

Neither of these two assumptions is capable of immediate and indubitable proof. Pre-Aurignacian *sapiens* man in Europe is still, as Dr Leakey admits "on the shelf," as are his Kanem and Kanjera men from East Africa.¹ In Europe and Palestine Neanderthal men are definitely associated with Levalloisian, in the period formerly called Mousterian, but no Neanderthal man has yet appeared in Africa. The association of Rhodesian man with this industry is purely speculative, since his accompanying fauna and implements were destroyed in a lead furnace. One further objection to this division might be cited—if Levalloisian survived into the Late Pleistocene in Africa, and eventually merged itself into the Neolithic, it must have shifted to a *sapiens* master somewhere along the road.

The assumption of parallel evolution seems much more firmly grounded, since it is, after all, an ethnological consideration and few ethnologists would today deny the participation of parallelism and independent invention in human cultures. As Dr Leakey clearly states, parallel growth and diffusion must both be invoked, and the two must be used in proper proportions.

Dr Leakey makes it perfectly clear that many of his theories are to be considered tentative. Every statement of an unusual nature is guarded by some qualifying word or phrase. The book is so full of ideas that if half of them shall be proved correct, its author will still have made a great advance in the study of African, and incidentally of European, prehistory. For those who refuse to be stimulated, *Stone Age Africa* contains an abundance of well arranged factual material.

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¹ Date of writing, December 5, 1936

Schöpfung und Urzeit des Menschen im Mythos der afrikanischen Völker. HERMANN BAUMANN. (ix, 435 pp., 22 charts. Berlin: Dietrich Reimer, 1936.)

Dr Baumann's valuable work rests on the preliminary analysis of 2,500 traditions. It presents a vast amount of material, carefully sifted, with numerous summaries, tables, and distribution charts. Notwithstanding these aids the book makes tough reading. For one thing, the author assumes as obvious certain mythological identifications which most of us should like to see demonstrated, and his exposition bristles with such terms as "manistic" (based on an intense belief in the ancestral line extending to the earliest period) and "chthonic" (involving belief in a personified earth being). Again while preserving his independence of Graebner, Schmidt, and Frobenius, Dr Baumann persistently refers to a scheme of his own, which cannot be readily judged excepted by Africanists. Each of his strata is characterized in terms of the postulated mythological types. Thus, the Old Sudanese-Nilotic group is credited with a lunar, manistic, and chthonic mythology. The author insists on the importance of a horticultural patriarchal culture, the "Old Nigritic," less ancient only than that of the Bushmen and Pygmies (pp. 4, 386). It is markedly manistic, with celestial beings unimportant or absent, but the first ancestor is sometimes considered a high-god, as when Unkulunkulu among the Zulu fuses with the notion of a sky deity (p. 24 *sq.*).

Baumann contrasts true creation by a celestial high-god, who moulds human beings out of clay, with the manistic technique of merely summoning them to appear out of caves, rocks, trees, swamps, etc. (pp. 164-74). In addition to these two methods he recognizes procreation by a male Sky and a female Earth deity; the activities of culture-heroes (*Heilbringer*), who may shade into the ancestral or Trickster type; and direct descent of men from a high-god (pp. 174-80).

Baumann argues for the antiquity of the high-god and the *Heilbringer* concepts in Africa—convincingly, it seems, both from the range and the character of their distribution (pp. 178, 182). Defining myths as concerned with cosmic views, he gives little emphasis to the Trickster, though of course he recognizes the impossibility of an absolute cleavage between folk-tales and origin tales. From this point of view he is impressed with the colorlessness of African fancy, which he contrasts with that of Oceanians and Americans. African mythology centers in anthropogony, not cosmogony (pp. 1, 185, 306). Where the author finds conceptions on a grander scale, as among the Yoruba, he scents Mediterranean and Indian influences.

Here lies one of the most fascinating features of the book, but also one on which the ethnologist must await the verdict of Egyptologists and Sanskritists. To the reviewer the parallels cited seem of unequal value, but several are doubtless noteworthy. Thus, in the Vishnupurana the earth goddess Prithivi is pursued by her son Prithu, who ravishes her or tears open her womb, while the Yoruba deity Jemaja—also identified as terrestrial by Baumann—is pursued and ravished by her son, her bursting body spewing forth the gods (pp. 134, 180). Again the Kanioka (southern Congo) story of origin of death parallels the Toda equivalent. God causes men to die for good because instead of serious mourning they laugh soon after a

trial-death (p. 285). The association of fire-making with creation (pp. 51, 177), of the vulva with a wound (p. 395 f.), and the cult of fig-trees (pp. 233, 236) are also traced to India, which is said to have specially affected Kenya and Rhodesia.

Baumann further accepts Talbot's and Meek's argument for Egyptian and other Mediterranean connections (pp. 157, 172). He traces the Bachama (northern Nigeria) hero Nzeanzo, who fights the God of the Dead, to the Isis and Horus myth (pp. 154, 180); calls attention to the Yoruba world-egg, in which the Sky and Earth deities are squeezed together (p. 193); and identifies the ram-headed Jukun (Nigeria) god Amma with Khnumu (pp. 158, 172). The Grussi (Volta region) conceive the sky as having originated from a cowskin thrown upward, so that the horns turn into sun and moon, the tail into the milky way (p. 147). Though the Eurasiatic and American earth-diver motif is absent, the notion of a primeval sea is noted, e.g., among the Yoruba, and is likewise derived from Egypt (p. 190). These influences appear strongest in the Nilotic region and on the west coast, but extend at least to the Kasai (p. 395).

Quite apart from such stimulating suggestions which must await the judgment of specialists, this work provides the first comprehensive survey of the field. We might have wished for fuller treatment of motifs irrespective of cosmic associations. For the comparative folk-lorist the material cited sometimes lacks definiteness. Thus, the Hausa story labeled "magic flight" (p. 96) evidently conforms to the classic type, but it is not clear whether this applies also to the instances, all too briefly alluded to, from the Zulu, Ronga, and Kimbundu. However, the folk-lorist, too, may profit from this rich collection. It is well to know that the dearth of African deluge myths has been greatly exaggerated (p. 307 sq.), that the earth-diver motif is really absent (p. 190), that the existence of separate men's and women's villages is widespread (p. 354). Finally, we may mention the Theft of Fire stories (p. 359 sq.), only one of several distinct categories of tales explaining the origin of this cultural feature.

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Negerkünstler. Ethnographische Studien über den Schnitzkünstler bei den Stämmen der Atulu und Guro im Innern der Elfenbeinküste. HANS HIMMELHEBER. (80 pp., 33 illus., map. Stuttgart: Strecker und Schröder, 1935).

Although the author of this little book, a layman, disclaims all attempt at making a stylistic analysis of negro art or a psychological analysis of African artists, he nevertheless furnishes more clues to both these questions than any book on African art I have yet seen. He simply presents the material he secured from his informants with the answers to questions relating primarily to the human beings who make and use the objects. Such questions concern the purpose of the objects, attitudes toward them held by their creators and their purchasers, incentives to finished craftsmanship, modes of apprenticeship, motor habits and procedure, materials used and means of securing them, standards and criticism of creators and of the general public, prestige value, taste and analysis of taste by artists.

The data here presented, like so much naïve material, furnish certain surprises more astonishing to the theorist than to the field-worker who becomes accustomed to them. For instance, it is interesting to note that none of Himmelheber's informants acknowledges a love for carving itself, but learns the craft because of the income and prestige it brings, both of which are highly valued. And this in spite of the fact that art appreciation and good taste are general in the tribes.

Perhaps the cruelty of the masters and the strenuous life the apprentice leads account largely for this attitude. Style is analyzed implicitly, and, of course, not completely, in the artists' criticisms of standards and in the remarks made by the Atutu and Guro laymen in choosing objects as first-rate or less than that. These remarks show that religious and other factors have set limitations on the style which one ignorant of its details could not detect.

The author, for example, generally agreed with the natives as to the relative merits of particular groups of objects, but in one case disagreed about two carved web-beams of looms. Upon analysis it turned out that the native chose his favorite because the forehead of the small head was nicely rounded whereas the other was flat and receding. The choice was correct from the Guro point of view since it is important for them to indicate with sure strokes the prominences of the frontal bones.

These are only suggestions which the reader will find more satisfactorily described in the book. If his interest in primitive art is like mine, he will wish that someone would settle down with these people and give us a thorough-going study of these and other questions suggested by them.

One should note that the author is conversant with the literature of the region, and that he used it to approach his own problems but did not allow it to color his own finds, since in his reading, as well as in his work with the natives, he seems to be well provided with discrimination.

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Im Lande des Gada: Wanderungen zwischen Volkstrummern Sudabessiniens. E. JENSEN AND H. RHOTERT. (Veröffentlichung des Forschungsinstituts für Kulturmorphologie. 608 pp., 40 pls., 174 figs., 3 maps. R.M. 14. Stuttgart: Strecker und Schroder, 1936.)

This substantial report relates in detail the itinerary and observations recorded during the Deutschen Inner-Afrikanischen Forschungs-Expedition of 1934-35 under the leadership of Professor Leo Frobenius.

The volume is divided into two equal parts, the former dealing with a chronological description of the journey written in diary form. Preceding this is a brief historical introduction dealing with the legendary period, the Axum kingdom, Amharic influences of the Middle Ages, and Mohammedan penetration by conversion and conquest. The historical review is continued into modern times, including the reigns of Theodor, Menilek, and Haile Selassie.

Throughout nine chapters the sections of the itinerary are dealt with rather after the fashion of the early explorers. One has to read carefully to make ethnological meanings, for the information is interwoven with much descriptive matter, which is of general interest rather than specific scientific value. Yet the course of the journey is made clear by a map (p. 310) indicating the route from Addis Abeba, southwestward to the Kenia frontier. This cartography, combined with division into chapters each of which deals with a specified part of the itinerary, enables the reader to make his localizations of customs, though no assistance is given in the comparative study of these. Each item of information seems to be isolated, we fail to see the pattern as a whole. This disjointed effect cannot be avoided when the diary method is used.

Nevertheless, the narrative contains references to town planning, funerary monuments, law, agriculture, pastoral pursuits, and totemism. The sacred serpent is not forgotten, and the able brush of artist Bayolé enlivens the pages with sketches of physical types, musical instruments, and domestic utensils. Like the subject matter the illustrations are miscellaneous; they are arranged by the hand of the artist rather than the ethnographer. The photography is excellent. Plate VI is particularly interesting from the culture contact point of view. The Egyptians were fond of humorous sketches of animal life, as for example, a cat herding geese. In this Abyssinian painting the precise Egyptian touch of humor, and of technique as well, is evident in scenes showing the wedding of the cat and the mouse, and the brotherhood of the animals. In the latter picture the carnivorous animals are seated at a meal with the herbivorous animals.

The latter half of the volume approaches ethnology more seriously under seven separate captions, including pastoral pursuits and agriculture, death and burial, megalithic monuments, and folklore. Comparative study of customs and their distribution is aided by several maps of Africa showing localities of occurrence. The book gives a glossary of Amharic and other words, a bibliography, and an index which is far too brief for the size of the book.

The work is a valuable record of factual material, also a testimony to the industry and skill of all concerned. But perhaps the collaborators would agree that an enormous amount of work remains to be done in further classification of facts, and in showing their present day function, as well as their specific relationship to the data given in the historical introduction.

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The Bantu Tribes of South Africa: Volume IV, Section I, Plates i-xl: The Va Thonga.

A. M. DUGGAN-CRONIN. Descriptive notes on the Plates by Henri-Philippe Junod. (28 pp., 40 pls. 25 s. Cambridge: Deighton, Bell and Co., 1935.)

This latest section of the series of volumes of illustrations of the racial types of South African natives contains forty photographic studies of the Va Thonga and their country. The plates are grouped under two headings: the Va Thonga of the

Transvaal and those of Portuguese East Africa. The volume might be an atlas or album to Junod's *Life of a South African Tribe*.

Mr Duggan-Cronin's fine photographs embrace, so far as they form a pictorial commentary on Thonga life, a fair variety of subjects. There are two views of landscape, which, with the background of some of the other pictures, give a good idea of the nature of the country. The majority of the other pictures are portraits: two are of named individuals, five of other notables, three of persons connected with magic or the healing art. Others illustrate the domestic and industrial activities of women, or men's occupations such as war, fishing, the manufacture of head rings, etc.

The subjects are well selected to show the considerable variety of racial types; the differences, for example, between "commoners" and "aristocrats" being rather remarkable. The contrast between the beautiful beaded costumes of the Transvaal women and the dingy attire of the people living in Portuguese East Africa marks almost as strong a divergence in local habits which is attributed to the influence of Transvaal Nguni, from whom came the conquerors of the Va Thonga. Mr Junod's notes to the pictures provide what might almost be called a summary view of the culture and not only of its material aspects, while the Introduction by the general editor of the series offers a very concise historical and ethnographical sketch of the people.

Since, as the editor says in the concluding paragraph of his introduction, "no one can underestimate the terrific impact of our civilization on Thonga life," the importance of preserving in such fine pictures as these, an outward presentiment at least of a distinctive way of life should, itself, not be underestimated. With the intensive recruiting among the Thonga of laborers for the Witwatersrand mines, it cannot be very much longer before "the natural set of ideas and customs of the Thonga in his kraal" has been altogether diverted into and lost in the current of change. The editor has, however, still hope that, presumably under like influence of "those in South Africa who know the value of the past," the "new forms of life may be grafted on the old tree, without hampering the source of genuine Thonga contributions to the future of South Africa."

There is a bibliography, admittedly incomplete, particularly as to the Portuguese sources. For a list, also said to be incomplete, of vernacular publications, reference is made to Bantu Studies, Vol. 7, No. 1.

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ASIA AND EUROPE

Transactions of the Asiatic Society of Japan. Second Series, Vols. 9-13. (London: Kegan Paul, Trench, Trubner and Co., 1932-1936)

The present review selects a number of items from the five volumes listed, omitting those papers of minor or incidental pertinence to anthropology. The editing of the volumes is well done. With few exceptions, annotations, bibliographic references,

and words otherwise ambiguous, are given in both Latinized and ideographic form. The Society's Editor is Mr Hugh Byas.

Early Japanese Law and Administration, by G. B. Sansom (Part 1 in Vol. 9; Part 2 in Vol. 11). Notes on Japanese law in the early historical period; valuable for information regarding social life. Sansom modestly comments that these notes pretend neither to completeness nor authority; that they were gathered while preparing his *Japan, a Short Cultural History*, and were intended for personal use. But the study of Japanese cultural history and of comparative law is the richer for his courtesy in making the material available for publication.

Shoku Nihongi, Chronicles of Japan from 697-791 A.D., translated and annotated by J. S. Snellen (Vol. 11). There has long been a demand for a translation of the *Shoku Nihongi*, which continues the Imperial Court records from the end of the *Nihongi*, the great classic translated by Aston. Mr Snellen presents a carefully annotated translation of the first three kwan (books) together with an Introduction. The *Shoku Nihongi* is an invaluable source of information regarding ancient Japanese social organization, law, social classes, kinship, and cultural forms generally. The translator promises to publish the remainder of the forty kwan as soon as possible.

The Ainu Bear Festival, by John Batchelor (Vol. 11). A summary of Ainu beliefs regarding the bear, with a description of the bear festival as witnessed over fifty years ago. Rapid disintegration of Ainu cultural life enhances the value of the descriptive material. Less discursive than Batchelor's other works, the paper does not digress to present theories of origin or hypothetical interpretations.

An Inquiry Concerning the Origin, Development, and Present Situation of the Eta in Relation to the History of Social Classes in Japan, by Shigeaki Ninomiya (Vol. 10). In Japan as elsewhere, popular belief in alleged racial differences is far more efficient in maintaining social discrimination than actual genetic differences. Mr Ninomiya thinks the eta, or outcasts, were originally derived from the etori, menials who gathered food for hawks and hunting dogs in the seventh century; adoption of Buddhism outlawed their occupation and forced them into butchering, leather-working, care of tombs, killing of animals and handling of bodies—occupations beyond the pale of Buddhist respectability. Joined through the centuries by slaves, condemned persons, disinherited warriors, and other outcasts, they dwelt in separate villages and came to be regarded as a source of defilement. The monograph includes a valuable summary of the history of Japanese social classes, showing the transformation of the class hierarchy of each era into that of the next. Bibliography and footnotes are valuable.

Alien Land Tenure in Japan, by Robert Karl Reischauer (Vol. 13, entire). A scholarly contribution to the study of Japanese law, international law, and property ownership. Anthropologists may be interested in the summary of the meagre data of Japanese concepts of the alien and his rights in pre-Perry days.

Nibukuwakami Jinja, by R. A. B. Ponsonby Fane (Vol. 10). Reconstruction of the ceremonies, and summary of the data accumulated by the Shintō priest Moriguchi Narakichi, relative to the probable location of an important early shrine of the

Japanese rain-deity. Comment by Dr Kato includes tables of Imperial offerings, at various dates, of horses—black for rain, white (rarely, chestnut) when sunshine was desired.

Inari: Its Origin, Development, and Nature, by D. C. Buchanan (Vol. 12, entire). This is the first specific study in English of the cult of Inari, ubiquitous in Japan: the multiform beliefs and confused theology which provide an underlying unity in the cult are described in rich detail. The fountainhead of Inari is Myōbu Shrine at Fushimi in Kyōto, Dr Buchanan has obtained most of his material there.

The real Inari is a female agricultural deity whose messenger is the fox, hence the popular confusion of Inari with fox-worship. Inari deities are legion, and the worshipper is often uncertain as to the identity of the deity of a particular shrine. Prior to invention of the term Shintō to distinguish indigenous Japanese religious practices from Buddhism, Inari flourished as a cult of food and fertility. Inari practices have always occupied an important place in the amorphous religion of Shintō, but Buchanan's contention that Inari was originally central and only later supplanted by the cult of the Sun Goddess Amaterasu may be slow of acceptance until further evidence is provided.

In modern Japan Inari is all things to all people: fox-spirit to the serving maid, god of food and plenty to the peasant, deity of wealth to the business man, phallic deity to lovers and prostitutes, patron of metal workers and industrial enterprises, fortune teller, and healer of the baby's croup. The red shrines are everywhere: in remote mountain nooks, in busy department stores, in shipyards, automobile factories, and—if not yet, before long—in aeroplanes. Inari is a pantheon rather than a god. Buchanan lists nine deities worshipped at Fushimi, food spirit, earth spirit, goddess of sexual attraction, god of wealth, god of harvest, and four deities of growing rice. Ten other subsidiary gods are also worshipped on the sacred mountain of Fushimi. A two-million yen "Inari cathedral" was built in 1930 at Toyokawa; in Tōkyō are the "Syphilis-preventing Inari" and the "Wife-love Inari," in Matsue is the "Children's Inari." Ancient and modern festivals, ritual, charms, and divination are described.

Less satisfactory are the chapters on "The Origin of *Inari*" and "The Fox and *Inari*." To the author under review, the dicta of Menzies, Frazer, and Hume settle the question of the origin and nature of religion, and origins are deduced from theories not too well substantiated. Race and culture are confused, and an oversimplified solution of the problem of Japanese origins lists "three distinct races, namely, the Proto-Caucasian, the Tungusic-Mongolian, and the Proto-Malay." The Ainu are assumed to represent the first "race" and Ainu practices are uncritically cited when convenient to explain Inari. Korean and Siberian tribes are regarded as representing the second strain, and scattered details of Korean and Siberian shamanism provide a lame explanation of shoulder-blade divination, votive offerings of horses, etc. To the Proto-Malay element are attributed practices analogous to customs described in Skeat's *Malay Magic*. A wider knowledge of other Asiatic peoples and of ethnographic literature might have altered the picture

Facile generalizations and rationalizations concerning "primitive" man and mentality are invoked to account for Inari origins, and although evidence cited indicates diffusion of the fox-cult from China, a personal "conviction" declares that fox-worship in Japan antedates Chinese influence. The legend of the youth who saw foxes breathing fire at night is explained by the assumption that the foxes had been gnawing phosphorescent wood, and Baelz's explanation of fox-possession is uncritically quoted: viz., that normal people think with one half of the brain, while the other half functions in possession.

With apologies to Shakespeare, the field work that men do lives after them; their theories are best interr'd with their bones. Dr Buchanan's monograph is of permanent worth, and despite the shortcomings noted, it will continue as an authoritative source. It is carefully annotated; the English bibliography is supplemented by an excellent Japanese bibliography; ideographs are provided wherever necessary; the index is adequate. The point is not stressed, but the evidence might convince a reader that the official declaration of the nonreligious character of Shintō is a mere Macchiavellian subterfuge to evade the religious freedom clause of the Japanese constitution.

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Roman Alpine Routes. WALTER WOODBURN HYDE. (Memoirs, American Philological Society, Vol. 2. xvi, 248 pp., map. \$3.00. Philadelphia: University of Pennsylvania Press, 1935.)

Professor Hyde's treatise embraces a much wider field than the title suggests. Although concerned primarily with the last century of the Republic and the Western Empire, the work is by no means limited to the confines of that interval of Roman era. Its broad historical aspect is best reflected in the significant observation that "the Alps, despite their height, have never formed an impassable barrier to man" (pp. viii-ix).

Viewed in the light of general culture history, and evaluated from the standpoint of continuity, the main subject is fittingly revealed in its proper relationship to preceding as well as to succeeding periods. The Romans did not have to open any new roads over the Alps. They simply adopted and improved various trails already laid out before their time and quite thoroughly distributed over the entire mountain system. The author maintains that "the use of the major pass-routes has been continuous from prehistoric times down to the present" (p. 194). By "prehistoric times" he specifically implies the Neolithic, Bronze, and Iron Ages, thereby adhering to the frequently preferred narrow definition. This perhaps explains the omission to mention the Paleolithic occupation, which is well documented deep in the Alps. It is obvious, of course, that a *continuous* use of ancient Alpine pass-routes is not demonstrable until from the Neolithic Age onward. Even then, however, this qualification has more of a relative than an absolute value. At any rate, some Alpine trails

were pioneered as early as the Neolithic Age, during which the Brenner Pass was settled along its entire course. Others were established in the Bronze and Iron Ages. These arteries linked widely separated areas and served to accommodate cultural traffic and ethnic movements. Over them came into the plains of northern Italy some of the peoples who were later amalgamated into the basic Roman stock. Certain cultural expressions, derived from trans-Alpine inspirations, and transmitted via such ancient trails, were absorbed in the forces which ultimately moulded the foundation of Roman civilization. Indeed, over the Alps eventually came the decisive onslaught which destroyed the Western Empire. A legacy of prehistoric pioneering with the Romans, the Alpine pass-routes became, in turn, a heritage of the Middle Ages, and finally of our own times. "Their story, then," the author concludes, "is in brief merely the increasing use man has made of them throughout the ages. For their number and location had been fixed for all time by the conformation of the Alpine barrier" (p. 194).

The scholar's wide familiarity with the Alps, and specifically his numerous reconnoitering excursions on foot, give a distinct advantage to his authority. The subject matter is conveniently treated under logical geographical subdivisions, and there is an appended discourse on the much disputed question of Hannibal's pass. Written in a fluent style, the volume is fully as enjoyable as it is profitable, and certainly fills a prominent need in American literature.

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The Tressé Iron-Age Megalithic Monument (Sir Robert Mond's Excavation). Its Quadruple Sculptured Breasts and their Relation to the Mother-Goddess Cosmic Cult V. C. C. COLLUM (xii, 123 pp., 35 pls., 14 figs. \$4.00. New York: Oxford University Press, 1935)

This megalithic monument, of the *allée couverte* type, is situated on the property of Baron Robert Surcouf in the Commune of Tressé, Arrondissement of St. Malo, Department of Ile-et-Vilaine. It belongs to a late phase of the Megalithic Age and had not been previously excavated, but had been mentioned as early as 1883 in an inventory of the megalithic monuments of the region.

The monument dates from the first century A.D., probably in the reign of Domitian. The contents of this native Gallic tomb include pottery (some hand-made, some turned on the wheel), flint and chert implements, beads, fibula fragments, bones, charcoal, an iron sword, and a Roman coin.

The chief interest of the monument lies in the two stones with pairs of sculptured bosses representing human mammae. These are supposed to represent the Great Mother, whose cult was so popular throughout Armorican Gaul in the days of the Roman Occupation (and perhaps even earlier), and which is believed to have spread from Asia Minor, Syria, and the Aegean Islands by way of the Mediterranean to the Iberian peninsula and Armorica.

The excavation and prompt publication of this remarkable monument is a reminder of the great debt archeology owes to the foresight and generosity of Sir Robert Mond.

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PHYSICAL ANTHROPOLOGY

The Physical Anthropology of the Seminole Indians of Oklahoma, WILTON MARION KROGMAN. (Pubblicazioni del Comitato Italiano per lo Studio dei Problemi della Popolazione, Serie 3, Vol. 2. xi, 199 pp., 36 pls., 6 figs., 3 maps. 20 lire. Rome: Failli, 1935.)

This publication is for the most part devoted to a very extensive analysis by Dr Krogman of a series of anthropometric measurements and observations taken on adult and sub-adult Seminole and mixed Seminole Indians of Oklahoma. In addition there are some notes by J. N. Hadley on the socio-economic status of the Oklahoma Seminoles, and a relatively small section by H. Hamlin which deals with certain vital statistics and health data on the same group.

As a fitting introduction to the discussion of the anthropometric data, Dr Krogman begins with a very clear and concise historical analysis of the ethnic make-up and population statistics of the Florida Seminoles as well as those of Oklahoma. The various elements which form the present day populations are pointed out and it is concluded quite rightly that

The Oklahoma Seminole is an extremely mixed type—non-Muskogean (Hitchiti) plus Muskogean (Creek), White, Negro. These are his essential ingredients. Add to this intermixture with other Indians and the crossing and re-crossing of all the variants, and the "full-blood" becomes at best a full-blood *Indian* rather than a full-blooded Seminole.

The anthropometric analysis which forms the main body of the report deals with observations, measurements and indices of adults of the following groups: pure-blood Seminole (60 ♂, 49 ♀), Seminole plus Creek (20 ♂, 23 ♀), Seminole plus White (7 ♂, 21 ♀), Seminole plus Negro (2 ♂, 1 ♀), pure Creek (5 ♂, 8 ♀).

Dr Krogman describes in detail the various characteristics of the above groups and finds among other interesting results that the Seminole and Creek types are substantially identical. Particular attention is paid to an attempt to establish the purity of his full-blood Seminole group by means of tests of degree of homogeneity. Comparisons are made between the standard deviations and coefficients of variation of the Seminole group with those of other series considered to be relatively homogeneous. The conclusion reached on this point is that the sample of male full-blood Seminoles has a degree of unity which argues for a minimum amount of mixture.

To the serious student of the physical anthropology of the American Indian who wishes to use these data, this reviewer must point out some important considerations. In the first place, it is found necessary to question the degree of accuracy

and reliability of the full-blood Seminole classification. Krogman selected his full-bloods on a purely genealogical basis from information given by the subjects themselves, which was later checked with Campbell's *Index* (derived from government census data). He designates as full-bloods those individuals in whose genealogy no alien blood has entered for three or four generations. Such a basis for selection is notoriously unreliable in southern and southeastern United States Indian groups, particularly when such data are obtained from the government rolls and when the possession of Negro blood is considered to be a social stigma. The lack of purity of the full-blood classification for his Seminole group is attested to by the material itself. For example, Krogman records a female full-blood Seminole with blue eyes with the comment (p. 34)

The blue eyes noted in a full-blood Seminole female was a puzzle. Her genealogy showed no White blood during the past four generations.

In addition Krogman tabulates a high percentage of non-straight hair. His figures for 59 full-blood Seminole males are 47 with straight hair and 12 or 20.3% with wavy or curly hair. For the females, out of a total of 49 he gives only 43 with straight hair and 6 or 12.2% with wavy hair. Moreover, his photographs of full-blood Seminoles show evidences of Negro and White admixture. With regard to this group, the fact that the standard deviations and coefficients of variation are not substantially higher than other relatively homogeneous series is no conclusive proof of the purity of the full-bloods. Admixture of the Seminoles with other alien stocks is of such long standing, approximately four centuries with Whites and a considerably long period with Negroes, that sufficient time has elapsed for a stabilizing of the mixed population into a relatively homogeneous group. It is true, however, if such mixtures were of a very recent nature, this fact would probably have been apparent in higher variabilities of the anthropometric characters.

And finally, the detailed tabulation of the full-blood Seminoles listed in the Appendix does not tally with the number described in the body of the manuscript. Instead of 60 full-blood adult Seminole males, the Appendix lists only 43, and instead of 49 females, the Appendix again gives only 38. What this disparity is due to, this reviewer is unable to say.

Notwithstanding these considerations, Dr Krogman is to be commended for his courage and ability in carrying through with such effectiveness a field study which started under the most trying and disheartening conditions.

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The Heritage of the Bounty: The Story of Pitcairn through Six Generations. HARRY L. SHAPIRO (xv, 329 pp., 18 illus. \$3.00. New York: Simon and Schuster, 1936.)

It is Section IV, "Biological," of the volume under review that is intended primarily for anthropologists. Having complete genealogical records in the Pitcairn Island *Register*, checked and amplified with the aid of trustworthy informants, Dr

Shapiro was able to determine precisely what combinations of Anglo-Saxon and Polynesian are present in practically every descendant of the *Bounty* mutineers and their Tahitian women living in Pitcairn. Both strains embody combinations of diverse racial heritage. Genetically, the study is therefore significant with respect to the behavior of heritable characters of two known composite racial types over a period of a hundred and fifty years. As such, it is unique and highly significant.

Hybrid vigor, well recognized in cross fertilization in the lower animal and vegetable kingdoms, is evident in Pitcairn both in stature and in fertility. Pitcairn Islanders are on the average taller than the average of the Tahitians or the English. The child-bearing capacity of the early generations of hybrid Pitcairn women is amazing. "Maria Christian was born in 1815, married at fourteen years of age, produced twenty-five children including twins, and survived three husbands" (p. 242). At the peak of their production the women of Pitcairn produced an average family of 11.4 children. (Averages in America range from 2.33 to 4.13.) In recent generations fertility decreases, possibly due to disease. Inbreeding seems not to have resulted in either physical or mental deterioration.

Dominance in inheritance of certain traits appears to be demonstrated. Tahitian skull form is dominant in length of head and constriction of brow—the distinctively Polynesian forehead, narrower than that of the English, is exaggerated so that the Pitcairners' minimal frontal diameter averages less than that of the Tahitians. On the other hand, Pitcairners have narrow faces like the English.

In the matter of eye pigmentation an interesting situation exists. The women, whose averages approximate theoretical expectations, have darker eyes than the men (p. 228).

	<i>Light</i>	<i>Intermediate</i>	<i>Brown</i>
Pitcairn men	6.45	64.51	29.03
Pitcairn women	4.84	37.09	58.06

This may indicate sex-linked inheritance of this trait. But that such exists is not proven, since no crosses of English women with Tahitian men exist to be studied. The persistence of the recessive character in the men interestingly illustrates the biological phenomenon termed "conservation of the genes."

Section III, "Cultural," gives an interesting picture of what happens when women with one heritage have been married and resettled in a new locale by men of another race and culture. Dr Shapiro has presented this phase of his story adequately for the purposes of his work. But this cultural study calls for original observation and analysis by a social anthropologist.

The author's naïve use of barbarous English spellings of Polynesian words and names deserves severe criticism. He appears to write with contented obliviousness to the painstaking monographs of his ethnological and linguistic colleagues. *Vide*: doudoe (tutu, candle-nut) on pages 145 and 146, and the spelling of the name of the famous Tahitian chiefess Purea ("Oberreah," page 35). In a different category and more blameworthy is the misspelling popoe (p. 149), for the well-known Marquesan

popoi, which is to be found repeatedly printed correctly in at least six monographs of the museum under whose auspices the author has done his Pacific research.

The Pitcairners have had the good fortune to be registered in the annals of science and history by a scientist whose sympathetic nature responded to theirs so genuinely that the reader feels the book to be as true in subtle human intangibles as it is accurate in fact. *The Heritage of the Bounty* is, moreover, delightful reading, flavored throughout with a characteristic charm and humor.

E. S. CRAIGHILL HANDY

BISHOP MUSEUM

GENERAL

The Emergence of Human Culture CARL J. WARDEN. (189 pp., 10 pls. \$2.00. New York: Macmillan Company, 1936.)

The outstanding feature of Dr Warden's book is the argument whereby the author makes his own title somewhat tautological. It is his thesis that culture is something essentially and exclusively human, possessed by no other animal. He demonstrates this at some length by a careful survey of social behavior of animal groups, in which he shows that all lack one or more of the essential mechanisms of culture, invention, communication, and social habituation. Such training or mimicry as may be observed among animals he dismisses as merely the perfecting of innate abilities, whereas social habituation is the transmission of invented behavior patterns. He does not, however, discuss the significance of regional variations in the behavior of animals of the same species, as in the hunting habits of lions in East Africa. Possibly he would attribute these to minute germinal variations in the animals in question, his only recourse, since he denies communication to all animals except man. With only this exception, Dr Warden's analysis provides a logical and convincing solution to the vexed problem of animal culture.

Insect, bird, and mammalian societies are made possible by inherited capacities for specific types of social behavior, termed biosocial mechanisms. These have evolved independently in a large number of widely separated animal forms, and there is no reason to believe that the biosocial mechanisms of one species will resemble those of another, unless it can be shown that the two species separated after the bio-social mechanisms had evolved. Hence, the social life of monkeys has no bearing on the biosocial mechanisms present in man, and it is only from the study of the great apes that we can get any idea of the biosocial substratum of human culture. The existence of such a substratum is assumed, since it is hardly conceivable that a non-social animal could develop culture. The biosocial equipment of man includes the tendency of young individuals to play together, of dominant males to protect the young and feeble, and the natural tendency to train as well as the innate ability to be docile. Such patterns, according to the author, were millions of years old in the anthropoid line before man and culture emerged.

Once culture is distinguished from biosocial tendencies, Dr Warden's remarks

about it tend to follow the conventional anthropological treatment: traits and trait complexes are defined and illustrated, but the point is made that any culture is more than the sum of its component traits, i.e., that it constitutes a system. A primary pattern of culture is worked out (somewhat like the older one presented by Wissler), and each of its component activities is related to one of the needs and traits present in human nature. The difficulties inherent in any such schematic presentation of complex and interrelated phenomena are recognized by the author, and any criticism of his primary cultural pattern could only repeat the limitations which he himself states. In a brief discussion of the secondary patterns of culture, by which he appears to mean the variations which individual cultures show on the primary pattern observable in all culture, he discusses the roles of invention and diffusion, emphasizing the factors common to both.

To the reviewer, the weakest parts of the book are the chapters on the evolution of man and the evolution of culture. Here there is little that is new, and (but this is only a personal judgment) not always the best of what is old. Dr Warden is very generous with time, giving *Pithecanthropus* and *Eoanthropus* an antiquity of one and a half million years and attributing them to the Pliocene period. Loyal to Central Asia as the birthplace of man, he goes so far as to suggest that in earliest times culture was more advanced in that region than elsewhere in the Old World. It is impossible to say that he is wrong, but the reader is entitled to some indication that there exists no proof that he is right.

In regard to culture and progress, Dr Warden questions the possibility of predicting cultural trends, and labels the dogma of social progress 'no more than a pretentious absurdity.' He foresees, however, the probable continuation of technological advance, and sounds the somewhat familiar warning that man must now work out more effective methods of social control, if civilization is not to become a social Frankenstein.

Most of the illustrations are already familiar to the student of anthropology.

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DISCUSSION AND CORRESPONDENCE

DR WISSLER ON "THE CROW INDIANS"

Dr Wissler's verdict that my book fails to give him a balanced and intimate picture of Crow life¹ must be accepted as a definitive statement of his impressions. Three statements of fact, however, require comment.

(1) Dr Wissler writes "One must search carefully to find the few names of females." His difficulty would have been lessened had he consulted the Index and the section headed "Names," which gives eight names of women and explains that personal names were not sex-linked. The section immediately succeeding would have yielded five additional women's names.

(2) The reviewer props up his contention that publication of technical papers is a waste of funds by quoting me as saying that my results had been "buried" there. However, his curtailment of my sentence distorts my meaning. The Preface says "buried *so far as the public at large is concerned*;" it states clearly that some non-anthropologists—Dr W. I. Thomas and the late Archbishop of Sweden—did gain access to those monographs. My statement therefore does not support Dr Wissler's budgetary recommendations.

(3) Finally, there is a point of method. I am grateful to Dr Wissler for making it so very clear that my book does not compete with "Anna Karenina" or "Gulliver's Travels." But, ungracious as it may appear, I cannot accept the kindly suggestion that my last eight pages escape dullness by becoming "weak as an informing document." The final chapter, like other parts of the book, rests preponderantly on verbatim quotations of native utterances or close paraphrases thereof. It is very easy to attain "human interest" by claptrap; the point is to attain it without relaxing in rigor of documentation. If I have failed in the major part of the book, it was not because it was inherently impossible to be truthful and stirring but presumably because of congenital disabilities.

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PAVIOTSO POLYANDRY

In a recent issue of the *AMERICAN ANTHROPOLOGIST*¹ Dr Julian Steward reports the occurrence of polyandry among the Shoshoni groups of eastern Nevada and southern Idaho. Their immediate neighbors to the west, the Paviotso or Northern Paiute of Nevada, are said not to have followed the practice. However, unpublished field data gathered among the Paviotso on the reservations at Walker River and Pyramid Lake indicate that polyandrous marriages were arranged in the past.

In the few cases recorded polyandry was always fraternal and never involved

¹ *American Anthropologist*, Vol. 38, pp. 654-56, 1936.

² Vol. 38, pp. 561-64, 1936.

more than two brothers. In one of these marriages, the union was first arranged between a woman and an older brother. Some time later the younger brother took up residence with the couple and the girl's parents, and was then considered a co-husband. The three slept in the same house and apparently lived together without any noticeable friction. When a child was born to the wife, both men claimed fatherhood and were so regarded by public opinion. It was not possible to learn whether or not the child addressed both by the term for father.

Another polyandrous marriage was arranged between the parents of a girl and two brothers. The brothers visited the girl's parents and talked over the matter. The brothers, reaching an agreement with the parents with no objection offered by the woman, came to live in the house of the parents-in-law. A preliminary discussion involving the consent of the parents and the girl is the usual practice in arranging Paviotso marriages. The practice of polyandry seems to differ from the usual pattern of Paviotso marriage only in respect to courtship. It was customary for the prospective husband to visit nightly the home of the girl whom he had selected as a prospective wife. He would sit quietly until the family went to bed. Usually he slept by the door and when morning came, he got up and without a word returned to his home. This continued for weeks, sometimes even months, until the girl's parents indicated by addressing him pleasantly and by offering him food that he was acceptable as a son-in-law. Then when the family retired he lay down with the girl and, if she did not get up and move her bed, they had intercourse and were then looked upon as married. Thereafter the husband lived with his wife's parents. This courtship, frequently a long drawn out affair, does not seem to have been practiced in the few instances of polyandrous marriages recorded.

There is no evidence to indicate that these were considered temporary unions for either of the brothers. In the first instance mentioned, the two husbands lived with the woman until her death. Public opinion seems to have regarded these marriages as permanent, as much so as any other marital arrangements. In this connection it may also be noted that informants were almost unanimous in holding that marriages in the old days were not frequently dissolved. Although the number of instances of polyandry was small, this permanency seems also to be characteristic of such marriages.

As in other marital unions, polyandrous marriages were not marked by ceremony or exchange of gifts. A marriage is thought of as established when a man and a woman have had sexual relations. When a man and woman, one man and several women, or two men and a woman are known to be living together and having sexual relations, they are regarded by public opinion as married without further formalities.

In the matter of residence polyandrous marriages conformed to the usual Paviotso practice. The husbands in all known cases went to live in the house of the wife's parents. The sons-in-law were then expected to contribute their efforts to the economic support of the household.

There is no evidence that one husband was always absent from the home. In fact, from the discussions of people who have heard of the practice, it seems that the

two husbands, the wife, and her parents lived together harmoniously in the usual one-roomed house. As with the much more frequent polygynous marriages there seems to have been no outward manifestations of sexual jealousy.

Under present conditions it is impossible to get an accurate idea of the frequency of polyandrous marriages. There can be no doubt, however, from the recorded material that polyandry was not a common practice. Certainly, it has had no observable influence on kinship terms such as has been noted by Dr Steward.² Nevertheless, such marriages were socially recognized as legitimate unions. It is possible that the Paviotso learned the practice from their eastern neighbors, the Shoshoni, for several informants were acquainted with the Shoshoni custom of polyandry.

It is difficult to see in the Paviotso institution of polyandry any organic connection with economic factors. Marriage involves no exchange of property, and women in their activities as providers of wild vegetable food have an economic role as important as that of men. Moreover, groves of pine trees, the only form of property to be inherited, belong to the family. As there is no fixed line of descent for such property, use is the more important factor in determining ownership.

There can be no doubt that, as Dr Steward has indicated with the Shoshoni,³ Paviotso polyandry is linked with the levirate. On the other hand, the Paviotso, unlike the Shoshoni, do not practice any form of preferential mating. Although Dr Steward holds that there is no excess of marriageable males to account for the development of the Shoshoni practice,⁴ it is quite likely that among the Paviotso the relatively frequent occurrence of polygynous marriages, in some cases involving as many as four wives to a man, may have resulted in an occasional shortage of available females. Under these circumstances the extension of the privileges of brothers, usually recognized only at the death of one, while both are living, then becomes intelligible. Given these two conditions, the relatively infrequent practice of polyandry may have developed indigenously among the Paviotso or, as may appear more likely to the diffusionist, the practice was copied from the neighboring Shoshoni, where it seems to have occurred more frequently and to have exerted greater influence on such aspects of the social life as the kinship system.

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NORTHERN PAIUTE POLYANDRY

In his article *Shoshoni Polyandry*,¹ Dr Julian H. Steward said, "It is reasonably certain that it [polyandry] was not practiced by the Shoshoni of southern Nevada nor by the adjoining Northern Paiute." During the summer of 1936, while making a culture element survey of the Northern Paiute for the University of California, I recorded the presence of polyandry among five of the eleven Northern Paiute bands visited. The bands admitting polyandry are now located at Burns in Oregon,

² Pages 563, 564

³ Page 562

⁴ Page 561

¹ *American Anthropologist*, Vol. 38, pp. 561-64, 1936

and at Pyramid Lake (Nixon), McDermitt, Winnemucca, and Owyhee in Nevada. The bands at Winnemucca and Owyhee are on the border between Paiute and Shoshoni peoples and probably were influenced by the Shoshoni in regard to marriage, as we know they were influenced in regard to other traits.

The informants at Winnemucca gave no details concerning polyandry, but at Owyhee I obtained a fair account of a present-day case which involved both Shoshoni and Paiute. At this reservation a Shoshoni woman was married for a long time to a Shoshoni man. A few years ago a young Paiute man started living with them; all three slept in the same bed. However, as the first husband was quite senile, sexual relations were only between the woman and her second husband. I should state that at the present time both Paiute and Shoshoni live on the Owyhee Reservation. However, my informant assured me that polyandry was known among the Paiute when they lived in Oregon about a hundred miles to the northwest.

At Burns, Oregon, the strongly Catholic informants gave no information beyond the admission that "old time Paiute women sometimes had two husbands at once." While at McDermitt, I was told that "right now a woman here has two husbands who are not brothers." But the informant went on to say that polyandry would not have been allowed in the old days. Although a hundred and twenty-five difficult miles separate the two, the McDermitt Indians have had much contact with the Shoshoni since the establishment of the Owyhee Reservation in 1877.

The most detailed evidence of Northern Paiute polyandry was obtained at Pyramid Lake, which is located in about the center of this area. On this reservation is a woman who has lived with two men for many years. The men are not brothers, and, my informants said, they share equally sexual privileges and economic burdens. The children call both men father, and together the men work their land. Unfortunately, no details could be had concerning any pre-contact cases at Pyramid Lake, although both informants had heard that in the old days women sometimes had more than one husband at a time.

However, polyandry was not a common feature in Northern Paiute social life, and it was not institutionalized. Dr Steward's explanation of Shoshoni polyandry as a result of the relative lack of male jealousy and the "extraordinary simplicity of social structure which made the relationship of both sexes to plural marriage almost identical" could also apply to Northern Paiute polyandry.

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BEADS FOUND IN MEDIEVAL AFRICAN RUINS

In continuation of his paper, *Some Tuareg Ornaments and Their Connection with India*,¹ Mr A. J. Arkell has published another paper, *Cambay and the Bead Industry, in "Antiquity."*²

¹ Journal, Royal Anthropological Institute, Vol. 65, pp. 297-306, 1935.

² Vol. 10, pp. 292-305, 1936.

Readers will recollect that the alleged Solomonic origin of the ruined cities of Rhodesia (Zimbabwe, etc.) was disproved to the satisfaction of the Royal Society by Miss Caton Thompson, who found glass beads of Indian manufacture which dated to about the tenth century A.D. in the lowest strata

Mr Arkell's papers are of considerable importance as it has been the practice of travellers and explorers to speak of finds of these medieval beads on deserted sites in Africa as evidences of ancient Egyptian or Asiatic culture. The present process of making beads at Cambay is described by Mr Arkell and presents a marked contrast to the Bicharin method. The present writer has some beads made from pebbles which were rounded and polished by Bicharin boys turning them round by stirring them in sand in a similar manner to stirring a pudding. The process is a long one and is a pastime among the herders and shepherds, similar to the whittling of sticks, etc., attributed to backwoodsmen.

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SOME LINGUISTIC FEATURES OF SPECK'S "NASKAPI"

At last Professor Speck has given us this long awaited and splendid ethnological book,¹ the result of nearly a quarter of a century's intensive field-work among these hunters. It will therefore be thought almost impudent for another to present adverse criticism. However, even at the risk of being labelled a chronic fault-finder I shall point out that the linguistics in this book are not all that they should be, and that at times this has an ethnological bearing.

Thus on p. 37 Kantce'təpeltə'k is translated "Our Great Master or Owner." A grammatical analysis shows such a translation is impossible, for the third person animate singular is obviously the subject and the third person inanimatae is the object, as is shown by the medial -It- to say nothing of the termination. So too nemictu'wəts "thunder" (p. 62) would seem to be plural, not singular in spite of Lemoine.² On p. 118 təpənəmə'k nəme'c is translated "he who governs fish." This is impossible, for the first word demands the first person singular as subject and the third person animate singular as object; nor is the second word an obviative in form as would be necessary if the third person animate had been subject. Observe on p. 119 there is the sentence "Məsəna'k" I am called, the one who is master of the fish." Of course "I who am the master of fish" is implied. So again on p. 127 "he sees his soul" is in error, as shown by grammatical analysis. Similarly, elte'ltamən "he thinks" (p. 184) is in contradiction to what more than a dozen and a half Algonquian languages tell us. All this indicates that Speck has relied far too much on native informants and interpreters without any serious attempt to check them.

Scattered through the book are numerous etymologies. But, I regret to say, hardly one of them can be sustained, they are "popular" etymologies but devoid

¹ Frank G. Speck, *Naskapi* (Norman, Okla., 1935)

² *Dictionnaire Français-Montagnais*, pp. 247, 272.

of scientific foundation. So, for example, we are told on p. 192, "Among the Mistassini beads are called *mi'tcots*, a term having apparently some relation to the verb 'to eat' (*mi't'cu*)."¹ The collective evidence of Sauk, Fox, Kickapoo, Menomini, Ojibwa, Algonkin, and Cree shows that the words under discussion only resemble each other because of a specific Montagnais-Nascapi phonetic shift (Cree *mīkis* "bead," *mītci* "eat," both in Bloomfield's transcription). So, too, on p. 50 we are told that *atcakwu'c* means "little soul" and "star," and that the plural used for both these meanings is the animate plural of *atca'k* "soul" which lacks the diminutive suffix. Now, even if these words are well recorded, the published Algonquian linguistic material, if studied in conjunction with Bloomfield's work on Algonquian phonology, show unmistakably that historically distinct words are fused.

But another possibility is present, namely, are these words correctly recorded? Any student of comparative Algonquian linguistics will observe that probably universally (or nearly so), where a Greek *spiritus asper* is to be expected before *k*, *t*, *p*, etc., none is recorded in the Indian words of this volume. Now I cannot categorically deny that there may be some Cree and Montagnais-Nascapi dialect(s) in which such a loss actually occurs. Nevertheless my experience with the Cree of Fort Totten, Files Hills, Moose Factory, Albany, Attawapiskat, and Weenusk, and the Montagnais-Nascapi of Lake St. John, Mistassini, Northwest River, Davis Inlet, Rupert's House, East-Main, Fort George, and the Great Whale River makes me very skeptical as to this possibility (unpublished material of another on Nichigun confirms this). Now a Temiscaming Algonkin text by Speck, published over twenty years ago, shows that the Greek *spiritus asper* is recorded, where expected before *t* (save once), but not before *k* (if *g*=*k*, save once). I can hardly credit this with being a true dialectic difference. Watkins gives Cree *'Acha'k* "spirit, soul," *Ūchu'koos*, *Ūchu'k* "star;" these in Bloomfield's transcription are *ahtcāhk*, *atcāhkus*, *atcāhk*; Lacombe gives *atchākus*, *atchakus*, *atakus*, *atak* for "étoile" ("petite étoile") and *atchāk* "âme" (disregarding in all cases capitals and italics). Any one who has used Lacombe knows the advantages and disadvantages of his transcription. However, Speck should have noticed the variants *atakus* and *atak*, for more than eleven years ago Bloomfield gave Cree *atāhk* "star."

I have recorded Moose Cree *Ahtcāhk* "soul," *Atcahk*, *Atcahkus* "star;" Albany Cree *A'tcā'hk* "soul," *watcā'hkuc* "star;" Attawapiskat Cree *hahtcā'hk* "soul," *watcā'hkuc* "star" (recorded as *watcā'hkuc* once); Weenusk Cree *hatcā'hk* "soul" (from a young informant without characteristic phonetics), *watcā'hkuc* "star;" Rupert's House and Fort George Montagnais-Nascapi *Ahtcāhk* "soul," *Atcahk* "star" and *Atcahkus* "star" respectively ("soul" at Fort George is totally different). Bloomfield years ago has given ample warning of the difficulty in distinguishing long and short vowels in Cree before *h* followed immediately by a consonant.

Fox *anāgwa* "star," Menomini *anāh* (Bloomfield; plural *anāhkuk*), Ojibwa *anāng*, Delaware (Brinton) *allanque*, Peoria *langwa* (Gatschet) prove that Proto-Algonquian archetype for "star" is **aθānkwa*, set up by Bloomfield years ago. Knowledge that **θ* appears as *t* in Cree and Montagnais-Nascapi is taken for

granted. The forms in *-s*, *-c* are diminutives. The *tc* in the variant forms of the words for "star" is unoriginal; just as Cree *Ati'm* "dog" (**aθemwa* as proved by Kickapoo *anemwa*, etc.) has beside it *Atci'mucic* "puppy." This consonantic change in diminutives is living and had its origin in such cases as Cree *Awā'cic* "child," *ndawā'cimic* "my child," *utawāci'mica* "his child" (Bloomfield *utawāsimisah*), where the *d* is for the regular intercalated *-t-* after a preceding nasal consonant; and the *tc* is for *t* (Proto-Algonquian *t*)³

In conclusion I agree with Miss Flannery⁴ on *Mistapeo* as opposed to *Speck*, and point out that the distribution of the earth-diver theme, as given on p. 55, is wholly in error: it occurs at Chimo, Fort George, and Rupert's House.⁵

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³ Similarly Plains Cree *nitsāhkus* (Bloomfield) "my sister-in-law" (ego female).

⁴ *American Anthropologist*, Vol. 38, p. 492, 1936.

⁵ Printed by courtesy of the Smithsonian Institution.

NOTES AND NEWS

GUERNSEY MEMORIAL COLLECTION

Dr and Mrs A. V. Kidder have presented to the Peabody Museum of Harvard University, in memory of the late Samuel J. Guernsey, a very valuable series of one hundred thirty-five pots collected by them between the years 1907 and 1925 from the Pueblos of the Southwest. This gift contains specimens illustrating the potter's art from approximately the middle of the nineteenth century to the present time. Of special importance is a series of vessels illustrating the growth of the two new styles of pottery developed at San Ildefonso and Hano, respectively, by Maria Martinez and Nampeyo. It is to be pointed out that this collection can never again be duplicated.

MEMORIAL TO JOSEF BAYER

During the session of the International Congress of Quaternary Geologists, held in Vienna, September 1st-9th, a monument to the memory of the late Dr Josef Bayer was unveiled at Spitz on the Danube. The spot very appropriately selected for the monument is near the world-renowned prehistoric station of Willendorf, where the Paleolithic figurine known as the Venus of Willendorf was found.

ANNUAL MEETING OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION, 1937

President N. C. Nelson has appointed the following committees for the annual meeting in New Haven, December, 1937:

Program Committee: J. M. Cooper, *chairman*, M. W. Stirling, W. D. Strong, F. M. Setzler.

It was voted that the Program Committee be instructed to issue a call early in October for papers designed for the annual meeting, the titles and full papers to be in the Committee's hands by November 1st, and that the Committee be authorized to select a limited number of papers and addresses to be presented at the joint sessions of the annual meetings of the societies involved [from the Minutes of the Andover meeting, December, 1935]

Nominating Committee: Fay-Cooper Cole, *chairman*, A. M. Tozzer, R. F. Benedict.

Within three months of his election the President shall appoint a Nominating Committee and transmit the names . . . to the Editor who shall publish the names . . . , with an invitation for suggestions, after considering such suggestions the Nominating Committee shall report its slate to the Council which shall pass on the recommendations, with such changes as are deemed advisable, to the annual meeting [from the Minutes of the Pittsburgh meeting, December, 1934]

F. M. SETZLER, *Secretary*

RECENT DEATHS

We regret to report the death of Dr Giuseppe Sergi, emeritus professor of the University of Rome, on October 17, 1936, at the age of 95. To the majority of anthropologists Dr Sergi was best known for his book, *The Mediterranean Race*, first published in Italian in 1895.

The lamentable death of Sir Grafton Elliot Smith, Professor of Anatomy in the University of London, occurred in London, January 1, 1937, at the age of 65. His contributions to anthropology will receive notice in a subsequent issue of this journal.

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NEW SERIES

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NO. 3 (PART 1)

CULTURE MIGRATIONS AND CONTACTS IN THE BERING SEA REGION

By HENRY B. COLLINS, JR.

IT is not my intention to speculate upon very early or original migrations of man into America which in all probability did take place around Bering Strait. During the past ten years archaeological investigations in this region have thrown considerable light on the problems of Eskimo prehistory. They have not, however, revealed the existence of any pre-Eskimo remains or provided evidence relative to original human migrations across Bering Strait. The purpose of the present paper is to discuss briefly certain facts brought to light by recent investigations in northern and southern Alaska that seem to indicate continental and intercontinental movements of culture or population in these regions, even though the movements are not of a primary nature or in the direction usually thought of.¹

The first such movement, which affected northern Alaska, is definitely secondary, but it is for that reason of essential importance to a proper evaluation of existing conditions. I refer to the Thule culture and the part that it seems to have played in the formation of the present Alaskan Eskimo culture.

When Mathiassen discovered the widespread remains of the prehistoric Thule culture north and west of Hudson Bay in 1921-24 he assumed, and with every reason for doing so, that it had spread to these regions from Alaska. He was led to this belief principally by the very close resemblances between the Thule culture and that of the modern Point Barrow Eskimo.

We find that of the 152 unquestionable Thule elements we recognise no fewer than 94 in the Pt. Barrow district, and, what is more, they are for the most part types which belong to the most characteristic in the Thule culture, as for instance 22 of the 31 "representative forms" of the Thule culture. . . . All in all, one must say that the likeness between the Thule culture and the Pt. Barrow culture is exceedingly great; and in respect to these it is not nearly so necessary, as in West Greenland, to turn to old finds for the purpose of finding parallels to the Thule culture. The greater part of the elements mentioned above are in use among the

¹ The subjects here considered are dealt with somewhat more fully in a forthcoming paper presenting the results of the writer's excavations on St. Lawrence Island, Alaska, in 1930

Pt. Barrow Eskimos to this day, or at any rate they were when Murdoch visited them in the 1880's, and for the most part these are elements which play a predominating part in their culture . . . There is hardly any doubt that the Pt. Barrow Eskimos are the Eskimo tribe living to-day that most closely approaches the Thule culture.

We must therefore imagine that the Thule culture, with all its peculiar whaling culture, has originated somewhere in the western regions, in an Arctic area where whales were plentiful and wood abundant, and we are involuntarily led towards the coasts of Alaska and East Siberia north of Bering Strait, the regions to which we have time after time had to turn in order to find parallels to types from the Central Eskimo finds. There all the conditions have been present for the originating of such a culture, and from there it has spread eastwards right to Greenland, seeking everywhere to adapt itself to the local geographical conditions. And it can hardly have been a culture wave alone, it must have been a migration.²

Mathiasen postulated an age of 1000 years for the Thule culture in the Central regions, which seems not unreasonable in view of the fact that his subsequent investigations in West Greenland have revealed a later stage of the same culture that appears to have been contemporaneous with the Norse settlements of the 13th or 14th centuries.³

If the Thule culture of Greenland and Canada had an antiquity of from 500 to 1000 years, and if its general character was such as to indicate close relationship with north Alaskan culture, particularly Pt. Barrow, one might reasonably expect that excavations in Alaska would reveal the immediate source from which it had sprung. However, when we compare the Thule elements with those of the Old Bering Sea and Birnirk, the two oldest stages thus far known in Alaska, we find far fewer correspondences than exist between the Thule and modern Pt. Barrow culture. Without going into detailed comparisons here, it may be stated that the Thule elements present in the older Alaskan stages are simple, fundamental elements, most of which are also present in the modern culture, these indicate therefore only a general, basic relationship between the Thule culture and Western Eskimo culture as a whole. On the other hand, when we take into account the specific aspects of those features which exhibit variability, we see that in almost every instance the immediate resemblances are between the Thule culture and the later (post-Old Bering Sea or post-Birnirk) stages of culture in Alaska. Thus, the Punuk stage on St. Lawrence Island, though exhibiting many distinctive features—some derivatives from Old

² Therkel Mathiasen, *Archeology of the Central Eskimos* (Report of the Fifth Thule Expedition, 1921-24, Vol. 4, Copenhagen, 1927), Part 2, pp. 174, 175, 176, 184.

³ Therkel Mathiasen, *Ilugluk, a Medieval Eskimo Settlement in Upernivik District, West Greenland* (Meddelelser om Grønland, Vol. 77, København, 1930), pp. 284-303.

Bering Sea prototypes, others new importations from Siberia—is yet in other respects very close to the Thule culture of the Central regions. The fact that most of the “Thule” features of the Punuk stage appear quite suddenly on St. Lawrence Island, with no indication of connection with earlier local forms, points to their having been introduced, along with the above mentioned non-Thule elements. For this reason the St. Lawrence finds themselves throw no direct light on the problem of the origin of the Thule culture. This, according to all indications, is to be sought farther north. However, we have lacked precise information on this point, and hence the exact status of the Thule culture in Alaska has been somewhat obscure.

At present we know of but one site in Alaska that can be regarded as definitely representing the prehistoric stage of Thule culture. This is the mound site at Cape Prince of Wales where Jenness first excavated in 1926 and where I carried on further work during the summer of 1936. On the basis of cultural sequences previously established for St. Lawrence Island and Point Barrow, the Thule site at Wales belongs to the intermediate stage of prehistoric Alaskan culture, being contemporaneous with the Punuk stage on St. Lawrence Island. It is later than the Birnirk stage, from which it seems to have been a direct outgrowth; the latter, in turn, has developed from the Old Bering Sea, the oldest stage of culture thus far known in northern Alaska.

While we may assume, on the basis of trait comparison, that the Thule site at Wales dates from about the time when other groups of Eskimo, probably east of Barrow, were pushing still farther eastward toward Hudson Bay, we have still to account for the even closer resemblances between the Thule culture of the Central regions and that of the modern Point Barrow Eskimo. A crucial point with regard to the status of the Thule culture in Alaska is that some of the most characteristic and important Thule elements—such as soapstone lamps, small ivory bird figures, drilled lashing holes and rivet holes on harpoon heads, and objects used in connection with the dog sled or harness—are lacking entirely at the prehistoric Alaskan sites but are among the most prominent features of the modern and protohistoric phases of North Alaskan culture. How are we to account for the fact that these typical Thule elements are prominent in the later culture of the north Alaskan Eskimo but are not found at any of the older sites? The only satisfactory explanation seems to be that such elements were introduced into northern Alaska within the past few centuries by a late return migration of Thule Eskimo subsequent to the original eastward spread of the Thule culture.

There is a strong probability that the modern Point Barrow type of house is also one of these later intrusive elements. South of Bering Strait we find a house which while varying somewhat from place to place is still of a single general type, in which the roof is at least partially domed and supported by uprights set either in the floor or along the walls, and in which low platforms extend around two or more sides. The Point Barrow house differs fundamentally from this type in roof structure and in the arrangement of the platform; the roof being gabled, with a double slant, and supported by a single transverse ridge pole resting on the wall uprights, while the single platform extending from the back wall is wide and high and occupies about a third of the room. Both the gabled roof and the rear platform are characteristic of Eastern Eskimo houses. Considering, therefore, that the Point Barrow house is the farthest removed of all Alaskan houses from the general type which occurs south of Bering Strait and that the features which set it apart from other Alaskan houses are the very features which connect it with those of the East, we seem to have valid grounds for assuming a relationship between the Point Barrow and Thule houses.

Boas has called attention to the fact that the folklore of the Alaskan Eskimo points to an eastern origin.⁴ This fact, and conditions with regard to linguistics, seems to provide further evidence of a relatively late wave of migration entering northern Alaska from the eastward. The linguistic evidence is particularly striking, for according to Jenness, the Eskimo dialects in Alaska north of Norton Sound are closer to those of Greenland and Labrador, more than two thousand miles to the eastward, than they are to those of the Yukon-Kuskokwim region immediately to the south.⁵

⁴ Franz Boas, *The Eskimo* (Proceedings and Transactions, Royal Society of Canada for the Year 1887, Vol. 5, Sec. 2, Montreal, 1888), p. 39.

⁵ Diamond Jenness, *Ethnological Problems of Arctic America* (Special Publication, American Geographic Society, No. 7, 1928), p. 174; *The Problem of the Eskimo (The American Aborigines, their Origin and Antiquity)*, Toronto, 1933), pp. 379-80.

Since the present paper was written the long awaited report on the skeletal material collected by Mathiassen on the Fifth Thule Expedition has appeared (K. Fischer-Møller, *Skeletal Remains of the Central Eskimos*, Report of the Fifth Thule Expedition, 1921-24, Copenhagen, 1937), and the results are entirely consistent with the above interpretation of the cultural and linguistic evidence. In 1934 the present writer suggested that since the three Eskimo groups whose culture was closest to the Thule—the recently extinct Sadlermiut of Southampton Island, the Polar Eskimo of Smith Sound, and the modern Point Barrow Eskimo—were all closely related physically, it seemed more reasonable to assume that the physical type originally associated with the Thule culture was exemplified by them than by Van Vahin's pre-Thule group from Barrow (Collins, *Eskimo Archeology and Somatology*, American Anthropologist, Vol. 36, 1934 p. 312). From Fischer-Møller's report we now see that this was actually the case: the physical type of the Thule Eskimos, as revealed by the skeletal remains from

The hypothesis that the north coast of Alaska has been subjected to a relatively late wave of migration from the eastward, would serve to explain in large measure the rather sharp line of demarcation between Alaskan Eskimo culture north and south of Norton Sound. A more important implication, but one that would naturally follow, would be that we are here provided with at least a partial explanation of one of the most striking phenomena of Eskimo culture, namely its remarkable uniformity. This homogeneity, both with regard to language and culture, which has been so often remarked upon and interpreted as indicating the recency of Eskimo culture, may instead be itself a recent condition, brought about through the levelling influences of a late wave of Thule culture from the eastward. Prior to this there was probably greater diversity of culture in the American Arctic; this would be true particularly of the earlier period when the Old Bering Sea and Dorset cultures occupied the regions of Bering Strait and Hudson Bay, respectively.

It might also be inquired whether the postulated late flow of Thule culture into northern Alaska might not be responsible in large measure for the conditions that led to the formulation of the theory that the Eskimo, coming from the eastward in relatively recent times, had entered as a wedge at Bering Strait, breaking off an earlier connection between the Palaeasiatic tribes of Siberia and the Indian tribes of the Northwest Coast. In summarizing the results of the Jesup Expedition, Boas cites the following evidence as pointing to such a conclusion:

The culture of the Chukchee, who inhabit the extreme eastern part of Siberia, is quite similar to that of the Eskimo, with the important exception that the Chukchee are reindeer-breeders, while the Eskimo are purely hunters. The similarity between the life of the Chukchee and that of the neighboring Koryak is great, although the characteristic Eskimo features tend to disappear. An analysis of the religious ideas and of the folk-lore of these tribes gives us the unexpected result that among the Chukchee we have not only a great number of Eskimo stories, but also a considerable number of Raven myths, which show a striking analogy to Raven traditions of the Indians of the North Pacific coast. Among the Koryak and Kamchadal the Eskimo elements become much fewer in number, while the relative proportion of Raven myths which show similarity to Raven tales of America is much larger. This feature is so striking that Mr. Bogoras and Mr. Jochelson have independently reached the conclusion that a close affiliation exists between eastern Siberian folk-lore and that of southern Alaska and British Columbia. Mr. Jochelson finds that the Koryak have many incidents in their tales in common with

Naujan, the most important of the Thule sites in the Central region, is seen to have been practically identical with that of the modern Point Barrow Eskimo and entirely distinct from the prehistoric Barrow type

the Old World and with the North American Indians, and quite a number which are common to the Koryak, the Eskimo and the Indians, but none that belong to the Koryak and to the Eskimo alone. This is clear evidence that contact between Koryak and Eskimo is more recent than that between Koryak and Indian.

This clew once given, we investigated the cultural similarities in this whole area, and found ample evidence that there must have been, at an early period, an intimate relationship between the Indian tribes of the Pacific coast and the peoples of eastern Siberia. The peculiar fact that this relationship comes out much more clearly some distance to the west of Bering Strait, particularly among the Koryak, proves that the similar traits of culture cannot have been transmitted indirectly through the Eskimo.

... So far as the available material allows us to judge, it would seem that the similarities between the Eskimo and the North Pacific Coast Indians are unimportant as compared to the similarities between the Koryak and Chukchee and these Indians. We must infer from these facts that the Eskimo are new arrivals on the Pacific side of America, that their original home was somewhere near, or east of the Mackenzie River, and that they interrupted, at an early period, the communication between the Siberian and Indian tribes, which left its [trace?] in many cultural traits common to the peoples on both sides of the Bering Sea.⁶

The possibility suggests itself that the Thule Eskimo may have brought with them from the Central regions not only Eastern dialects and culture but also an Eastern Eskimo pattern of folklore, and that it was the introduction of the latter that produced the break at Bering Strait. This, of course, is only a supposition, which would be difficult either to prove or disprove. However, since the effects of the presumed late wave of Thule culture seem to have been felt to some extent by the Chukchee, it would appear as by no means improbable that their mythology, along with that of the Western Eskimo, had been influenced in the same way. The Koryak and Kamchadal, on the other hand, would not have been affected to the same degree, and consequently the Indian elements in their mythology would have remained prominent. A suggestion as to how the resemblances in Siberian and Indian mythology may have come about will be mentioned in connection with the problem discussed below, namely, cultural relationships in South Alaska, the Aleutian Islands, and Kamchatka.

The investigations of Jochelson, Hrdlička, and de Laguna have provided detailed information on the early forms of culture in the Aleutian Islands, Kodiak Island, and Cook Inlet. A basic relationship with northern Eskimo culture is shown by the occurrence here of such types as chipped stone and rubbed slate blades, flakers, adzes, whetstones, drill points.

⁶ Franz Boas, *The Jesup North Pacific Expedition* (International Congress of Americanists, 13th Session, 1902, New York, 1905), pp. 97-99.

toggle harpoon heads, foreshafts, socket pieces, barbed dart points, awls, needles, spoons, shovels, wedges, side prongs for bird darts or fish spears, drum handles, pendants. In only a few instances, however, are there specific resemblances to the northern types; the South Alaskan forms are often distinctive in appearance and rather far removed from these.

Furthermore, when we consider the special types which give the Cook Inlet, Kodiak, and Aleutian cultures their individual stamp, we see that these are elements which, though more or less characteristic of the general pattern of culture prevailing in South Alaska and the North Pacific region, are with a few unimportant exceptions, lacking in northern Eskimo culture. The following elements, which de Laguna attributes to the prehistoric Cook Inlet culture, may be cited as examples: dismembered burials, burial on top of refuge island, wooden masks for the dead, trophy heads, artificial eyes on trophy skulls, utilized human bones, notched and grooved stones in large numbers, the splitting adze, pestles, grinding slabs and stones, slate "awls," stone clubs, stone saws, elaborate stone lamps of special form, slate ulus or scrapers with chipped edge, slate and shale mirrors, beds or grooves on dart heads for the blade, dart heads with wide flattened tangs, harpoon socket piece in two parts, foreshafts with wide flattened tangs, compound fish hooks with curved shanks and barbs, cut animal bones, labrets, fish vertebra rings, nosepins.

With northern Eskimo culture there has evidently been a basic, early relationship, but the development of South Alaskan culture has been virtually independent of influences from the northward. On the other hand there is unmistakable evidence of cultural relationship between South Alaska and a fairly restricted area along the east Asiatic coast.

De Laguna has listed a number of typical Cook Inlet features which occur also in Kamchatka.⁷ In addition to certain widespread types like stone blades, dart points, etc., these include the refuge island, notched and grooved stones, stone with hole, grinding stone and slab, oval stone lamp, lamp with ring, labret, large bone arrow head with blade but no barbs. In Neolithic Japan we find a larger number of the simple, more widespread types and fewer of the special Cook Inlet types. Among the latter are notched and grooved stones, toggle harpoon heads with closed socket and line hole in plane of the spur, large arrow head with blade but no barbs, and broken and cut human bones.

There is also a clear relationship between the houses of the Aleutian Islanders and those of the Kamchatkans. The features which distinguish the

⁷ Frederica de Laguna, *The Archaeology of Cook Inlet, Alaska* (Philadelphia, 1934), pp 216-17

Kamchadal house from that of the neighboring Koryak (rectangular floor plan, low sleeping platforms along three sides, and absence of the elaborate storm roof) are features which it shares with the Eskimo houses south of Bering Strait. Furthermore, the absence of an entrance room and the presence of a roof entrance, brings the Kamchadal house into direct relationship with the Aleutian house particularly.

The roof entrance, the feature which distinguishes the Aleutian house from adjacent American forms, has a wide and sporadic distribution to the southward, where it is found from southern British Columbia to California and again in the Pueblo region of the Southwest. In Asia it is known only among the Kamchadal and Koryak, who occupy the coastal region nearest the Aleutians, although Bishop is of the opinion that the beehive shaped pit dwellings of the Chinese Neolithic were entered at the top.⁸

The case of the stone lamp is very similar. In addition to the common feature of the suspension hole on the small hunter's lamp in the Aleutian Islands and Kamchatka, which de Laguna has pointed out, there is also a general similarity in size, shape, and treatment of the rim that unites the South Alaskan and east Asiatic lamps and sets them apart as a regional group distinct from the crescent-shaped stone lamps of the Central and Eastern Eskimo. Since the South Alaskan stone lamps connect in all probability with the mortars and other forms of stone vessels in the non-pottery area extending from the North Pacific coast down to California, we seem to have a continuous distribution of stone vessels from Asia to America. It should be emphasized, however, that the cultural continuity thus indicated is strictly North Pacific—northward to and including the Aleutian chain on the American side, then westward to Kamchatka and the Kuriles in Asia. There is no evidence that the Eskimo north of Norton Sound (probably even north of the Kuskokwim) or the Asiatic tribes north of Kamchatka, were in any way affected. Here, on both sides of the Bering Sea, was an extensive area where stone vessels were originally unknown and where pottery was the all important material.

The distribution strongly suggests that the stone lamp, the roof entrance on houses, and also the labret, are American elements which were introduced into Kamchatka and the regions immediately adjacent, for while they are widely distributed in western America they are restricted in Asia to that part of the coastal region lying closest to the Aleutians. In addition to these three elements, the others which were mentioned previously as being common to Cook Inlet and Kamchatka—refuge island,

⁸ C. W. Bishop, *Archaeological Field-Work in China* (Explorations and Field-Work of the Smithsonian Institution in 1934, Washington, 1935), p. 44.

notched and grooved stones, stone with hole, grinding stone or slab—were probably introduced in the same manner. It is difficult to see how the connection could have been established other than by way of the Aleutian chain, for while most of the elements mentioned are among the most characteristic forms of South Alaskan culture, particularly of the Aleutian Islands, they are lacking in the north; therefore, they could hardly have passed over at Bering Strait.

Most of the more recent writers who have discussed the problem of Asiatic and American relationships have felt that the Aleutian Islands were not a migration route into America. This view, I think, is entirely correct, for it is difficult to imagine Asiatics leaving the mainland in Kamchatka and setting out eastward over the open sea where there was no land visible. And even if they had reached the Commander Islands they would have before them the still greater stretch of open water to Attu, the westernmost of the Aleutians. Improbable as such a theory would appear from a geographical standpoint, the strongest evidence against it is cultural. The fact that the known cultural remains on the Aleutians are of an essentially Eskimo or American character, as demonstrated by both Dall and Jochelson, shows that the islands must have been peopled from the Alaskan mainland. The presence of pottery at all of the known prehistoric sites in Kamchatka and its absence in the Aleutian Islands would itself tend to preclude the idea of a west to east movement (from Kamchatka to the Aleutians), for if this had occurred, it would seem that pottery would have been introduced into the latter region. When we consider, on the other hand, that the early Aleuts must have been expert navigators to have settled and maintained contacts between the widely separated islands, it would seem by no means an insuperable feat for them to have pushed on and reached the Commander Islands and then the Kamchatkan peninsula. The Commander Islands were uninhabited when discovered by Bering in 1741, and no traces of kitchen middens or aboriginal house ruins have ever been reported. It is a question, however, whether these have been really searched for. Some of the Aleutian middens, even the largest, are at the present time very inconspicuous, appearing either as grass covered knolls along a hill slope or as natural ridges along the shore. The indications of cultural connections between the Aleutians and Kamchatka are so clear as to lead to the expectation that evidences of aboriginal occupancy will eventually be discovered on the Commander Islands.

If the hypothesis of a westward migration from the Aleutian Islands to Kamchatka should be borne out, we would seem to have at least a partial explanation of the cultural resemblances between the Northwest Indians

and the Palaeasiatic tribes of Siberia which were revealed through the investigations of the Jesup Expedition. As pointed out above, it seems by no means improbable that the late wave of Thule culture which penetrated into northern Alaska as far west as Bering Strait might have introduced, among other Eastern culture traits, an Eastern form of folklore which blended with and to a certain extent supplanted a mythology in which Indian-Siberian elements had been more prominent. Whether the Indian elements in Siberian mythology had been transmitted directly across Bering Strait or over the Aleutians to Kamchatka would remain an open question. The fact that these elements are strongest among the Kamchadal and Koryak would seem to favor the Aleutian route, but an adequate explanation would require a more thorough analysis of Alaskan mythology than has as yet been made.

Whatever the conditions may have been with regard to mythology, there seems little likelihood of Indian elements of material culture having been carried back into Siberia by way of Bering Strait after the Eskimo had become established there, for the Old Bering Sea culture, elaborate though it is, is in every sense Eskimo and, with the possible exception of art, shows few significant resemblances to the Northwest Coast. In the Aleutians, on the other hand, we find an aberrant form of Eskimo culture which embodied a number of important elements characteristic of South Alaskan culture generally, and when we observe that some of these have succeeded in gaining a foothold in Kamchatka and the regions immediately adjacent, it seems plausible to regard them as American elements which drifted into Asia over the Aleutian chain.

UNITED STATES NATIONAL MUSEUM
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THE INDOGERMANIC MOTHER LANGUAGE AND MOTHER TRIBES COMPLEX¹

By C. C. UHLENBECK

LIKE a very deep furrow there runs through the Indogermanic linguistic stock a dividing line, broken into capriciously by secondary shiftings, between the centum and satəm languages. These shiftings have brought it about that we can no longer separate centum and satəm from each other by a single line, as even centuries ago there were already centum languages in the satəm area, and even further afield. But by the employment of a certain amount of imagination we can conceive how the deep furrow at one time separated two unified areas. What we cannot conceive is that the different satəm groups, independently of each other, might have transformed the old palatal explosives into spirants. Such a complete convergence within a relatively little broken-up area, even in historic times, would indeed border on the incredible. At least the germs of the later developments must go back to a period of unbroken continuity of the satəm groups.

Before I proceed farther, I wish to recall to the reader's mind the absurdity of such old saws as "The Indogermanic languages have all developed from a single undivided Indogermanic mother language," or, "The Indogermanic speaking peoples are all branches of a single Indogermanic mother tribe." Neither of these statements is true. One can say—indeed, one must say—however, that the various Indogermanic languages have developed from a mixture of the dialects, which have not come down to us, of Indogermanic conquerors with the different substratum-languages of the conquered peoples. Also one can, and must, say that the Indogermanic speaking peoples are descended principally from Indogermanized non-Indogermans, who spoke other languages, possessed other cultures, lived under other laws, and worshipped other gods and spirits than the Indogerman conquerors. But the conquerors must at one time have spoken not too divergent idioms, for otherwise it would not be possible to reduce countless words and forms of the Indogermanic languages by regular laws to identical prototypes, or, to be more reserved in our expression, to fit them into cogent comparison-formulæ. Whether a complete unity of language ever preceded the dialectic heterogeneity of the Indogermanic conquerors I prefer not

¹ This article is based on the author's more extensive papers *Oer-Indogermaansch en Oer-Indogermanen* (translated by Dr Raymond Kennedy), and *Eskimo en Oer-Indogermaansch*, which both appeared in *Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeling Letterkunde*, Vol. 77, Series A, Nos. 4 and 6, Amsterdam, 1935. Cf. also *Opmerkingen over het Eskimo-probleem* (Jaarboek der Koninklijke Akademie van Wetenschappen, Amsterdam, 1936).

to say, though I have my own opinion in the matter. By "Indogermanic mother language," therefore, I mean the complex of dialects of the Indogermanic conquerors as it existed in the period of their most intimate association, however this intimate interrelationship may have been brought about. The speakers of the thus closely circumscribed "Indogermanic mother language" I call the "Indogermanic mother tribes complex." In order to avoid all misunderstanding I add to my previous remarks the following. Since the subduement—partly or wholly, permanently or temporarily—of Indogermanized regions occurred in widely separated periods, we cannot consider the "Indogermanic mother language" exactly the same language as that of the Indogerman conquerors. Most of the conquests must have taken place at a time when the language of the conquerors was already more differentiated than it was in the above defined "Indogermanic period of unity." Among other factors, the sharp line of division between centum and satəm languages points to the justice of this assumption.

However much stress we lay upon the division into centum and satəm languages, there are still other considerations which open for us still deeper perspectives, still wider vistas. Through the whole of the Indogermanic mother language, cutting across both centum and satəm, there runs another, older, much more sharply defined line of division, which proves that the Indogermanic mother language itself was already a mixed language. This line of demarcation, however, is entirely different from other lines drawn by Feist and Pokorný.

In my opinion the Indogermanic mother language was obviously composed of at least two mutually unrelated, or at the most only very distantly related, complexes of elements, which I shall refer to as A and B. A includes, in addition to the pronouns (or at least the majority of pronouns) the verbal roots which are so characteristic of Indogermanic, with their innumerable derivatives. To B belong, in general, the isolated words which cannot be linked to verbal roots, such as the numerals (or nearly all numerals), a series of relationship terms, many names of anatomical parts, of animals and of trees, and numerous others of the most generally used words of all kinds. The criterion employed in distinguishing A-words from B-words is, therefore, whether or not a given word belongs to a family of words which stand in obvious etymological relation to each other, joined together by and grouping themselves around a verbal root. At the same time, one must not overlook the possibility that some words which appear to us as isolated at one time might have had large groups of related words associated with them. In such a case the type of the word must be the deciding factor.

If its formation is analogous with that of words whose root and kinship relations we can demonstrate, this is sufficient reason for us to regard it as an A-word. If, on the other hand, it cannot be paralleled with other unrelated words which are derived from a verbal root by means of known suffixes, then we may place it in the category of B-words. To express our meaning more concisely: our criterion is the distinction between actual or potential group affiliation and absolute isolation. I am fully aware of the fact, however, that in many cases there is ample room for doubt. Nevertheless, the uncertain instances prove to be not so numerous, if examined carefully, as one would think offhand. It must be kept in mind that I am concerned exclusively with words of the Indogermanic mother language, that is, with words which occur in more than one Indogermanic sub-group and which exhibit the phonetic structure naturally to be expected in the language or languages involved. Etymologically undefinable words which are confined to continuous areas of distribution within the sphere of Indogermanic I do not include in the B-words. There is a much greater probability that in such instances we are dealing with substratum words, though there is always a very small chance that a particular B-word which has disappeared entirely elsewhere has survived in a narrowly restricted territory, an isolated corner or enclave, and can no longer be distinguished from a substratum word. To be sure this chance is very slight, since the B-words are among those most in general use in everyday life and do not easily yield ground to new terms.

The grammatical system of the Indogermanic mother language belongs partly to A, partly to B. Probably the language which forms the basis of A was relatively more lucid and simple in structure, monoschematic and regular, in the manner of Uralic and Altaic. The polyschematism (Ernst Lewy calls it "form variation"), the heteroclitic declension type, the unpredictable irregularity, which were characteristic of the Indogermanic mother language, must then have come from the language which underlies B. In the New as well as in the Old World polyschematic and irregular languages are encountered, but the Indogermanic mother language is such a peculiar complex of apparently irrational declensional caprices as is very seldom found outside our linguistic area, at least in the Old World. In America however, we need not seek far for "irrational" polyschematism and perplexing irregularity. That polyschematism and irregularity at one time were taken for specifically Indogermanic peculiarities may be explained partly by the contrast presented by Uralic, Altaic, and a few other linguistic stocks, and partly - and this is especially important - by the fact that some Indogermanists have neither the desire nor the courage

to verify impressions which they have gathered from their own field of research by comparisons with the results of investigation in other, adjacent fields.

From the B-language must also have been derived the classification of nouns in the Indogermanic mother language, although it was to all appearances strongly altered and simplified. There are all kinds of signs which point to the validity of the assumption that a more complicated scheme must have preceded the three-class system. We need not now go into detail regarding these indications, especially since all information concerning the gender of Indogermanic can be found in Royen's standard work, even though he has not arranged his material conveniently.² Only to one point do I wish to call attention in passing. I cannot agree with Meillet when he states that the apparently archaic feminine is a relatively late development. That the Indogermanic feminine in its original phase, inaccessible to us now, was actually the grammatical expression of the female sex as such is anything but certain, and careful consideration of the data known to us demonstrates that such an assumption is not very likely to be valid. The fact that, judging from the evidence of gender-motion and myth, the feminine was already often associated with the female sex in the Indogermanic mother language allows no conclusion to be drawn regarding its original value. The remarks of Meillet concerning the sexual division of labor in the period of the Indogermanic unity have no worth for the gender problem, since his statements apply equally well to the Uralic and Altaic speaking peoples, who make no grammatical distinction between "masculine" and "feminine." Nieuwenhuis seeks the origin of grammatical differentiation of the sexes in primeval sexual-totemistic conceptions. He is unable to prove that a sexual gender has ever developed on this foundation, but he assembles coincidences from ethnographic and linguistic sources which might possibly be viewed as pointing to this interpretation. He does not fully realize the wide range of the knotty problems of classification which obtrude themselves in always varying aspects in every linguistic area. That which applies to one language or group of languages need not always be valid for another. The fact that, for example, the Semitic two-class system is not necessarily to be regarded as having developed along the same lines as the Indogermanic distribution between masculine and feminine, here with the neuter as a third class, is self-evident to any historically minded scholar. Nevertheless, Nieuwenhuis' basic idea certainly cannot be disavowed on a priori grounds. It deserves to be taken into serious consideration in all future investigations of the problem of nominal classification.

² Cf. my critique in *Anthropos*, Vol. 25, pp. 649-56, 1930.

cation in Indogermanic and elsewhere, and this not so much because of its originality as because of the possibilities which it opens up.

If our conjectures regarding the character of the language from which the B-complex arose have any validity, the first question we must ask is this: Are languages of the complicated type involved actually known—either from an earlier or a later period—which stand in evident genetic relation to the assumed prototype of the B-complex? The word “Caucasian” comes involuntarily to our lips, as we think of a casual remark of Prince Troubetzkoy. The study of Caucasian has made immense progress in recent years. Not to mention such specialists as Jakovlev, I shall name only René Lafon and Georges Dumézil, through whose comparative studies the excellent work of Adolf Dirr, which we read with great edification in 1928, has already been superseded in many respects. Nevertheless, I hesitate to employ Caucasian in attempting to solve the problems of the B-complex; much less am I prepared at the moment to formulate an hypothesis regarding the matter. There are, beside, still other languages—dead for centuries and known to us only fragmentarily—which might possibly be related to the B-complex, or which could perhaps better serve the purpose of explaining the B-complex than Caucasian. Or can it be that we must still seek a solution elsewhere?

Though I must, for the present moment, disappoint the reader as to the origin of the B-complex, I think I can throw some light upon the other, perhaps still more important group of elements, which I have designated as the A-complex. First let me call attention to a matter which will prove to be of considerable importance in the prosecution of our investigation. Surely the reader has not failed to observe that the *centum-satəm* distinction appears to have no direct correlation with the A-B-division. The *centum* and *satəm* languages share equally in both the A- and B-complexes, which together make up the Indogermanic mother language. Notwithstanding this fact, the *centum-satəm* division may well bear some relation to the A-B grouping, insofar as there may have been a stronger contingent of A-Indogermans among the speakers of the *centum* languages and a greater proportion of B-Indogermans among the peoples possessing *satəm* languages. On theoretical grounds one might as well assume the exact reverse of this statement to be valid, so that *centum* would be more closely related to B and *satəm* more intimately bound up with A, but in the ensuing remarks it will become evident that our first assumption has the greater probability of being true. As a matter of fact, it is possible to demonstrate that between A and *centum* there exists an anthropological affinity, which is lacking between A and *satəm* or between B and *centum*.

If we now attempt to find language, related to A-Indogermanic outside our linguistic stock, we have not far to seek. Everything which has thus far been brought forward as indicating a so-called "primeval relationship" between Indogermanic and Uralic fits easily into the A-complex. Now, for the first time, the many and striking points of similarity between Indogermanic and Uralic which have been noted by Anderson and Wiklund, Paasonen and Collinder—not to mention others—attain their full historical implications. None of these famed scholars seems to have fully realized the obvious possibility that the Indogermanic mother language might have been a mixed language having Uralic as one of its components. I shall not take up the question of the degree of relationship between Uralic and Altaic. If the oft-supposed "Ural-Altaic linguistic stock" may indeed be regarded as having constituted a more or less well-defined unit—an assumption which is by no means self-evident, even if we accept an historical relationship between Uralic and Altaic—the Indogermanic mother language still would not be properly regarded as a true Ural-Altaic tongue, but rather as an easily recognizable mixture of "Western Ural-Altaic," as Uralic might then be called, and an entirely different language which cannot yet be clearly identified.

Not entering into the discussion of some other possible distant connections of the Indogermanic mother language, or rather of its A-complex, with certain languages of the Far Northeast, suggested by Koppelman and Güntert, I shall pass on to the consideration of an allied problem, which seems to promise more immediate results. About thirty years ago I began to reflect upon the remarkable fact that the Eskimo languages, which are spoken from the Asiatic side of Behring Straits to the east coast of Greenland, reveal intimate lexical relations to Indogermanic. I did not know at that time what to do with my findings, for in those days I was still too much involved with the old neo-grammatical idea that "primeval relationship" and "borrowing" were entirely different concepts, and I had not yet realized to what degree mixing and blending prevailed in the history of languages, as indeed they do in the history of peoples and races. Partly under the influence of Hugo Schuchardt and Franz Boas, and partly as a result of my own independent cogitations, I arrived at a more realistic view of linguistic interrelations. If at that time I did not give much thought to my comparison of Eskimo and Indogermanic words, this was principally because I was then too much occupied with and impressed by what appeared to me far more striking points of relationship between Eskimo and Uralic. My Eskimo-Indogermanic observations were merely a by-product of other investigations. In 1905 my article *Uralische Anklänge in den*

Eskimosprachen appeared, and was followed in 1906 and 1907 with supplementary notations, in which were included some lexical comparisons between Eskimo and Indogermanic. I realized fully that grammatical similarity between Eskimo and Uralic had been observed before that time, but I did not learn until much later, from Thalbitzer, that the illustrious Rasmus Rask was among my predecessors in the field, while Rink also had not missed certain important points of similarity. The Eskimo-Uralic problem was treated at large by Aurélien Sauvageot in 1924, but I do not believe that he contributed much that was new to the difficult question. The phonological correspondences which he adduced were not convincing, and, as a consequence, his lexical comparisons were unfortunately chosen. William Thalbitzer aligned himself against Sauvageot in 1926, at the Congress of Americanists in Rome, with a paper entitled *Is There Any Connection Between the Eskimo Language and the Uralian?* Thalbitzer is right in regarding as unproven a "primeval relationship" between Eskimo and Uralian. Nevertheless, the correspondences between the two linguistic families are too marked to be attributed to convergence. At any rate, they point to a very ancient cultural connection between the Eskimo and the Uralians. Probably Thalbitzer's stand can best be explained by the fact that he lays too much stress upon the concept of "primeval relationship." The great linguistic stocks of the world must have evolved by a process of constant mixing, blending, and splitting.

However we may explain the correspondences between Uralic and Eskimo, and between Eskimo and Indogermanic, we cannot close our eyes to the fact that the prehistoric relations of Indogermanic reach far—very far—to the east, farther than we could ever have suspected some decennia ago.

The Indogermanic speaking conquerors must have approached Europe from the east. They came by way of South Russia from the Aral-Caspian steppes, where they had at their disposal the wide spaces which they needed for their horses and war-chariots, their great herds, and their seminomadic mode of agriculture. Although we are even yet sometimes inclined from the evidence of prehistoric, post-Megalithic, ancient Indogermanic centers of culture in Central Europe to claim the starting-point of this culture for our continent, we can scarcely any longer doubt its Asiatic steppe origin, since Wilhelm Koppers has made this so very probable in his brilliant article *Die Religion der Indogermanen in ihren kultur-historischen Beziehungen*. He has overwhelmingly convinced us of the close bonds which link the ancient Indogermanic heaven god—the broad-handed god of the Vedic songs, whom Guntert has recognized in the Hallristningar of Sweden—with the

heaven god of central Asiatic and Siberian herding peoples. Karl Ferdinand Johansson and Elias Wessén had already demonstrated to us what an intimate significance the horse-sacrifice must have had at one time for the entire ancient Indogermanic world—a significance which would almost justify our accepting the *açvamedha* as the symbol par excellence of ancient Indogermanic culture. Again, Koppers' painstaking comparison of the peculiarities of the *açvamedha* ritual, exhaustively described by Dumont, with the details of the horse offering of the Altaic nomads showed the full importance of this ritual in the problem of Indogermanic diffusion. And where we find horse and horse-sacrifice with ancient peoples of higher civilization before the appearance of Indogermans, there we have always to do with early influences of the same culture-sphere to which the Indogermanic mother tribes complex belonged. Hermann Güntert too seeks the point of departure of the Indogermanic conquerors, just as earlier scholars with less convincing evidence had done, in the steppes of Asia.

Anthropological data also support this theory. As Ariëns Kappers has discovered, the centum-satəm division is linked with an important difference in skull form. In general, the ancient bearers of the centum languages were mesocephalic; those of the satəm languages, dolichocephalic. The peaks of the various index frequency curves are indeed very significant, but these peaks, which at one time distinguished the centum from the satəm group and which even today can be taken, to a certain extent at least, as characteristic for them, find correspondences, I understand from Ariëns Kappers, much farther to the east. The West Eskimo reveal a mesocephaly similar to that of the centum-speakers, while the Greenland Eskimo are dolichocephalic like the satəm-speakers. Among some of the Uralian peoples a mesocephalic strain, reminiscent of the West Eskimo on the one hand and of the centum Indogermans on the other, is still apparent. This is what I was alluding to above when I spoke of an anthropological affinity between A and centum. In this connection I wish to emphasize the fact that the satəm tendency to transform palatal explosives into spirants is lacking in Uralian, and cannot, therefore, be derived from tendencies of the A-complex. In its retention of the explosive character of the palatals, centum reveals a special relationship with the originally Uralian A-complex. We should welcome the discovery of other linguistic affinities between A and centum, but for the time being the quest of such connections would involve very great, perhaps insuperable difficulties. It is obstacles like these, however, which challenge us to further research. Who can say but that some day I may be able, after sifting assumed or actual, but for the most favorable cases only secondary, acculturation phenomena in

Fenno-Ugrian and satəm, to lay before you a few striking points of similarity between the A-complex and the centum languages which can stand the test of criticism? At present, however, I stand far short of this goal. The question is of minor importance for what I wished to bring to your attention here. Whatever may be the relation of A and B to centum and satəm, the Uralian character of the A-complex and the mesocephaly of the centum-speakers stands unchallenged.

Since Paudler has fully demonstrated the thoroughgoing differences between the Dalian (or Falian) and Nordic types, there is no longer any justification for seeking the point of departure of the Indogermanic conquerors on German or Scandinavian soil. The mesocephalic Nordics are the centum speaking conquerors, the Dalian types represent the subjugated peasant population of the conquered Central and North European lands. Egon von Eickstedt considers the Dalians or Falians, which he designates as Dalo-Nordics, as a sub-race of what he calls the Nordic race. For the prehistoric problem, with which we are here concerned, this purely physical anthropological question is unimportant. What is important to us is the knowledge that the Dalians had been living in Central and North Europe for countless centuries, when the Nordic or, if you wish, the Teuto-Nordic, semi-nomads arrived upon the scene.

LUGANO-RUVIGLIANA, TICINO
SWITZERLAND

THE CHARACTERISTICS AND FUNCTION OF CLAN IN A SOUTHERN ATHAPASCAN CULTURE

By GRENVILLE GOODWIN

THE Southern Athapaskan group treated here is the White Mountain Apache, one of five groups or tribes of the Western Apache. The other four are Cibecue, San Carlos, Southern Tonto, and Northern Tonto.¹ The Western Apache apparently are the only Southern Athapascans, except the Navajo, who have true clans.² It should be of interest to see what part the clan played in the culture of a people who did not lead the sedentary life of the Pueblos, but relied largely on game and wild plant foods to supplement agricultural products.

The ten White Mountain clans are named: 'iyà'áiy.' (a species of plant), "Hills Running Down to the River in a Line People," "Rock Jutting Into the River People," "White Water People," "Black Water People," "Long People" (because they ate from a tall tree), "Narrow Peak Jutting Upwards People," "Adobe Bank on Wash People," "Notch Between Hills People," and "Cottonwoods Meeting from Two Lines People." All names except the first refer to traditional places of clan origin. The clans are said always, as now, to have varied in size.

The clans are grouped into three phratries and one unrelated clan, with interrelationship among the clans of each phratry. The clans of the other Western Apache groups are different in name but are grouped into similar phratries, and bear relationship to White Mountain clans. When a captive was taken he became a member of the clan of his captor or that of the person to whom he was given. A Navajo captive, however, retained his native clan identity if such clan were present in the Apache group.

At present all ten clans are represented in both the Eastern and Western White Mountain bands, but it is felt that a clan properly belongs to the band in whose territory it traditionally originated.

CLAN AND KINSHIP

It is believed that all members of a clan are related by blood, whether traceable or not. Origin from a single woman, however, is not suggested. Actually the individual readily distinguishes between his clanmates related to him by blood and those not so related. The recognized blood kin com-

¹ The material on which this paper is based was obtained during twenty-two months of field work in 1932, 1935, and 1936 among White Mountain Apache on the San Carlos and Fort Apache Reservations in Arizona, under the auspices of the University of Arizona.

² Dr M. E. Opler, who is familiar with Chiricahua and Warm Springs Apache, Mescalero, Lipan, and Jicarilla Apache, tells me none of them have clans.

prise only true siblings, mother, and those with whom blood connection can be traced through the intermediacy of females of the clan irrespective of generation. In explanation of relationships between clans it is commonly said that all are offshoots from one root or parent clan. An individual is spoken of as being the clan of his mother, but is said to be the child of his father's clan.³

Kinship terms⁴ denoting age and generation can be applied to and reciprocated by any member of one's phratry. Individuals of one's own generation are considered siblings, those of the first ascending generation are siblings of one's mother, of the second ascending generation are maternal grandparents, and those of the third great grandparents. In the first descending generation, individuals are classed as children of one's sister, those of the second as grandchildren, and of the third as great grandchildren. The kinship terms employed are those proper in reckoning actual blood kinship to ego through a mother when the individual is of the speaker's generation or of one descending; through ego's mother when of an ascending generation. The use of parent-child terms between ego and the first ascending or descending generations, except between actual parent and child is unknown.

Reciprocal kinship terms can similarly be extended to any member of the speaker's father's clan or phratry. For one's own generation or those descending, the terms are identical with the terms employed for blood kindred related to ego through their fathers, and for ascending generations, those for kindred related through ego's father. Parent-child terms can be used in the extended sense only where a joking relationship exists (between ego, the child, and paternal clan kin, the parents), and usually between proximate generations. This is discontinued out of respect when ego's father dies. It is improper to joke with close blood kin of one's father (except his parents, with whom a mild joking relationship may exist), and thus parent-child terms are not used with them.

Two individuals whose fathers are of the same clan or phratry use reciprocal sibling terms, the fathers being siblings by extension. Such sibling terms may supersede any usage based on some other recognized kin bond between the fathers.

³ Compare A. L. Kroeber, *Zuni Kin and Clan* (Anthropological Papers, American Museum of Natural History, Vol. 18, Part 2, 1917), p. 48.

⁴ See M. E. Opler, *The Kinship Systems of the Southern Athabascan-Speaking Tribes*, *American Anthropologist*, Vol. 38, pp. 620-33, 1936) for a kinship chart of the White Mountain Apache. The extension of sibling terms to cross-cousins there is an error, apparently due to my forgetting to correct faulty information given Dr Opler some years ago.

Relationship may be established through the speaker's parents with persons of any generation by clan alone, except when two individual's fathers are of the same clan or phratry. In the latter case the extension is permissible only to the descending generations. When two persons can derive no kin term from common forebears, one may be selected according to their personal contacts or respective generations.

Cross-cousin terms are extended to the majority of members of the speaker's father's clan and phratry who are not closely related by blood, and this irrespective of their generation or sex. In fact, where there are generation differences, cross-cousin terms are used in preference to those terms denoting difference in generation mentioned above. The use of cross-cousin terms implies one of two types of behavior, joking or non-joking, the latter attitude resembling behavior to a member of one's own clan. The joking relationship does not commonly occur between actual cross-cousins. Hence it may seem obscure that a joking relationship prevails between cross-cousins in the extended sense just mentioned, namely between a person and many members of his father's clan or phratry. But joking between such individuals is most common, as a rule, when they are of much the same age, as real cross-cousins would be. Joking in milder vein may also be established with the mate of such a distantly related individual with whom one uses cross-cousin terms. Another mild joking relationship may be established between a person and any distantly related member of his or her mate's clan or phratry, without regard to generation, but provided that no respect relationship exists between them. This occurs principally with mates of clansmen addressed by sibling terms, and may be the result of the common custom of marrying into the father's clan, with the members of which there is commonly a prior joking relationship.

When an individual marries into his paternal phratry its members may continue to address him by kin terms in use before marriage, or terms suitable to a relative by marriage may be used, in each instance the appropriate behavior goes with the term. The choice depends on the extent of previous familiarity. A choice is again involved in addressing the child of a couple, one of whom is of the speaker's clan, the other related to the speaker by clan but not of the same phratry. The speaker commonly uses a term derived through the former, a fellow clansman. If, however, the other parent is more closely related by blood or his father is of the same phratry as the father of the speaker, the term is derived through that parent. In this case one sometimes addresses the parent who is fellow clansman by a term for an affinal relative rather than by a term denoting a blood or clan connection.

Much hinges on the personal relations of clan-linked individuals in

their choice of terms and the corresponding types of appropriate behavior. Strangers of the same clan are not apt to use kinship terms to one another save in an emergency. Close blood relationship usually takes precedence over kinship based on clan alone, though an exception may occur when the linkage between two individuals is the common clan membership of their fathers.

Clans and phratries are exogamous; marriages within are considered incestuous and examples of witchcraft. Marriages into the father's clan are not only acceptable but preferred: compilations show a decided tendency in this direction. Widows and widowers are commonly obligated to remarry into the same family and clan; if marriage outside the family is necessary, it must yet be within the clan or phratry. The surviving mate is spoken of as the *'itsà'* of the clan of the deceased, e.g., "the *'itsà'* of White Water People."

Relationship by marriage to the whole clan of one's spouse is exemplified by the use of affinal terms with all members of the clan who so desire. However, those terms implying greatest respect are usually reserved for use between a person and the immediate blood kin of his or her mate. A term denoting kinship by affinity with the whole clan is commonly used for a man. A special term designates groups of persons of either sex who have in common marriage into the same clan or phratry. Members of such a group use reciprocal sibling terms and in some cases behave accordingly.

CLAN TOTEMS AND CLAN NAMES

Each phratry, and the isolated clan, has associated with it an animal or bird. These are respectively the hawk family (particularly the eagle), deer, road-runner, and bear. A phratry member speaks of the associated animal as "my relative" or by some kinship term, thus indicating the conceptual nature of the tie. Needless molestation of a phratry animal is rebuked, though reference to this often lies at the basis of ridicule. Reference to a phratry is not uncommonly in terms of the animal "relative" but this is usually a joking allusion.

In addition to these distinctly phratry animals, there are others associated with one or more specific clan within the phratry. Certain clans within the phratry affiliated with hawks are related to the king bird and mocking bird, one has raven as "relative," and another is related to supernatural beings called *gá'n*. At times the latter are even alluded to as "*Gá'n* People."⁵ Likewise certain plants are related to the clans.

⁵ *Gá'n* are a class of supernatural beings represented in a curing rite by masked dancers. They correspond to the Pueblo *katchina*.

A further association is with corn, of which seven differently colored varieties are recognized. Each variety is identified with certain clans or phratries, but the specific linkages vary with locality.

An exclusive design is possessed by one clan only. This is a black rim painted about the shoulder of basketry water bottles. A member of this clan was privileged to destroy vessels on which this marking was illegitimately used. The painting of clan designs on the body during the Victory Dance was not here practised, in contrast to other Western Apache groups.

The phratry affiliated with hawks had the exclusive right to make and wear a certain type of buckskin cap. Some male members of this phratry wore two eagle feathers in their head bands, while men of the phratry related to road runner occasionally used feathers of that bird in the same way. Similarly, members of the phratry kin to bear sometimes wore bear claws.

Explanations of the origin and nature of clan and phratry associations with animal "relatives" are highly fragmentary. Deer is related to "Long People" by a mythical deer charm bestowed upon this clan through the *gáin*; "Hills Running Down to the Water in a Line People" are related to the *gáin* because the two lived in close proximity; and the 'íyà'áíy.' have the plant of that name as "relative" because it grew profusely where the clan originated. Explanations are lacking for other clans.

An individual is often named for a deceased relative of the same clan. By this act it is hoped that the person will acquire some of the attributes of his predecessor, such as good luck and health, or power and greatness. Occasionally a boy refused such a name, being unwilling to assume responsibility for maintaining its traditional glory and achieving the position expected of its bearer. One's clan might take away a name if its recipient proved unworthy to bear it. Such names are kept strictly within the clan, and largely within the lineage. Before one lineage may bestow a name held by an unrelated or distantly connected lineage within the clan, it is necessary to obtain the latter's permission. Payment often is made for such privilege. Names embody no allusion to clan totems.

CLAN LEGENDS AND RELATIONS TO SURROUNDING TRIBES

The brief clan legends merely relate how the people long ago settled at various places to farm and assumed the names of these localities. Previous abodes are not mentioned, in contrast to the tales of other Western Apache groups. Non-clan myths, however, mention a place somewhere north or northeast of the present territory as the ancient home.

The mythical names of two phratries, not now used, are said by the

Apache to be those of current clans among Navajo and Zuñi. A third phratry is closely related to a clan now present among the Navajo as well as other Western Apache peoples. These relationships were recognized and used to advantage by both Navajo and Apache. The confident belief that related clans existed also among the Zuñi affected relations with that people. Similar ties are recognized with those Yavapai possessing clans but not with the peoples of northwest Mexico, Pima, Papago, or other Yuman tribes.

CLAN IN RELIGION AND CEREMONY

Clan is a factor in religion and ceremony, but a relatively unimportant one. Since ceremonies are common property, a member of any clan may participate. The Bear Ceremony, however, is said to have been bestowed on a member of the phratry related to bear because of his phratry affiliation. Bear shamans of this phratry may emphasize the relationship. Laymen of the phratry are immune from attacks of the animal because it is a "relative."

Because of the kinship with eagle, a member of the phratry related to hawks was asked to officiate, if possible, in plucking eagles. This does not imply a connection between the phratry and a hawk ceremony: no such tie exists. Similarly, there is no ceremonial connection between deer and the rite for successful hunting attributed to the clan related to deer. The mythical deer charm received from the *gá'n* is thought to be solely responsible for this ability. At least one member of the clan related to the *gá'n* claimed great physical endurance from his connection with these tireless supernaturals. Men of the same clan are believed born with a power which protects against enemies and their weapons. These instances exhaust the totemic linkage of clan and ceremony.

Only in the *Gá'n* Curing Rite was special function ever allotted to various clans or phratries. Certain masks and body paintings, by which the dancers were distinguished, were prerogatives. The particular allocations vary with locality. In some places the position of dancers in line was clan determined.

The clan element in this ceremony, though recognized, was not an unalterable constituent. It was common for those initiating the ceremony to ask the best *gá'n* dancer of a particular local clan for permission to make the mask of his clan in the hope that he would wear it. However, if this dancer was unable to dance, and no other good dancer of his clan was available, a skilled dancer of any clan might wear the mask and design.

The close psychological relationship within a clan is well illustrated by a feature of the girl's puberty ceremony. The woman chosen to attend the

girl must be of an unrelated clan, for she endows her with womanly attributes other than those naturally inherited from the girl's own clan. For the same reason the man who attends the girl must be of the same clan as the woman attendant.

Finally, in ritual acting or dancing, paired participants of opposite sex must be of unrelated clans. This, however, is based purely on the general social situation and on kinship.

LOCAL GROUP, CHIEFTAINSHIP, AND CLAN

Though families might function independently, yet most of them were affiliated with larger social units, which I will style local groups. The adhesion of a family to its local group was loose, and temporary separation was not uncommon. The nucleus of a local group usually comprised members of from one to three clans. Three-fifths of the adults of these nuclear clans were women because of customary matrilineal residence (although patrilineal residence was not uncommon). Generally one of the nuclear clans was in preponderant numbers, but at times two were equally represented. The additional members of the local group were almost all men, these having married into the group and commonly being of two to four other clans. Individuals married both within and without their local groups. The nuclear clans were usually not related, permitting marriage between them.

A nuclear clan often contained but one or two matrilineal lineages, but sometimes as many as five or six. The intermarriage of the nuclear clans formed a network of affinal ties and paternal blood connections which, together with other clan and blood ties, served as the principal factor for binding the local group together.

Clan and local group are not equivalent. No clan is limited to a single local group, but instead is represented in many.

Almost every local group was associated with a particular farming site in the vicinity of which they spent much of their time. Not all families of the group, however, owned farms.

A local group commonly had one hereditary chief, a member of its dominant nuclear clan. In addition each local group had several head men or semi-chiefs, who achieved their positions not so much through heredity as through ability. They usually belonged to the nuclear clans, but sometimes a like position was held by an outsider, affiliated merely by marriage. Again, some local groups were led by such sub-chiefs in the absence of any hereditary chief.

Hereditary chieftainships followed clan and blood lines. A chief must be of the same clan as his predecessor, and was usually his brother, sister's

son, or sister's daughter's son. He might be a relative even farther removed but the blood relationship must be traceable and not too distant. No one type of blood relationship took precedence over another.

The choice of a new chief was controlled principally by the members of his local group. To be sure there is some indication that with formal hereditary chieftainships only members of the clan of the chieftainship in question actually participated, being summoned for the purpose from all local groups within the band. But this must not be taken too literally. The only distinction between these formal hereditary chiefs and other men acting as chiefs seems to have been the greater respect in which the former were held, not in any exercise of special prerogative.

Because of the mixed clan composition of a local group, its chief controlled not merely his clanmates but all residents. Though there was sometimes more than one chief of a clan in one of the two White Mountain bands, there is some evidence that but one of them held formal hereditary position, this setting him apart as the leading chief of his clan. This does not mean, however, that he directed the affairs of his whole clan.

A chief's direct control was limited to his own local group; outside of it his influence was purely personal. He exercised indirectly some control over fellow clansmen of other localities since he might appeal to them for support in times of trouble. Response was not a personal obligation but a duty to clan and kin. Thus some individuals considered themselves subject to two chiefs, namely the chief of their clan at their original home and the chief at their present residence.

CLAN AND AGRICULTURE

Clan ownership of material property appears only in connection with farm lands. Certain sites, associated with the mythical places of origin of various clans, belong to the respective clans. A site more recently established by a group of members of one clan is likewise considered the property of that clan. Formerly most of the farm lands at a given site were held by a single clan, the dominant nuclear clan of the local group associated with that site, though land was often held in lesser amount by one or two other clans.

Most locations were controlled by one, sometimes two local groups. Predominating matrilineal residence perpetuated control of the bulk of real property at particular places by the dominant clans. Some other sites were farmed only by a part of a local group, one clan being in the majority. Commonly a place is considered as belonging to a certain clan for all of these reasons. Members of unrelated clans and those not of blood kin were

ordinarily not able to own land in such a place. They might, however, obtain a parcel of ground for temporary use through permission of the owner and chief of the dominant clan. But control of particular areas was effectively maintained by the dominant clan through the feeling against acceptance of outsiders and preference for intra-clan associations.

The individual farm is held by its owner and his immediate family and may not be redistributed by clan or chief. The chief was consulted only when one desired to lend a farm to a person unrelated by blood, clan, or marriage. Even this was largely a formality; endorsement was assured in most cases.

MUTUAL RESPONSIBILITY OF CLAN MEMBERS

The feeling of kinship solidarity within a clan is indicated by the gambling custom which necessitates that opponents be of unrelated clans in games of high stakes involving but two players. Opposition between two close relatives is socially unacceptable, but this does not imply clan ownership of property. The same feeling extends to relatives by blood and marriage, if closely akin. These considerations hold only when two players are concerned; in games with several participants the personal aspect is lost.

The clan played an important role in cases of murder, rape, and injury, whether accidental or intentional. Unless settlement was reached through payment, a blood feud resulted between the clans involved. Material payment varied with degree of harm and the social position of the injured person. Settlement was attempted by tactful members of the offender's clan. Sometimes the mother of the offender was sent; other members of the clan, not necessarily blood relatives, accompanying the emissary. After satisfactory payment was made, speakers emphasized that no further bad feeling should exist between the clans.

The injured individual or close blood kin in his clan might in some cases seek revenge by destroying property of the offender or even of a member of the latter's clan if closely related by blood. In atonement payments, those concerned usually were closely related to the defendant by blood as well as by clan affiliation. Blood kinsmen of other clans were not necessarily involved, though they might give aid.

Blood feud was imminent if no payment was arranged for a serious offense. Any member of the offender's clan of the same sex was in danger, for killing one was considered equivalent to killing the other. Close blood kin, however, were most apt to be singled out. Again, blood kin in other clans and relatives by marriage were not involved. This position was usually accepted by the clan without a thought, though close clan kin might

ward off vengeance by turning the fugitive over to the injured clan for execution. This was done only if the offense was considered inexcusable; the strength of kin feeling made such action rare. In feuds the clans, not the families, were spoken of as enemies, indicating the degree to which each entire clan was involved.

A person killed in war was avenged primarily by his clansmen, though close relatives by blood or marriage or even members of related clans might participate. A War Dance was instigated by the chief of the local group of the deceased or of the one in which the closest surviving kin lived. If the closest kin resided in a local group whose chief was not of their clan, they either asked for the support of that chief and his local group or called on some chief of their own clan. If unable to obtain ample support from either source, they approached any influential relative by blood or marriage.

A distinctive term was applied to the local group giving the War Dance and to all members of the victim's clan. Messengers were sent to invite chiefs of other local groups to participate. Chiefs of the same clan as the victim or of related clans were commonly approached first, but all with strong followings were invited, regardless of clan.

At the War Dance each chief was called out in turn to perform with his men. The participants were mainly of the same clan as the chief, but some might be merely fellow residents of other clans.

Near the close of the ceremony, twelve of the bravest men were selected to dance. If possible, these were from different clans so that all might be represented.

In the Victory Dance, given on the return of a successful war party, certain old women danced so that they might receive some of the booty. Their clan affiliation was a matter of indifference. They dance also in gratitude for the vengeance secured for all their kin who had ever been slain by the enemy, but fellow clansmen were uppermost in their minds.

The participation of the clan in warfare for vengeance did not involve the exclusion of other individuals or units unrelated to the clan. The clan was merely the largest element in such an enterprise. War parties drew on as wide a field of support as possible, but greatest reliance could be placed on clansmen.

CLAN AND DEATH CUSTOMS

Clan plays a minor role in the observations at death. When a person is expected to die, he is attended by members of his clan and even of related clans, especially women. This is both an expression of sympathy and the fulfillment of a duty. A bereaved family invites all who will to join in a

wake of one night's duration. Though guests include mere friends, relatives on the father's side, and the like, yet clan representation is notable, including not only those closely related by blood but others as well. In a few other death customs the clan is involved, but in these blood kinship is of equal importance.

COMPARATIVE SUMMARY

One cannot fail to be impressed by the many parallels in clan with the Hopi, Zuñi, and Navajo.⁶ Though what we discuss below are for the most part characteristics of clans rather than functions, this should not be taken to imply that an equal number of functional parallels may not also be cited.

None of the White Mountain Apache clans bear the same names as those of the Navajo, yet duplication is found between the Navajo and other Western Apache groups. Also, nine of the ten White Mountain clans claim to be directly descended from or related to certain Navajo clans. Though the names of Hopi and Zuñi clans are wholly unlike those of the White Mountain Apache, they do show a striking resemblance to the names of the plants, birds, and animals associated with the clans of the latter. Hopi or Zuñi clans, sub-clans, and mythical clans bear the following names: eagle, hawk, katchina, raven, deer, road-runner, bear, corn. All of these figure as "relatives" of individual White Mountain clans or phratries (save that corn is "relative" to all).

Some of the Zuñi clans are divided into sub-clans; the Hopi have phratries comparable to those of the White Mountain people. Among both Hopi and Zuñi the katchina and raven or crow clans or sub-clans are related, paralleling the White Mountain grouping of clans with these "relatives." The same holds for eagle and hawk clans of the Hopi and these White Mountain Apache clan "relatives," though this may have a zoological basis. Moreover, the "relatives" associated with White Mountain phratries are not found as names of unrelated clans or sub-clans at Zuñi or Hopi, except in the case of katchina (gá'n) and hawk. Many other Zuñi and Hopi clan names fail to find parallels in White Mountain "relatives," but it is significant that eagle, deer, road runner, and bear are all well established clans at one or the other of these pueblos.

Among the Hopi, according to Lowie, in some cases clan members are not supposed to molest unnecessarily the animal, etc., whose name the clan

⁶ Contact with Zuñi was maintained by trading parties, frequent during the nineteenth century and probably long before. Similar trade existed with the Navajo during the intervals between frequent hostile relations. Other Western Apache groups had contact with the Hopi, though the White Mountain Apache did not in recent times.

ers, though it may be killed in hunting. Lowie also mentions non-hunters tossing coyotes into the air to mock members of the Coyote clan.⁷ This is strongly reminiscent of similar practise among the White Mountain Apache.

Animal "relatives" for clans and animal names are closely related concepts, and the former might easily have developed into the latter. At least three Western Apache groups possess even more numerous "relatives" paralleling clan names among Hopi and Zuñi. Are any such similarities to be found masked under the clan names of the Navajo?

Though unimpressive as compared with Hopi and Navajo clan mythology, White Mountain clan legends contain similar ideas of migration and settlement at particular sites. The same variation in size of clans is also found. Similar localization of clans characterizes the Navajo but apparently not Hopi or Zuñi. The opportunity for clans to remain localized may have been greater among Apache and Navajo, due to their different economic and social system, and might be said to indicate a simpler phase of the clan. But White Mountain clans themselves were widely scattered, each being represented to at least some extent in almost every local group. The major representation in the local group of two clans on occasion is reminiscent of the dual clan character of the Hopi village of Shipaulovi.⁸ Moreover, an approach toward village association of clans as among Zuñi and Hopi is found where members of more than one local group lived in close proximity, using the same or adjacent farming sites. But such close association was temporary, lasting only through planting and harvesting and part of the winter. During the remainder of the year they were separated by other economic pursuits. The resemblance to the Western Pueblos would be considerable if agriculture had supplied the greater part of the food and people remained at the farms all year.

Perhaps the greatest difference from Zuñi and Hopi lies in the total lack of clan fetishes among the White Mountain Apache. The only approach to such a concept is the mythical deer charm mentioned above. The tearing of parts of the animal or bird related to the clan is a dubious counterpart of the fetish.

Association of clan with ceremony through fetishes or ceremonial office is lacking. But the connection of one clan with the Bear Ceremony and the power of protection from the enemy said to be possessed by another

⁷ Robert H. Lowie, *Notes on Hopi Clans* (Anthropological Papers, American Museum of Natural History, Vol. 30, Part 6, 1929), pp. 337-38.

⁸ *Ibid.*, p. 324.

are reminiscent of this.⁹ The religious fraternity or society as found at Hopi or Zuñi does not exist among the White Mountain Apache, where indeed the participants in ceremonies are members of any clan.

Though true clan prerogatives in ceremonial function, as known at Hopi and Zuñi, are absent, yet certain similarities appear. The clan related to deer had special luck in hunting that animal: members of the clans related to eagle were asked to pluck feathers of this bird, and an old man attributed his great endurance to the gá'n, his clan "relatives." Might not these be seeds of more extended ideas and practises?

The Hopi association of various kachina and kachina masks with certain clans is not so unlike the White Mountain association of the leading dancer in the Gá'n Rite with two clans. This dancer represents a particular kind of gá'n: the association is confined to some localities. Similar clan connections with this ceremony have been mentioned above. Kroeber states that Zuñi clan enters more into communal rituals wherein masked dancers are the principal participants than in the fraternity ceremonies.¹⁰ The only White Mountain ritual in which the clan took such a part is the Gá'n Ceremony—an interesting similarity. The many likenesses between gá'n and kachina must prove beyond a doubt that they are one and the same from the wider point of view.

In a Navajo war ceremony used for curing, Reichard states, a certain participant must be related by clan, and equally, another must be unrelated to the individual for whom the ceremony is given.¹¹ This resembles the practise already cited in the White Mountain girl's puberty rite.

Though Kroeber found land owned by individuals and not by clan at Zuñi,¹² yet Hopi clans seem to have owned land and this was subject to redistribution within the clan. Neither condition typifies the White Mountain Apache, where land belongs to the individual or family, but farming sites are felt to be clan property and clan controlled.

The method by which certain Hopi and Zuñi ceremonial offices descended within the matrilineal lineage of a clan (inheritance from brother or maternal uncle, etc.) is not far removed from White Mountain inheritance of chieftanship. The latter also have within the clan matrilineal

⁹ Reichard states that certain Navajo ceremonies are said to have originated with certain clans, but that members of any clan may practise them (*Social Life of the Navajo Indians*, Columbia University Contributions to Anthropology, Vol. 7, 1928, p. 31).

¹⁰ Kroeber, *op. cit.*, p. 162

¹¹ Reichard, *op. cit.*, p. 32

¹² Kroeber, *op. cit.*, p. 178

lineages between which no blood ties are to be found, and this situation is duplicated among Zuñi, Hopi, and Navajo.

Kinship systems differ among these tribes, yet in all is found extension of kinship terms to all members of the individual's clan or related clans, and to the father's clan or related clans. The greatest difference is in the use of parent and child terms for certain persons not actually bearing this relationship. The White Mountain Apache do not share this trait, proving that at least one tribe with true clans does not make the extension. Certain archaic kinship terms given by Reichard for the Navajo are of interest in this connection.¹³ These terms sometimes appear in place of the extended use of parent-child terms, and in every case these archaic terms coincide with those used in the same position by the White Mountain Apache.

There is a surprising extent of similarity in clan between White Mountain Apache and the triad, Navajo, Hopi, and Zuñi. The equivalence of clans felt by the Apache is thus readily intelligible. In comparison with Zuñi and Hopi, clan is here so simplified as to be merely an outline, but the rudiments are still present. Parallels between White Mountain Apache culture and that of the Western Pueblos are not exhausted with clan organization.¹⁴ Both religion and economic life—the latter at least in relation to agriculture—bear Western Pueblo impressions. The connections existing between religion, agriculture, and clan in White Mountain culture indicate that clan traits borrowed from the Western Pueblos must have been taken over in association with other traits intimately associated with them in their original Pueblo setting.

The White Mountain clan is felt in three fields: it controls social life to a certain extent; it affects agriculture through clan association with farming sites, and it plays some part in religion.

TUCSON, ARIZONA

¹³ Reichard, *op cit*, pp 76-81

¹⁴ Additional comparative material from Robert H. Lowie, *Hopi Kinship* (Anthropological Papers, American Museum of Natural History, Vol. 30, Part 7, 1929); Elsie Clews Parsons, *Hopi and Zuñi Ceremonialism* (Memoirs, American Anthropological Association, No. 39, 1933); Ruth L. Bunzel, *Introduction to Zuñi Ceremonialism* (Forty-seventh Annual Report, Bureau American Ethnology, 1933, pp 467-544)

POLITICAL STRUCTURE AND STATUS AMONG THE ASSINIBOINE INDIANS

By DAVID RODNICK

THE Assiniboine, a Siouan speaking Plains tribe, link culturally with the western Dakota. In 1851, at the signing of the Fort Laramie peace treaty, the Assiniboine occupied part of the northern great plains from the Missouri River north to the South Branch of the Saskatchewan River and from the one hundredth degree west to the one hundred and tenth degree longitude.

The Assiniboine are interesting for their crystallized, yet loose, formal political structure and for the manner in which the culture permitted political change on the pre-reservation level.

The band was the political unit in Assiniboine life. It was autonomous in nature and completely sovereign. Individual affiliation within the band was loose, since it was relatively simple to form new bands, or for an individual to leave one and join another. An individual called himself a member of the band in which his parents had lived at the time of his birth. Upon marriage he could either elect to remain in his own or else join the band of his wife's people. Due to the fact that such affiliation was not too infrequently changed, the members of a band were normally related to one another.

The band was primarily territorial in character. Each band hunted over a different portion of territory; the bands coming together only at certain seasons of the year when the important ceremonials were given. At times the various units were so separated by distance that few of them could get together, say for the annual Sun-Dance. As a result it often occurred that two and even three performances of the Sun-Dance were given simultaneously by various bands in different parts of Assiniboine territory. Such an occurrence took place as late as 1882, when one Sun-Dance was held east of present day Malta, Montana, while the other was held west of Lake Bowdoin.¹

Over and above the cohesion of the individual band was a feeling of nationalism based upon relationship, either near or distant, with the members of the other bands. The term Nakota was used to include all individuals of groups felt to have had common origin in the dim past. Little contact was had by the American Assiniboine with the Stony Assiniboine of the Rocky Mountain section of Alberta, or for that matter with the so-called Swampy-Ground Assiniboine living northwest of Lake Winnipegosis in Canada, yet these groups were felt to be related and the tradition of com-

¹ Simon Firstshoot, informant

of origin was handed down despite lack of contact. As an example, when, in 1886, some Stonies from Alberta visited the Fort Belknap Assiniboine at Montana for the first time,² they were recognized by the latter as distant relatives: this on a basis of similarity in dialect and because they did not fall into the category of being affiliated with the various Dakota tribes. This level common origin for both groups was assumed and the Stonies were welcome as visiting relatives.

On the coming together of several of the bands, either at trading posts or at the annual Sun-Dance, or because scarcity of game had compelled them to hunt in territory belonging to other bands, the dances of the various societies would be held, since these did not need to be held at specified times. War parties were also organized at this time among the young men of the different bands. In almost all cases war parties were representative of one to three bands, rather than of the tribe as a whole.

Inter-band feuds of momentary duration took place occasionally. These, however, were conflicts between two large families, rather than tribal band affairs. As a rule they took place over a murder or the stealing of someone's wife. Where lasting grudges were held in these affairs, new bands were invariably formed of the conflicting partisans.

No formal political organization existed for the Assiniboine as a tribe. On coming together for the annual affairs the bands would camp in a huge circle, with each band having its specific place in this circle. Yet each band governed itself without interference during this stay. Occasionally, the various political units would form a cooperative pact, but this never proved permanent. Crazy Bear, one of the band chiefs of the American Assiniboine, was appointed tribal chief by the United States Commissioners at the treaty of Fort Laramie in 1851, yet his authority extended only over a few of the bands that were found in American territory at that time.³ The distant bands, as well as the Canadian Assiniboine, were little influenced by his authority.

Interestingly enough, the invitation to the Stonies was tendered by a group of Blood Indians who had informed the Fort Belknap Assiniboine of the presence of the Stonies the previous

Crazy Bear had gone with three other Assiniboine to the Platte River, at a time when the other band chiefs were afraid to accept the invitation, since the route along the Yellowstone River was occupied by marauding bands of Blackfoot. The American Assiniboine accepted Crazy Bear as their chief primarily because of the power of the white soldiery supposed to be behind him. Crazy Bear was quick to accept this political opportunity, and in all his speeches, he was careful to point out that he was the only individual who stood between the American Assiniboine and the destructiveness of the American soldiery. In a sense, then, the Assiniboine acceptance of Crazy Bear was a negative one.

Each band had its own chiefs, with one of the chiefs outstanding because of personality, its council, and Soldiers' society. Political decisions were made by each band individually. New bands were continually being formed under the leadership of some prominent chief and with these formations new councils and policing societies were organized on the basis of the already existent political patterns. The power of the band chief was nominal and could only be enforced through the cooperation of the band council and the warriors who formed the Agi'cita or Soldiers' society.

The size of the bands varied through the years. By assuming that eight individuals in a tipi formed the average dwelling unit, we infer that the bands in the early eighteenth century ranged from two hundred to eight hundred persons. References to their numbers are had in the journals of La Verendrye, although no names for these units are given. Thus mention is made of an encounter with a band of forty tipis in 1737,⁴ of an encampment of one hundred and two tipis in 1738,⁵ a band of sixty tipis in 1739,⁶ and a group of one hundred tipis in 1743.⁷ Whether these groups formed distinct bands can only be assumed at this late date.

Names for these units were first given by John McDonnell in 1793, who lists them as distinct "tribes." His names include three that persisted into reservation days (Canoe, Girl's, Wood Mountain),⁸ one that ceased to exist (Big Devil) and which later was named Mountain Village.⁹

Lewis and Clark, whose visit was brief, list but three bands: Canoe (700 individuals), Girl's (850), and Big Devil (1600) or the later Mountain Village band. Under *Remarks*, they wrote: "These bands act entirely independent of each other, although they claim a natural affinity and never make war on each other."¹⁰

Eleven minor bands, found mainly near trading posts and totaling 7000 individuals, were listed by Alexander Henry in 1807.

1. Little Girl (200 tipis), inhabiting Mouse River and Moose Hills.
2. Paddling and Foot (200), from the lakes of the Qu'Appelle River to the Missouri.

⁴ L. J. Burpee (ed.), *The Journal of La Verendrye and his Sons* (Champlain Society, Toronto, 1927), page 311

⁵ Burpee, *op cit*, page 446

⁶ *Ibid.*, page 359

⁷ *Ibid.*, page 366

⁸ L. R. Masson, *Les Bourgeois de la Compagnie du Nord-Ouest* (Quebec, 1880), Vol. 1, p. 27.

⁹ *Ibid.*, page 288

¹⁰ Meriwether Lewis and Wm. Clark, *Original Journal of the Lewis and Clark Expedition, 1793-95* (New York, 1904-05), Volume 6, pp. 104-105.

3. Canoe (160), living west of the Paddling and Foot bands.
4. Red River (24), living west of the Canoe.
5. Rabbit (30), living around Cypress Hills.
6. Stone (40), living near Thunder Mountain.
7. Eagle Hills (38), living between the Bear Hills and the South Branch of the Saskatchewan.
8. Those-Who-Have-Water-For-Themselves-Only (35), living near Elk Hills.
9. Saskatchewan (50) from the South Branch of the Saskatchewan to Eagle Hills.
10. Foot (33), living west of Eagle Hills and south of the South Branch of the Saskatchewan River.
11. Strong Wood (40), living between Battle River and the South Saskatchewan.¹¹

Of the above listed bands, Maximilian in 1833 noted only the Girl's, Stone, Canoe, and Strong Wood bands. Added to the list for the first time were the Big band, Band of Old People, Mountain and Bones bands.¹²

The six bands of American Assiniboiné given by Denig, as of 1854, correlated in part with the more complete number later obtained by Lowie.

1. Wato'paxnauⁿ. In Denig's time this band was called after Left Hand, a chief who governed them about 1800. This is the Mountain Village band of present designation.

2. Wazi'a wintca'cta, or Northern People. Originally this unit was called Lake People; the name being changed upon migration into the United States in 1839.

3. Wato'pabin, or Paddlers.

4. Witci'abin, or Girl's band.

5. In'yaⁿtoⁿ'waⁿbin, or Stone band.

6. Hu'deca'bine, or Red Buttock band. This group was formed about 1845 from a split in the Paddlers.¹³

In the summer of 1935 I checked the list of bands as received by Lowie in 1908. The list as corrected both in phonetic spelling and notation follows:

1. Tcaⁿxta'da—The Strong Woods Assiniboiné of Canada.

2. Un'ska'na—The Wanderers (Canada).

¹¹ E. Coues (ed.), *New Light on the History of the North-West Journals of A. Henry the Explorer* (New York, 1899), Volume 2, pp. 522-23.

¹² R. G. Thwaites (ed.), *Travels of Maximilian, Prince zu Wied* (Early Western Travels, Volume 22, Cleveland, 1906), p. 387.

¹³ Compare E. T. Denig, *Indian Tribes of the Upper Missouri* (Forty-fifth Annual Report, Bureau of American Ethnology, 1930), page 430. I have taken some liberties with Denig's list in light of later corrections.

3. Wazi'a wintca'cta—Northern People. At present on Fort Belknap reservation.

4. Wato'paxnatuⁿ—The name means "Those who propel boats," suggesting a past affiliation with the Paddlers (Fort Belknap).

5. Tcaⁿxe wintca'cta—Wood Mountain People (Canada).

6. Taniⁿ'tabin—Buffalo Hip (Canada).

7. Hu'deca'bine—Red Buttock (Canada).

8. Waci'aziya'bin—Fat Smokers (Canada).

9. Witci'abin—Girl's band (Fort Peck).

10. Iⁿyaⁿ'waⁿbin—Stone band (Canada).

11. Wato'pabin—Paddlers (Fort Peck).

12. Cuⁿktce'biⁿ—Dog's Penis (Canada).

13. Cahi'aiyeskabin—Cree Talkers (Canada).

14. Xe'natoⁿwaⁿ—Mountain Village (Fort Belknap).

15. Xe'bina—Mountain band (Fort Peck).

16. Iena'umbisa—Those Who Stay Alone.

17. Ini'naⁿu'mbi—Cypress Hills band (Canada).¹⁴

The pattern of behavior for the band chief formed the Assiniboine ideal of a "good man." Ideally, the chief's main concern was for his people. His interest in himself, or in his family, had to be submerged in this general reference to group welfare. Throughout the holding of his high rank, the chief strove to have himself well liked, since popularity was a prerequisite for the acceptance of his authority. This implied a certain amount of kindness on his part. At the same time, it was necessary that he be generous with his possessions. As a chief he had to give more feasts with their outlay of presents than any of the others in his band. Thus, another prerequisite for chieftainship was wealth, for the more given away at feasts the greater the number of individuals who would be obligated, in terms of the prevailing pattern. Since the chief was more often than not a successful hunter and warrior, he possessed supernatural power to a greater extent than any of his followers. As chief, this gift was to be used for the benefit of his group. As instance of this, I was told by Returning Hunter of his grandfather, Wing Crow, a band chief, who had gained power to cause buffalo to appear at will and whose popularity was greatly enhanced by the several times he had saved his people from starvation. As chief and leader of his people, no thanks were due him for this supernatural aid, such action being the obligation of office.

¹⁴ R. Lowie, *The Assiniboine* (Anthropological Papers, American Museum of Natural History, Volume 4, Part 1), pp. 33-34. Corrections and additions were made by August Moccasin, informant.

Again, due to the psychological set-up in the pre-reservation culture, the chief was expected not to injure the feelings of any of his followers. His behavior at all times was to be modest; his boasting to be restricted to the ceremonies where it was necessary to establish his right to his rank. The greater the chief in Assiniboiné life, the more modest was his overt behavior to be. For although greatness came from success in warfare and the hunt and the receiving of "power" from various spirits, yet fame could be of little benefit were it to fail of recognition by most of the band members. The fundamental pattern in the ideal of a "good man" was behavior that would receive approbation. Only those were elected to chieftainship who came closest to this ideal in the judgment of the band. Wealth coupled with parsimony and selfishness could never serve as prerequisite for the rank of band chief.

The duties of the chief were similar to those of a presiding officer. Decisions on important matters were made by the band council, not the chief. At these meetings the chief's counsel was considered as an individual opinion, rather than as representing the voice of the band. Certain judgments could be made by him independent of interference by the council; these being the right to set the line of march when traveling, where camp should be made and when it should break up, the right to regulate the going out of war parties or the sanctioning of such parties, and the right to call councils on matters of general interest. Whenever the chief employed the prerogatives of his office, he could be certain of having his decisions enforced by the Soldier's society.

Where more than one chief existed within the band, one was selected to rank as leader. The other chiefs, however, were invited by the leader to confer on all matters that pertained to the office. These accepted the leadership of the head chief and never attempted to embarrass him by publicly acting without his knowledge and consent.¹⁵ The minor chiefs were important members of the council and in status ranked equally with the leader. A successful chief was able to exert his power in the council by utilizing the friendships of his under-chiefs. Yet, in estimating the power of the band chiefs, it must be remembered that in no case were they able to overrule the majority opinion of the band as expressed through the other members of the council.

The council was composed of those men within the band who had reached a high status through success in war and the hunt. All men of family and all young warriors whose prestige was considered of sufficient importance by band opinion were eligible to join in the council meetings.

¹⁵ See Denig, *op. cit.*, page 544

there being no election to the office of councillor. The council was the legislative body of the band, no decisions being final unless made by it. Thus, no peace could be made with an enemy tribe without the majority consent of the council members present. At important affairs, such as occurred at gatherings of most of the Assiniboine bands, a council was formed that for the moment embraced the individual councils of the various bands. This had the right and the power to speak for the tribe as a whole. Such a gathering was brought together to pick representatives to meet the American commissioners at Fort Laramie in 1851.

Equalling the council in importance and rank was the Agi'cita or Soldier's society. These were the men whose function it was to act as a policing body in carrying out the decisions and policies of the chiefs and the council. In performance of duty they were to be superseded only by the agency police of the early reservation days. Members of this society were chosen from that group of men, twenty-five to forty-five years of age, who had previously distinguished themselves for bravery. The number of members varied. Denig stated that a band of two hundred tipis would have fifty to sixty men and a smaller unit of sixty tipis, ten to fifteen men.¹⁶

Superficially, the power of the Soldiers' society was such as to make it seem that they were the principal body within the band wielding political power. Larpeur, whose stay with the Assiniboine paralleled that of Denig, was of the opinion that the chief's power was of little consequence as compared with that of the soldiers.¹⁷ Larpeur's observation may be based upon the fact that he confused the policing power of the soldiers with legislative power, which was theirs by reason of their right to sit in councils as warriors.

The duties of the soldiers were varied. Under the leadership of their head man, picked by them for his influence and bravery, they protected strangers in camp, punished individuals who hunted individually at a time when the entire camp was in need of buffalo, arranged preliminaries of trade and peace, and in general carried out the decisions of the council. If theft was committed in camp the soldiers made a search of each tipi in order to find the stolen goods. Theirs was not the power to punish the thief, except when the latter resisted the recovery of the goods. As members of the Soldiers' society, they still continued to lead war parties and engage in warfare.¹⁸

The form and function of the Agi'cita, or Soldiers' society, was super-

¹⁶ Denig, *op. cit.*, page 436

¹⁷ Charles Larpeur, *Forty Years a Fur Trader* (Chicago, 1933), page 334

¹⁸ Denig, *op. cit.*, page 443, Larpeur, *op. cit.*, page 334

cially so akin to the pattern of the white military police of the early reservation days that many of the present day Assiniboiné were led to believe that the Agi'cita was an imitation of the white soldiery. That no imitation of pattern existed is evidenced by the policing of the Assiniboiné line of march in the early eighteenth century, as observed by La Verendrye.¹⁹

In every camp there existed one or two old men, in earlier life warriors and men of note, who notified the camp of all important decisions.

Although all individuals within Assiniboiné society were felt to be equal in needs and consideration, yet not all were felt to have those qualifications necessary for social approval derived from the pattern of the "ideal" man. Definite gradations of status existed, based completely upon merits that were arbitrarily defined by tradition. The distinctions in status were approximate steps toward the culturally defined goal, from the individual least approved to the one idealized by group opinion.

For an individual to be highly respected as the symbolized "ideal," certain prerequisites were necessary. These included success in warfare, since without that membership in the various societies would be virtually impossible; good fortune in hunting, that lavish feasts be given; procuring of as many horses as possible through any available means, to have the status accorded to wealthy individuals and to have the enhanced prestige gained by the giving away of these animals; possession of certain "powers" that stamped the individual apart from others less fortunate; pragmatic knowledge; and women who would aid in the storing up of non-income producing wealth. Coupled with these were the patterns of generosity, modesty, and consideration for those who formed group opinion.

There was no guarantee that rank once gained would continue to be held, since status was a purely relative matter in the judgment of the band, rank being defined in terms of competition for the cultural stamp of superiority.

Wealth of parents, however, was an important step in preparing a child for high status. Since the nucleus of rank depended mainly upon the desire to be well-known and considered by those who formed group opinion, the giving away of horses and presents by parents in order to have others praise their child was an important step toward eventually receiving social approval. An individual born to poverty would have a more difficult struggle, to rise on the basis of his abilities to the higher levels of status, than one of equal merit whose way was prepared by well-to-do parents. At all important ceremonies and feasts, presents were given away by parents in order that the recipients might "pray" for their child. It was not only a

¹⁹ La Verendrye, page 317.

matter of making certain that the child, by these means, would benefit as a recipient of supernatural aid, but also of importance that it have many well-wishing friends for later life. The acceptance of presents, whether in the form of horses or otherwise, definitely put the recipient under an obligation of friendship in later life.

Being old was of itself not sufficient to command respect. Old men and women were mainly respected for their past and the "powers" they still continued to enjoy. The concept "old" meant, to the Assiniboine, middle-aged, rather than old in the sense used today. The older a man became the more imperative it was for him to compensate for his lack of vigor by greater kindness to children and adolescents. To most old men in the aboriginal culture, age was a matter of which to be ashamed. Physical weakness and loss of memory brought in its train a gradual lowering of respect and the feeling of being a burden upon relatives. This was felt most keenly by the aged and consequently most of them sought death by permitting themselves to be abandoned on the plains.²⁰

Shamans who had not made their reputation in warfare or in the hunt were not considered high in status. These were important to the society, but of little influence in fashioning or changing public opinion. The same prerequisites for rank applied to them as to others. Shamans who were considered important by the band were those who had aided their war parties in securing various successes, or those who had amassed non-income producing wealth through their supernatural gifts.

The most influential body of men in the band was comprised of those of family, between forty and fifty, who had been camp soldiers and warriors of some note, who, due to their age, did not engage to any extent in warfare. They were mainly interested in preserving their status through securing recognition of their past acts and by charitable deeds. Their great concern was with their children and with acting as arbiters of socially desirable behavior.

The rank of various individuals within the band was known by their outward manner of decorating robes and tipi covers.

Women had no rank or social status except the respect due them by relatives; the prerequisites of rank applied only to men. The influence of women in councils was minimal, as their participation in such gatherings was forbidden by tradition. As women, no comparison could be made between them and men. As in all Plains societies, patterns of status were definitely masculine in form, with tendencies toward feminine traits being held in derision.

NEW HAVEN, CONNECTICUT

²⁰ Returning Hunter, informant. Also compare Denig, *op cit*, page 422.

THE present paper is based on three field seasons among the Mohave. References to other tribes are omitted because the data on the Mohave themselves are contradictory enough to make comparisons with other tribes undesirable.

All Mohave exhibit much interest in matters relating to the soul and its fate;¹ an interest quite in the general pattern of their culture, which has been described by Kroeber as a "dream culture."² This is only natural, since dreams are believed to be "real adventures of the soul." Interest in and knowledge of these matters is not limited to the more sophisticated or to shamans. Almost identically complete data have been obtained from two shamans and two lay informants.

From the point of view of immortality there are two categories of human beings. Except for certain accidents they may meet with while in human shape the souls of twins are immortal. The souls of other persons are mortal.

Origin of souls. Souls spring from the state of "aliveness" after conception has taken place. The souls of twins, however, have always existed in heaven and have no father and mother. After cremation the souls of twins return to heaven using another branch of the forked road which they followed when they came down to earth to assume human shape. Only once in their disembodied existence do they enter the womb of a woman at conception, in quest of incarnation. "One life is all they want," the informant commented ironically. After the death of their human bodies they continue to lead a deathless life in heaven.

The souls (matkwí'cà').³ All human beings have four souls, all of which resemble the body and duplicate its actions. They also have activities of their own. In the foetal stage they follow the actions of the mother and dream of "how to be born." The souls of foetal shamans, however, dream of how *not* to be born—that is, how to kill their mother during parturition. In other intra-uterine dreams they witness the origin of the world and listen to the instructions handed out to mankind by Pahó'tcatc, "who does it all over for them."⁴ Thus they exhibit already in the foetal state their shaman-

¹ Cf. R. B. Stratton, *The Captivity of the Oatman Girls* (San Francisco, n.d.), p. 155.

² A. L. Kroeber, oral information.

³ The phonetic system employed is that of the American Anthropological Association: *Phonetic Transcription of Indian Languages* (Smithsonian Miscellaneous Collections, Vol. 66, No. 6, 1916).

⁴ Cf. A. L. Kroeber, *Handbook of the Indians of California* (Bulletin, Bureau of American Ethnology, No. 78, 1925), p. 754.

istic propensities: the instinct to kill⁵ and their nexus with the supernaturals.

These souls are occasionally visible to their owner and to shamans. Occasionally they may also appear in dream to other persons. It is impossible to tell however just *how* one knows which soul one sees. "One just knows it." They can act independently of each other, as when one of the souls goes to visit the land of the dead.

The four souls. (a) Hlákù'yticitc. This is the "real shadow," the one whereby one is "proud" or "vain." It is the "second self" of a person, and in a way the core of his identity. It stands behind him as a rule. After death this soul alone survives and goes through three further metamorphoses. It receives the impact of the shaman's power when a person is bewitched. This soul of the bewitched person will see in dream the "real shadow" of the bewitcher. A shaman who wishes to divert suspicion from himself or cast suspicions of witchcraft upon another shaman, or to do both at one time, will kidnap the "real shadow" of the shaman he wishes to incriminate and make the bewitched person see this kidnapped "real shadow" rather than his own in his dreams. Furthermore, the "real shadow" will be the one to have commerce with the ghosts of the deceased—shamans always excepted whenever they send another soul to recover from the dead the abducted soul of a patient. The kidnapping of this soul, except for the purpose of using it for the above-mentioned deception, means death.

(b) Cúma' tc máhò'tvetc is the "power soul." It is somewhat akin to a more personal "mana" and does not seem related in any way to the concept of the guardian spirit. "One is what one is through the power of this soul." It brings both good and bad luck to its owner, according to his dreams (cúma' tc ála'yk, dream bad; cúma' tc áho' t, dream good).⁶ It gives general good luck, special powers (especially shamanistic powers), luck in love, etc., but can also bring unhappiness and bad luck. It is not unlike the "sacrum" concept of Durkheim and Mauss: it is sacred and dangerous. Although it dies at cremation, its effects persist beyond the grave insofar that "one remains in the other world what one has been in this world" (i.e., a shaman will remain a shaman even in the land of the ghosts). It is this soul the shaman sends to the ghostland, when he tries to recover a departed soul. He must not tarry there, however, lest his own deceased relatives should try to induce him to remain with them.⁷ This soul is seen when death is impending.

⁵ Cf. John G. Bourke, *Notes on the Cosmogony and Theogony of the Mojave Indians of the Rio Colorado, Arizona* (Journal of American Folk-Lore, Vol. 2 pp. 169-89, 1889), p. 175.

⁶ Cf. Kroeber, *op cit*, p. 754.

⁷ Cf. Kroeber, *op cit*, p. 778.

(c) Cúna'kavokyé'ttcitc is the soul of "worldly wealth," and the soul through whose agency one acquires wealth. It never causes trouble and its sight augurs success. This soul also dies at cremation.

(d) Matmakwí'ca: cúma'tc mítce'mvetc is seen only when one is about to die. Once it has been seen there is no recovery. It is either seen approaching or heard talking or coughing (or both) in the house. The ailing person will then think "It is like me." After this vision death is but a matter of hours or days. This soul too dies at cremation.

Twins also possess these four souls. Their immortality, however, is contained in their "real shadow." They strip off their other souls at death.

At cremation the "real shadow" changes into a ghost, known as the *navañí'*, and is carried away by a whirlwind either to the land of the ghosts—or into a rathole, if the chin of that person had never been tattooed.⁸ In the latter case the ghost will never reach the land of the dead. The souls of twins return to heaven.

When a person dies of witchcraft his bewitcher takes away his soul "to his own place" and keeps it there until he himself dies. Then they both proceed to the land of the dead. If the bewitched person is a shaman his bewitcher will "exile" his soul until he himself dies. The exiled soul is released and can join the other ghosts in the land of the dead. A certain shaman told one of my informants that he exiled the soul of a rival shaman whom he had bewitched "far beyond *Avi'kwama'.*"

The entrance to the "land of the dead" (*cálí'a' yt*) is somewhere near Needles, California, almost by the Colorado River on the Arizona side. There is something that looks like a big invisible "wash" containing a big invisible shed near a place called *Ahateku-pí'lyk*, which is but a few feet from the land of the dead.

For four days and four nights the ghost will visit his former haunts and be seen by his relatives and spouse in dream without any untoward effect to them, even if the wife or husband should dream of intercourse with the ghost. After that period the ghost settles down in the land of the dead and makes merry. According to Bourke⁹ this ghostland is near Bill Williams' Fork, and when a man dies his relatives often request a shaman to visit the land of the dead to check up if he reached it. Should the messenger fail to find the ghost there, witchcraft is assumed to have caused the death and the suspected shaman is killed to keep his victim company.

Usually the ghosts leave the living alone. Should one dream, however, of visiting or being visited by a ghost after the four days and nights have

⁸ Cf. Kroeber, *op cit*, p. 729

⁹ Cf. Bourke, *op cit*, p. 174

elapsed, it is assumed that either the living long for the dead or vice versa and that the dream represents a true soul-adventure. Such dreams cause illness and death,¹⁰ and a shaman is called in, either to bring back the patient's departed soul, or to cure the disease known as ghost-wéyla'k (wéyla'k navādí'). A curious form of this disease is the return of a dead infant many years after its death to the womb of his mother, wherein he makes himself a body from the clotting menstrual blood, without the help of spermatozoa. Dreams of sexual relations (especially incestuous intercourse) with ghosts are more fatal than other dreams. Baths and fumigations with arrow-weed scare away the ghosts.

One may visit the land of the dead with the help of a shaman specializing in the cure of witchcraft, who must under no condition let go the hand of the person he accompanies thither, lest the dead should keep him in the land of the ghosts.

After a while the ghosts die and are cremated by their fellow ghosts according to Mohave custom, since the ghostland is but a pleasant replica of the land of the living.¹¹ From the charcoal of the funeral pyre "some sort of a stink-bug, rough to touch, not the gray one, but the black kind," will be born. This insect is known as matkaθúa'n. It "faints" when a child picks it up in play and is then revived by blowing warm breath on it. (Blowing is a shamanistic practise.)

When this insect dies it becomes another kind of "bug" (?) known as úhu'lye, which has a long tail "like a rat." (According to verbal information from Mr A. M. Halpern this word means "mole" in Yuma.) When this being dies life is completely finished.

The cycle of metamorphoses has been differently described by Bourke¹² who claims that the ghosts turn into three different kinds of owls successively, then into a "water-beetle," and then into air. This explanation was unacceptable to my informants, who claim that although owls are somehow associated with *death* (mainly as evil omens), they are not associated with the *dead*. Unusual activities of owls, such as entering houses or roosting upon the shoulders of human beings augur of death in the same way that certain dreams augur of death.

In view of the accurately timed successive metamorphoses later generations cannot catch up with those predeceasing them by several years.¹³ Shamans, however, can bring about a compromise for their own benefit

¹⁰ Cf. Bourke *loc cit*

¹¹ Cf. Bourke *loc cit*

¹² Cf. Bourke, *op. cit.*, p. 180

¹³ Cf. Bourke, *op. cit.*, p. 174

by slowing up the first metamorphosis, and thus do away with the regret of never again meeting their predeceased beloved ones.

A shaman will thus bewitch a person of whom he is fond. He will seldom if ever bewitch those he hates, unless they be rival shamans. He segregates the ghosts of his beloved victims in an accessible place. He will visit them in his dreams, or else they will visit him, and sexual and other relations will occur between the dreamer and his disembodied captives. His victims are not seldom his own relatives with whom he cannot have commerce other than incestuous in the flesh. Berdache shamans have intercourse with ghosts of the same sex. The bewitcher keeps his victims in that place until he himself dies of a violent death. Should he fail to be killed, but die a natural death, he will lose his hold on them, and they will proceed to the land of the dead. Under these conditions they will "not belong to his group." "He does not bewitch people he dislikes. They would disrupt his nice group of followers." We already saw that if a shaman bewitches another shaman he exiles him until his own death to some far-away place.

In order to be able to maintain their hold on their ghostly retinue, shamans not seldom induce other people to kill them, by baiting them.¹⁴ This is a form of vicarious suicide. Should a shaman who bewitched a number of people die himself of witchcraft, his bewitcher will exile his ghost in turn and "take over" the ghost-retainers of his victim, until he himself dies or is killed.

Should twins die of witchcraft or be killed for practising witchcraft, they lose their immortality and follow the pattern of simple humans. In no case can they take the ghosts of their victims to heaven.

There is a curious ambivalence of emotions toward the dead. The lure of the ghosts and the charms of the ghostland have been described above. Yet Hall¹⁵ describes the case of a woman who was put on the pyre in what appears to have been a cataleptic state or apparent death and who revived on the pyre and was saved from the flames. After her unexpected revival she was treated with indifference. This incident mirrors the "ashes to ashes and dust to dust" aspect of the problem very adequately. Last of all this fear of ghosts is not unmingled with regret caused by the impossibility of joining those who died a long time ago. This is very apparent in the

¹⁴ Cf. Kroeber, *op. cit.*, p. 778, and Bourke, *op. cit.*, p. 174

¹⁵ Cf. Charlott Hall, *The Burning of a Mojave Chief* (Out West, Vol. 18, 1903), p. 63. Yet Mooney (*The Ghost-Dance Religion and the Sioux Outbreak of 1890*, Fourteenth Annual Report, Bureau of American Ethnology, Part 2, 1896, p. 785) claims that resurrection and the return of the dead was the principal doctrine of the Ghost Dance religion among the Mohave. If so, no one today remembers it.

following custom: should a person exclaim in surprise or anger nápa'ua, meaning "father's father," one of his own relatives sitting nearby will "feel bad" and say "Sure, there he comes—now he stumbles; he has hurt his toe. He falls—he will not come."

The conflict between longing for the dead and the impossibility of catching up with them should one live too long after they died leads to an appalling number of suicides and attempted suicides (ordinary, funeral, and shamanistic pseudo-suicides or vicarious suicides).

On the other hand, the selfsame lack of belief in personal immortality (and perhaps their respectful envy of the immortality of twins) induces the Mohave, according to their own testimony, to stress tribal continuity and to treat children and twins with the utmost kindness, lest the sensitive ones should "make themselves sick and die." It may also be ultimately responsible for the kind, considerate, and jovial character of the Mohave, which stresses some of the finest values of mutual kindness and mutual help.

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THE MUNICIPIOS OF THE MIDWESTERN HIGHLANDS OF GUATEMALA

By SOL TAX

LINGUISTIC DIVISIONS AND ETHNIC UNITS

EXCEPTING the Pipil, which is a Nahuatl language, and the Xincan (of undetermined affinities), the Indian languages of Guatemala all belong to the Mayan stock. The map (fig. 1) shows their present-day distribution. In the area labelled "Maya" the Maya-proper, or Yucatec, is spoken, closely related to it are the Lacandón and Mopán. The Chorti language has close affinities with the Chol of Mexico, which was also, until recently, represented in eastern Guatemala. The remaining languages have not been conclusively sub-classified, although there appears to be some merit in a recognized division of them into Mames, Quiché, and Pokom.¹

No matter how, with further research, Guatemalan languages may eventually be classified, and however the linguistic boundaries may be amended, it is likely that the names included in the map (fig. 1) represent real divisions and will remain, as they are, fixed in the literature of Central American linguistics.

These names are fixed as well, however, in ethnological literature. They are given to political groups of the time of the Conquest, and we speak of "kingdoms" of the Quichés, the Mams, and so on. Manuscripts written in early colonial days in Indian languages are referred to in these terms: thus the Tecpán manuscript is called "The Annals of the Cakchiquels" and the Popul Vuh is referred to as the "Quiché Bible." Contemporary ethnologists also make free use of the terms: Schultze-Jena writes on the "Quiché of Guatemala," Sapper on the life of the "Kekchi Indians," LaFarge on the "Jacalteca Indians of Guatemala," while McBryde calls Sololá a "Cakchiquel Market-Center." Should "Quiché Indians" be used to mean simply "those who speak Quiché dialects" and "Cakchiquel" those who speak Cakchiquel dialects, there would be no confusion. But in many cases the terms so used appear to refer to cultural and tribal groups, and one is left to infer that the Quiché, for example, are an ethnic entity with one definable culture and a political and social organization comparable with that of, say, the Iroquois. Thus more than one writer has called the Indians of Chichicastenango "the noble family of the Quichés," and the contribution of at least one ethnologist (Schultze-Jena) is almost nullified because he assumed that Quiché culture is enough of a unit to allow him to use

¹ Dr Andrade has now been working in the field and his findings give indications of the incompleteness of our present knowledge. The map (fig. 1) takes cognizance of some of his amendments to the recognized linguistic boundaries. His work is yet unpublished.

data from both Chichicastenango and Momostenango without distinguishing their sources.

That the Indians today who speak dialects of one language (such as Quiché or Cakchiquel) are not in any sense organized as a social group is evident from even a cursory study of Guatemala. There is no evidence,

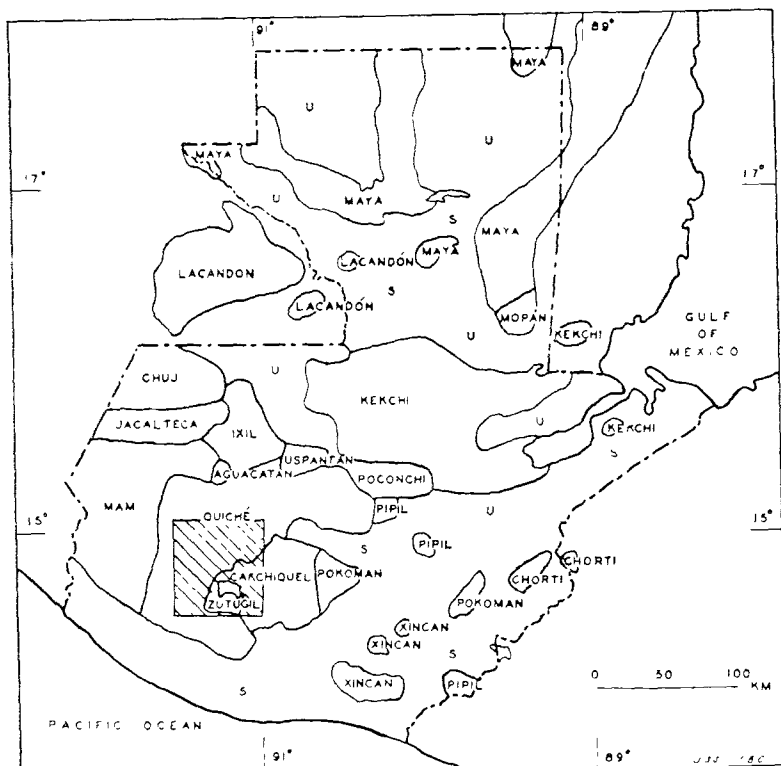


FIG. 1 The present-day language boundaries of Guatemala. U - unpopulated, S - Spanish (Largely after Urrutia)

furthermore, that there is more in common in the culture of any two Quiché-speaking communities (for example) than in that of a Quiché and a Cakchiquel community. Nor is it clear that such linguistic terms as Quiché represent political or cultural groups that existed at the time of the Conquest. In short, there is no reason to believe that culture, nationality, and language are more closely linked in Guatemala than in other places. The linguistic terms cannot be used unquestioningly, therefore, to describe

ethnic groups. In the nature of the case the coincidence of language and cultural divisions is something not to be assumed but to be determined.

Fortunately toward this end it is possible to isolate—quickly and certainly—groups of people who do represent, without question, social and cultural units; and it is possible to name and define their type of organization and to describe their respective cultures. The people of Guatemala live in *municipios* which are territorial administrative divisions commonly recognized in all governmental matters, but which are also—as it happens—the basic ethnic divisions and cultural groups into which the country is divided. A description of these *municipios* in one section of the country, which is the purpose of this paper,² will supply ample evidence that studies in the ethnology of Guatemala must begin with studies of the cultures of individual *municipios*.

THE MUNICIPIOS AS PHYSICAL UNITS

A high and rugged plateau runs the length of Guatemala from northwest to southeast. A narrow coastal plain borders the Pacific to the south, and a very broad one occupies most of the Atlantic drainage to the north. The mountainous country between, above about 4,000 feet in altitude, is usually referred to as the Highlands. Of the 353 *municipios* into which all Guatemala is divided, some 290 are in these Highlands; it is this group which is truest to type, which is home to almost all of the Indians of the country, and which is our chief concern. The *municipios* in the Highlands run smaller in area and larger in population than those of the lowlands. Thus, while the density of the country as a whole is 18.2 per square kilometer, in the highland region it is about 47, and in some sections as high as 100. In general, most of the highland *municipios* are 100 to 250 sq. km. in area and have populations of from one to five thousand.

On an east-west axis Guatemala City is almost in the center of the Highlands. To the west of it lies what may therefore be called the Western Highlands. The central area of this portion, which may be labelled the Midwestern Highlands, is shown in Figure 2. Since this area includes three traditional linguistic divisions—Quiché, Cakchiquel, and Zutugil—it is ideal for a study of the cultural meaning, if any, of these terms. It includes forty *municipios* that have been studied to some extent, of which twenty-four are Quiché, eleven Cakchiquel, four Zutugil, and one (San Pablo) either Cakchiquel or Zutugil.

The geography of the region is simple. The continental divide runs diagonally through the center, here the altitude reaches 3,000 meters and

² Based on sixteen months' field work, chiefly in the Midwestern Highlands

never falls below 2,500 meters (about 8,200 feet). North of the divide the fall is gradual and not great, south, it is rapid with the descent to the Pacific coast. The three outstanding features of the physical geography are:

1. The string of volcanoes that breaks the descent to the Pacific coast. They afford barriers to passage from the highlands to the coast, and strictly limit the number of trade routes.

2. Lake Atitlán, which some geologists claim is a crater lake while others say it was formed by a damming of the river waters by the rise of the volcanoes to the south of the lake. The lake is more important as a means of passage than as a barrier to communication.

3. The great irregularities of altitude due to the general formation, the presence of hills, and the numerous barrancas (canyons) that intersect the surfaces of hills and plains alike. These are the real barriers to travel, even if they are, to a remarkable extent, overcome.

Although the extreme heights are not inhabited, the population is otherwise distributed without much regard for altitude. The density is about the same at altitudes of from 1,500 to 3,000 meters. The largest settlements are at altitudes of from 2,000 to 2,500 meters, and they throw the balance in favor of that median, but there are other settlements, and most of the farm country is higher, while the fewer lower altitudes are also proportionally represented.

Because the boundaries of the municipios as they are marked off on the map (fig. 2) make no pretense of being accurate (since there are no official maps showing municipio lines, nor any unofficial ones by competent cartographers) it is not possible to tell why, geographically, the municipio boundaries are placed where they are. It may be said, however, that about a third of the borders are natural boundaries: volcanoes (with the dividing line through the peak or center), rivers, the one lake, and changes of altitude. The remaining two-thirds appear to be arbitrary, but closer mapping would undoubtedly reduce this proportion by taking into account smaller streams, ravines, and hills. The geography is more clearly important in delimiting the populated sections, or the towns, of the municipios. Not shown on the map is the suddenness of the rise of the land from the shores of Lake Atitlán; cliffs and volcanoes begin their precipitous rise from the very edge of the water, and only at the mouths of rivers and at passages between volcanoes is there room for settlement near the water, the towns of the lake all take advantage of such conditions, but their areas are in most cases strictly limited. The same may be said for many towns not on the lake (such as Chichicastenango) which are bordered and hemmed in by deep barrancas.

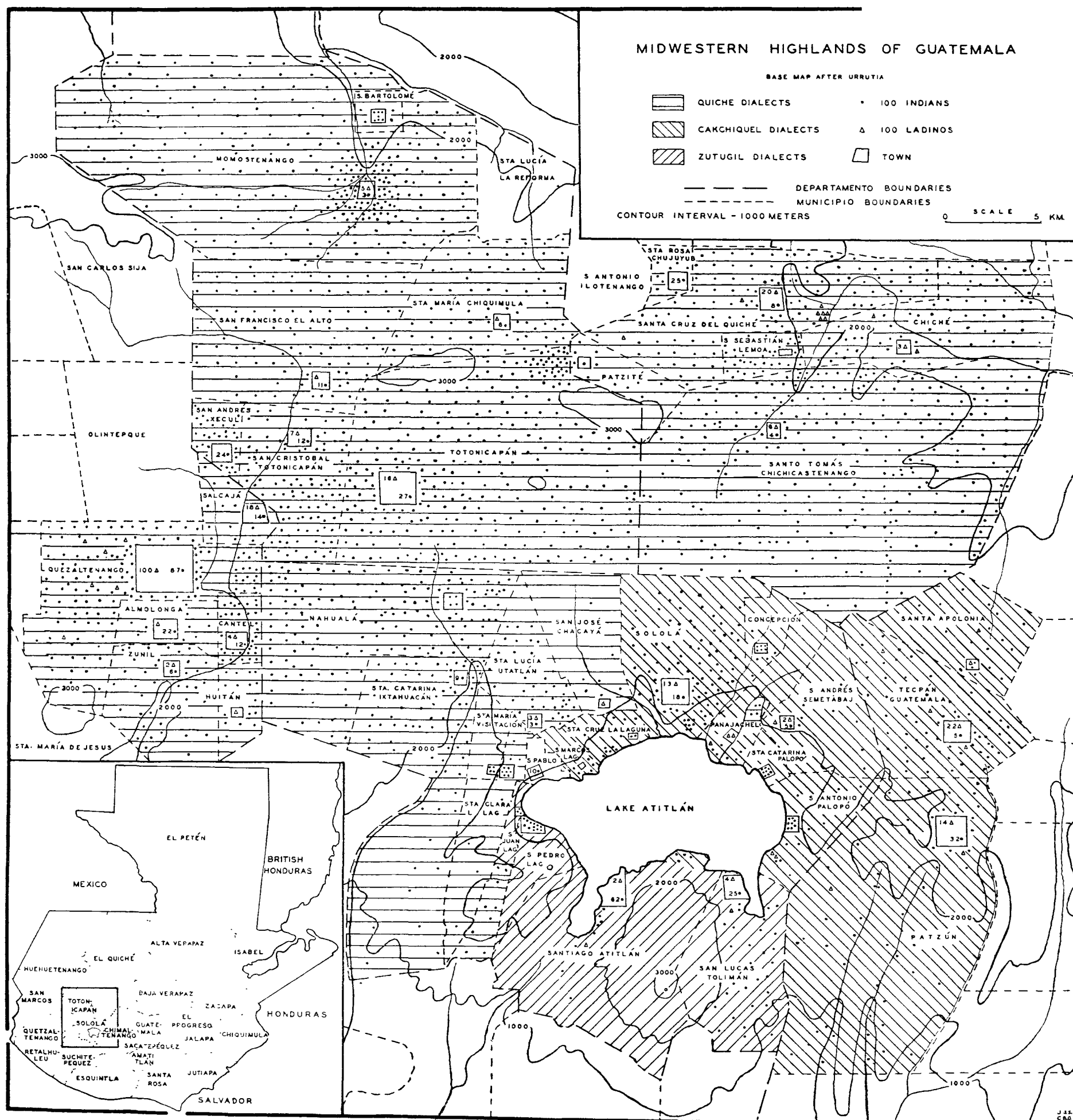


FIG. 2. Map of the Midwestern Highlands of Guatemala.

The rugged character of the country as it influences the divisions of the people is most important, however, in its effects on communication. The surface area delimited by the map (fig. 2) is about 2,000 square miles; yet in travel distance it is closer to 8,000. There are very few places (such as the valley route from Totonicapán to Quezaltenango) where more than a mile or two can be traversed without encountering changes of altitude—mountain obstructions or deep barrancas—which double or triple the distance to be travelled. Thus, from the town of Chichicastenango to that of Tecpán is (on the map) about twelve miles; yet it is a hard day's journey on foot and a matter of two hours even by automobile. So are distances correspondingly increased between most points in the area, with the exception that Lake Atitlán, to Indians who use canoes or who take transportation in motor launches, is an easy avenue.

The Indians, far from being isolated by these geographic conditions, are much given to travelling in spite of them: in commerce (wherein man himself is the chief beast of burden), in travelling to religious fiestas, or simply in going to see new places and new faces, much of the time of the Indians is spent in plodding over the rocky trails. Insofar as it makes the distances longer, the forbidding topography therefore makes travelling more important rather than less, and any tendency it might have toward isolating Indian groups is counteracted by the energy of the Indians themselves. Contradictory though it may seem, nevertheless the Indian groups—the municipios—bear, in their differences, the marks of isolation; and that they do cannot be attributed to their geographic isolation but rather to a resistance to the natural effects of constant contact!

TYPES OF MUNICIPIOS

Apparent in the map (fig. 2), and summarized in Table 1, are certain fundamental differences in the social set-up of the municipios. In the first place there are differences in the locale of the inhabitants, depending upon the percentages in the town and in the country surrounding the town respectively, in the second place there are differences in the social constitution, depending upon the relative numbers of Indians and of ladinos. The two sets of differences, although hardly functionally related, combine in different ways, and the manner in which they are combined in a particular municipio furnishes the crude data on its basic sociology.

Every municipio has its pueblo, or town-center; it is that whether or not it has any permanent residents. A municipio, the town of which has practically no permanent Indian residents, can be called a "vacant-town" municipio, and such municipios follow a definite pattern which can be

TABLE 1. A CLASSIFICATION OF THIRY-FIVE MUNICIPIOS OF THE MIDWESTERN HIGHLANDS
ACCORDING TO ECOLOGICAL TYPE AND THE COMPOSITION OF POPULATION

Town	Country	Minor Settlements	Municipios (and their Populations)			
			Vacant town Type	Combination Vacant town and Town-nucleus Type	Dispersed Town	Close-knit Town
Class of People in						
			Small Town Variety	Large Town Variety		
(None)	Indians	(None)	Lemoa (3726)			
Ladinos	Indians	(None)	Huitán (1764) Chacavá (844)			
		(Finca)*	Chiché (6723)			
	Ladinos and Indians	(None)	Utatlán (1593) Sta Apolonia (1974)			
			Monostehango (25,704)			
			S. Francisco (10,854)			
			S. Cristóbal (11,448)			
			Zunil (4450)			
	Indians	(None)	Chichimula (8043)	Quezalten'go (30,125)	Almolonga (3087)	
			Chichicastenango (25,137)	Teopán (9219) Patzún (9912)	Cantel (6657)	
<hr/>						
	Ladinos		Sometabaj (1600)	El Quiché (17,073)		
<hr/>						
	Indians			Totonicapán (29,970) Sololá (11,319)		

Town	Country	Municipios (and their Populations)			
		Minor Settlements	Vacant-town Type	Large Town Variety	Combination Vacant-town and Town-nucleus Type
			Small Town Variety	Dispersed Town	(lost-knit Town)
					S. Lucas (5775)
	(None)	(Finca) and Foreign Indians†		Panajachel (1450)	Atitlán (7675)
	(None)	(None)	Nahualá (16,325)	Xecul (5271) Sta. María de Jesús (399) Sta. Clara (1428)	
Indians		Indians		Sta. Cruz la Laguna (1197)	
		(Ladinos)			S. Antonio (1963)
Foreign Indians		(None)			S. Juan (1228)
(None)		(None)		S. Marcos (489)	Sta. Catarina P. (844) S. Pablo (1000) S. Pedro (2226)

* I.e., commercial plantations with ladinos and Indians; local and "foreign"

† "Foreign" refers to Indians who have come in from other municipios to live

described in terms of Chichicastenango which, as far as its local Indian population is concerned, is a typical example:

The municipio of Chichicastenango has a population of some 25,000. Of the native Indians, all but a dozen or so families have homes on their farms which are distributed over the countryside surrounding the town. Here they live and work, or (if they are travelling merchants or day-laborers employed elsewhere) have their base. The scattered households consist of simple families or various kinds of extended families. Since, in the inheritance of land, there is a division among the sons, a family's closest neighbors are likely to be relatives, yet the contacts of families here in the country are infrequent and casual.

In the town there are the church (or, rather, in this case two churches), the court-house, and the market-place. Many of the Indians own houses in the town as well as in the country, but during ordinary days these stand empty. Indians who are officials live in the town during their terms of office; the others come to town, if at all, on market-days (Sundays and Thursdays), when they use their town houses as headquarters, and for religious fiestas (especially the week in December when the day of the patron Santo Tomás is celebrated), when they live in their town houses or in those of friends.

The individual oscillates between the town and his country home, and the rhythm of cultural activity—weekly in economics, yearly in religion, and over a lifetime in the political and religious organization—corresponds exactly to periodic movements of the people from country to town and back again. Thus produce is grown and harvested in the country and is taken to town to sell; other commodities are bought in town and taken to be consumed in the country. Or a child is born in the country, the father goes to town to register the birth, there also he asks the prospective godfather to baptize the child; the godfather comes to the child's home in the country on Saturday and performs a rite, the family goes to town on Sunday, the godfather comes to their town home to take the child to have it baptized; the family returns to the country at night. Or a man dies in the country, where a wake is held, the next day there is a funeral to the cemetery in town; the bereaved and their friends then go to the family's town house for the night, which they spend drinking. So it is with courtship and marriage; so also in the lives of political and religious officials, so in almost all phases of the culture. A geographic duality appears to be typical of the vacant-town municipios.

A variation of this type occurs when, due to the introduction of outside racial and political factors, the town is large and has considerable importance for other municipios (such as when it is the capital of a departamento). It is still a vacant-town as far as the local Indians are concerned, but with its great population of ladinos it hardly gives that appearance. The large town, however, also affects the residence of the Indians in that it furnishes permanent employment to a number of them, who thereupon come to live in it permanently.

A municipio in which practically all of the people live in the town we may call a "town-nucleus" municipio, and the type may be described in terms of Santiago Atitlán, which is its largest, if not its purest representative:

All but a few hundred of the 7,700 inhabitants of the municipio of Atitlán live in a large town of closely packed compounds of houses ranged along fairly regular streets. The people have their farms in the surrounding country (chiefly on the sides of the volcanoes) and the men leave home early in the morning when necessary, work in the fields all day, and return home at night to sleep. There are no houses in or near the fields, and the men bring their lunches. Many of the men are merchants, and may be gone several days of each week, but of course the homes to which they return are in the town.

With the men usually away during the day, women constitute most of the visible population. There are certain designated watering and laundry places on the lake shore, and here they congregate—at regular hours—to do their work and to exchange gossip. The women rarely leave the town, and some spend their entire lives within its bounds. Twice a day, in the plaza, there is a market for two or three hours and here they come (no men buy or sell in the market) and sit in groups or in rows displaying their wares and, again, exchanging information. Time not spent at the lake shore or in the plaza is occupied with cooking and weaving at home, but since the people live in family compounds—which themselves are close together—there is no little social life at home as well.

Naturally, most of the normal functions of life—individual and social—are confined to the town. The rhythm of life, unhampered by coincident movement in space, is at once quicker and less emphatic than in the vacant-town municipio. The round of days in the week loses most of its significance and what there is is not so uniform for all individuals. To be an official for a year affects one's time, but not his residence nor his whole mode of existence. The annual fiesta is just as important, but more attention is given the images in the church during the whole year, and the fiesta itself does not necessitate a bodily movement of all of the people. There is no duality, and life is evened out.

One important difference between the two types is that whereas in the first municipio-boundaries are all important and the term "pueblo" is thought to refer to the whole municipio, in the second the town itself is everything and the municipio-boundaries comparatively incidental.

There is one important variation from the town-nucleus type: in some municipios the town contains all of the people, and they work the surrounding fields from it; but the houses of the town are distributed over a considerable area so that there is space for a limited cultivation of fruits, vegetables, and other crops. The people here do not lead a dual existence, however, and the chief result is that the one home is more important to both the men and the women than in the pure type.

The third type of municipio is a combination of the other two. Perhaps half of the people live in the town and work their fields closely surrounding it, but the other half live on their farms and come to town (where they may have houses) only on the usual occasions. A municipio of any of these three types may have other settlements—small villages or plantations—in addition to the town; these settlements may have their own churches, subordinate court-houses, and even markets; but in general the people of the settlement depend upon the town (but to a lesser degree) in their economic, political, and religious organization.

It remains to define the classes of people who live in these various kinds of municipios. Presumably an "Indian" is a pure-blooded descendant of the people of pre-Columbian Guatemala, and most of them probably are. A ladino, on the other hand, is supposed to represent a mixture of Indian and Spanish in remote times. Actually the distinction is commonly based on language and culture rather than physical type, Indians speak Indian languages, wear Indian costumes, have Indian surnames, and live like Indians. A ladino has a Spanish surname and speaks Spanish as a mother tongue; he wears European-type clothes, wears shoes, lives in a house with windows, is usually literate, and has, in general, a better standard of living than his Indian neighbor. None of these criteria hold universally, but on the basis of all of them one can usually make a safe judgment.

The ladinos in the Midwestern Highlands are mostly town-dwellers. They are the ones, in fact, who make the large towns large; but they also inhabit otherwise entirely "vacant towns;" they are found in fair proportions in three of the "town-nuclei;" they inhabit subsidiary settlements, and in two municipios they share the countryside with Indians. In Table 1 it can be seen that while they favor one type of municipio, they may be found in any arrangement.

A third social class may be termed "foreign Indians." They are native to one town, and still speak its language and wear its costume, but they have migrated to another. They too are found in both major types of municipios; they sometimes constitute the population of a smaller settlement and in some towns (such as Chichicastenango) the Indians marked on the map are mostly those who constitute the foreign colony.

When both the type of municipio and the character of the inhabitants are taken into consideration, few municipios can be classed together as exactly alike. One generalization is, however, apparent: that all of the town-nucleus municipios shown on the map are in the area immediately surrounding the lake. Since such a pattern easily appears to follow from the peculiar geography of the lake itself, it may be fair to conclude that the

difference is explicable in terms of geography and that (since the lake is unique) the town-nucleus municipios are atypical. Whatever the origin, however, it cannot be gainsaid that in respect to town-types (and whatever cultural consequences they may have) there are in the Midwestern Highlands two distinct types of society, and their distribution cuts across linguistic divisions. That the areas representing social types do cut across broad linguistic divisions is important chiefly because if they should have happened to coincide something might be made of the fact. The fact of the matter appears to be, however, that the lake municipios are alike *only* in respect to town type (and its consequences) and in most respects they, like the other municipios in this region, differ individually each from every other.

THE MUNICIPIOS AS SOCIAL UNITS

From the point of view of the Indians themselves, the people of each municipio constitute a unique group, united by blood and tradition and differing from all others in history, language, and culture. There are, indeed, exceptions: Chichicastenango and Chiché, Santa María Chiquimula and Patzité, Santa Catarina Ixtahuacán and Nahualá are thought of in pairs. In each case the tradition (and in the last case definite history) is that each pair once constituted a single municipio; and those respects in which they are now the same are indicative of the character conceded to single municipios. In each case the people of both municipios speak the same dialect, different from those of others, have the same patron saint and a common tradition of origin, have the same costume, and apply to themselves the same name. In these exceptional cases, two municipios are thought of—in history, culture, and race—as one; but otherwise each municipio is a discrete unit. That the belief has its counterpart in objective fact is very clear: the municipios do differ in language, obviously in their saints, their names, and their costumes, probably also to some extent in physical type. They are, moreover, conscious of their unity and their uniqueness: they disapprove of marriage outside the municipio; when they travel they consider themselves strangers on foreign shores and think of a person from the home municipio as a “countryman,” and if a colony is established in another municipio the people of the colony keep up their home customs and mix only so far as is necessary with the local Indians.

The names applied to the Indians of the various municipios and recognized by all others as well as by themselves are taken from the names of the municipios themselves, in Spanish fashion. Thus one from Quezaltenango is a Quezalteco; from Totonicapán a Totonicapeño, from Atitlán an Atiteco; from San Andrés a Sandresano; from Santo Tomás Chichicastenango as well as

from Chiché a Maxeño (from the Indian equivalent of Tomás); and so on. There are Indian equivalents of these terms, but since the dialects differ, the Indian names differ to some extent: at any rate, they are usually translations of the Spanish, so that (in Atitlán) a Pedrano becomes ax-sampegra and a Juanero ax-saxwan. Sometimes the Indian name takes its origin from the part of the municipio name not used in the Spanish name: thus, in Spanish a person from San Antonio Palapó is an Antoñero, but the Atiteco calls him ax-palpo'oxi'; also, a person from either Nahualá or Santa Catarina Ixtahuacán is a Nahualeño in Spanish, but ax-catalina in Atitlán. Whatever the name, however, what is most important is that the Indians are labelled by municipios, and furthermore that these names are more than simply identificatory: a Maxeño is a Maxeño no matter where he lives, even if he is two or three generations removed from the homeland. If one asks a man from Patanatic (a colony several generations old in the municipio of Panajachel) from whence he hails, he will answer that he is a Totonicapeño. These names are used in Guatemala, therefore, very much as our terms "Jew" and "Gypsy" are.

Together with the name goes a costume. Typically each municipio has a costume for its men and one for its women which differ from those of even its neighboring municipios; sometimes the differences appear objectively minor, but they are always sufficient to distinguish the wearers and to label them. The climate, in general, influences the type of costume, so that at altitudes from four to six thousand feet cotton is the basic material used, but at higher altitudes wool is more frequent. But custom and style are even more important: thus the women in Chichicastenango (which is quite cold) wear their cotton skirts knee-length while in Atitlán (much warmer) they are ankle-length, and where in Chichicastenango the men wear knee-length woolen trousers, in San Marcos la Laguna the men cover their entire bodies with long woolen cloaks.

Some idea of the differences that obtain in the costumes of even closely neighboring municipios can be had from a comparison of the women's "uniforms" of Atitlán and San Pedro.

Skirt. Both wear ankle-length wrap-around skirts, but the Atiteca's is bright red with fine white lines, while the Pedrana's is of a dark red, blue, or green tied-dyed material.

Belt. The Atiteca simply tucks in the outer edge of her skirt, but the Pedrana in addition to this, binds her skirt at the waist line with a three-inch woven cotton belt.

Blouse. The Atiteca wears the loose huipil, the sleeves of which are not cut or sewn, but the Pedrana has a European style blouse, with tailored sleeves. That of

the Atiteca is white with orange and lavender stripes and silk figures of the same colors, while that of the Pedrana is all white (with corded stripes of the same color).

Carrying cloth: The Atiteca's cloth is a rectangle 64 by 24 inches, of which only one end has fringe; the Pedrana's is much longer (120 by 20 inches) with fringe on both ends. The Atiteca's consists of wide red and blue stripes with some finer stripes of white and lavender; the Pedrana's is dark red, blue, or green, with tie-dyed designs.

Head-dress: The Atiteca has a long tape (335 inches long and an inch wide) with the ends decorated. She winds her hair in the center part and then winds the whole tape around her head, evenly, so that the final effect is that of a halo, or of the rings of Saturn. The Pedrana, on the other hand, braids her hair into two braids and simply lets it go at that.

The uniformity of costume within a municipio is only relative: there are minor variations, but there is rarely any variation so wide as to confuse the identification. Furthermore, costumes change in time; in some municipios the men are substituting European trousers and shirts for their old garb, but the changes in most places occur within the native costume pattern itself—in matters of color, texture, and design—and the costume as it changes remains typical of that municipio.

Since Indians are rarely encountered without their clothes, it is not easy to check (without anthropometric measurements of the finest kind) the Indians' judgment that they differ by municipios racially or physically. Since, however, the municipios are in large degree endogamous one should rather expect differences. Actually there appear to be many: Chichicastenango women appear to be shorter and uglier than those from Momostenango, for example; and the Atitlán women seem slimmer than most of the others and their faces have a Malayan cast. The men of San Pedro are notoriously handsome, and the women are famed for their beauty: there appears to be, in San Pedro, a very strong mixture of white blood. These are examples of superficial judgments made by Indians and tourists. They may have no importance for the physical anthropologist, but they certainly do in connection with the ethnic nature of the municipios. An Indian thinks of the people of his municipio as related by blood, and to him the costume-labels are superfluous or incidental.³

The Indian judgment concerning language differences has, fortunately, been better tested. The Indians of a municipio know that their language is different from their neighbor's: it is part of their uniqueness. Actually, Dr. Andrade has found, the dialects as one goes from municipio to municipio

³ Partly as a result of the endogamy, another difference found in municipios is that each tends to have its own typical Indian surnames.

TABLE 2. COMPARATIVE VOCABULARY (OF SELECTED WORDS) IN THE TEN LAKE MUNICIPALITIES
(From material collected by Dr M. J. Andrade)

	Cakchiquel			Zutugil			Zutugil or Cakchiquel		Cakchiquel	
	Panajachel	Sta. Cat.	S. Ant.	Tolimán	Atitlán	San Pedro	San Juan	San Pablo	San Marcos	Santa Cruz
I	yín (yen)	riyín'	yín	rin	imin'	imin'	imin'	imin'	rien'	rin
Those	rit	riol'	yet	atal'	atal'	atal'	atal'	atol'	rial'	rat
He	riš ³	ria ³	riya ³	rixa ³	xa ³	arxa ³	xa ³	rixa ³	rixa ³	rixa ³
We	riox'	riox'	riox'	oxox'	axox'	oxox'	oxox'	roxox'	riox'	riox'
You	riš	riš	riš	išiš'	išiš'	išiš'	išiš'	riš	riš	riš
They	rie ³	rie ³	riye ³	rixe ³	xae ³	xae ³	xae ³	rie ³	rie ³	rixe ³
Where?	kučú'	ač'	ač'	ap'ar'	p'ar	p'a	p'a (la)	bar	či	či
When?	xampe ³	xampe ³	xampe ³	ač'ike'	ap'axan'	naq-	baxan'	baxan'	xan	xan
What?	atus'	atiš'	atus'	naq	naq	naq	naq	ač'ika'	naq	č'ika'
Cat	sian'	syam	syam	siam'	syaw	syaw	mes	mas	wiš	syam
Pig	aq	aq	aq	aq	axq	ahq	kuč	kuč	kuč (aq)	aq
Fish	kor	kar	ker	č'u ³	č'u ³	č'u ³	č'u ³	č'u ³	č'u ³	č'u ³
Little	timik'	č'itino ³ y'	č'uti'	č'u'ti (kolr')	kola'	tino ³ y'	tino ³ y'	č'itimak ³	tillaš'	timik'

in the Midwestern Highlands do differ remarkably. Furthermore, the differences seem to include phonetic variations, vocabulary, and grammatical usages. By way of example, Table 2 summarizes the differences in typical words as Andrade found them in the lake villages. The comparison runs from Panajachel around Lake Atitlán clockwise to Santa Cruz. Both Cakchiquel and Zutugil dialects are included. That there is a difference is apparent, but that it is not great (assuming the word list is indicative) is also clear. But what stand out are the differences even within the language groups as one goes from town to town. The differences are actually greater in the spoken language than they appear in the table, for not only are there differences in phonetic systems, vocabulary, and grammar, but intonations vary remarkably and they strongly affect the mutual intelligibility. Atitlán is a striking example of this: the Atitecos not only speak in a higher pitch (especially when the least excited) and with different intonations, but they normally speak at a measurably faster rate than their neighbors. People in any of the other villages claim that they cannot understand the Atitecos at all, and they think that the language of Atitlán is utterly different. Curiously, however, the Atitecos can pretty well understand any of the others, who, they admit, speak "more clearly" than they do themselves.

While none of the dialects around the lake are entirely unintelligible one to another, the differences in every case are sufficient to make understanding difficult. The Indians manage to communicate, nevertheless, in all ordinary matters and this is partly accomplished by their partially learning each others' languages. The fact that they do manage to communicate in no way minimizes the importance of the differences, which, if they function in no other way, continually remind the Indians of each municipio of their unity and their uniqueness.

Perhaps no case in the whole area is as striking as that of the four villages of San Pedro, San Juan, San Pablo, and San Marcos. The towns are within sight of each other; San Pedro and San Juan are separated by only a few minutes' walk; and most of the land in San Juan is owned and worked by Pedranos. That there should be any difference at all in their languages is surprising enough, but that the differences are so great seems little short of the marvelous. That these four municipios differ in language as they do certainly lends assurance to the assertion that not the groups known as Cakchiquel and Zutugil, but rather the individual municipios are the linguistic units to be primarily studied in the area. What is said for the lake region is more or less true for the whole of the Midwestern Highlands. Elsewhere the town-centers of the municipios are not so close; thus the case does not appear so striking. Yet in some ways the facts are

even more emphatic, for with the Indians living in the country there are many cases of close neighbors (on different sides of a municipio boundary who speak different dialects.

If the municipios typically are set apart one from another by language, costume, and a consciousness of racial, cultural, and historical uniqueness, as well as by differing ecological compositions and population elements, they constitute economic, political, and religious units as well.

THE MUNICIPIOS AS CULTURAL UNITS

The irregularities of topography, and the varieties of geographic and geologic conditions in the Highlands, give rise to different production areas, but geography alone cannot account for the economic specialization of the municipios of the Midwestern Highlands. It may be said at the beginning that although the milpa as home-garden agriculture is common to all of the municipios, each one devotes itself to some economic pursuit that, as income-producing activity, supersedes the milpa in importance. Few of the municipios grow sufficient maize for their own needs, and fewer still grow enough to enable the people to buy—with the proceeds of the excess—the other necessary commodities. Those which do may be said to be specialized in the growing of maize (and beans) as a money-crop; as far as I know, only Tecpán, San Andrés Semetabaj, and Patzún, of the municipios shown on the map (fig. 2), are so specialized. The Indians of the other municipios must earn money in other ways if they are to buy their necessities, including, in many cases, maize.

Specialization in production leads inevitably, of course, to commerce, and the Indians of some municipios specialize in this commerce (or distribution) itself. Roughly, there are three kinds of specialization: agricultural, industrial, and commercial. A fourth is rather negative: Indians of municipios not specializing in some particular pursuit are forced to resort to common labor in their own or neighboring municipios or on the plantations on the coast. Likewise, even in municipios where most people have a more particular occupation, there are always some individuals who for at least part of the year depend upon labor in the fields of others for their living.

The municipios that specialize in the growing of maize and beans have already been mentioned; others (especially Patzité, and to some extent most of those of the higher region) grow wheat as an important money-crop; tropical fruits such as oranges, limas, and limes, jocotes, matazanos, and aguacates are grown in some of the lake municipios, as is coffee; fruit such as apples and peaches are grown especially in Chichicastenango.

quiché, and Totonicapán; vegetables are grown almost exclusively in Almolonga and Zunil and in Sololá and Panajachel, potatoes in Chichicastenango and Totonicapán as well as in Almolonga. Such miscellaneous crops as anise, chick-peas, and maguey are grown in particular municipios around the lake. The relation of geography to such agricultural specialties may be illustrated by the growing of vegetables.

In the rainy season vegetables could be grown anywhere; yet, as far as I know, no municipio grows vegetables only in the rainy season—probably partly because that is also the milpa season. In the dry season, irrigation is always necessary, and both favorable circumstances and technical knowledge are necessary for such agriculture. There are many places where there are streams that could be diverted into irrigation ditches; some are better than others, but numberless places are no worse than Sololá, where ordinary streams are used in intensive agriculture. In most places there is neither the interest nor the knowledge to make use of the natural resources; or the people have some other sufficient specialty. A few places are especially favorable for irrigation, such as Almolonga and Panajachel, where large rivers furnish abundant water to irrigate their valleys. Panajachel is perhaps the most important onion and garlic center, and it grows a few other vegetables as well; Almolonga grows onions and other vegetables, but no garlic. The techniques are much better developed in Panajachel, yet in Almolonga they use the terraced hillsides in the rainy season as well as the valley in the dry season—something not heard of in Panajachel. In Panajachel onions and garlic are the important things in life—much more important than maize—and have entered into the values of the culture very thoroughly; economically, the people spend almost all of their time in the onion and garlic patches; even the women and children become expert, and many of the women do not grind corn or weave, while almost none of them spin cotton. Their specialty is all-important. The growing of vegetables is spreading now to other towns on the lake, where the water of the lake itself is used on the crops on the lake shore.

Industrial specialization has gone even farther than agricultural. It would be difficult to speak of the "technology" of the Indians except by specifying particular municipios. Pottery, for example, although used everywhere in the home almost to the exclusion of other vessels, is made in only four municipios of the Midwestern Highlands, and each makes a special type hardly in competition with the others. Totonicapán makes many varieties of fine glazed ware; San Pedro Jocopilas and Santa Apolonia make large pieces of unglazed ware, chiefly water-jars and griddles; San Bartolomé makes a large pot used in Momostenango for dyeing textiles. Mats are made in important quantities only in Santa Catarina Palopó. Furniture is a practical monopoly of Totonicapán; so are leather goods. Blankets and other woolen textiles are made in Momostenango and San

Francisco, while other types are woven in Chichicastenango and Nahualá. Many of the necessities are made outside of the area altogether (practically all baskets, for example) and traded in. Other things are made in the area and also in municipios outside: thus rope and hammocks are a specialty of San Pablo, but most of the rope and other maguey products used in the Midwestern Highlands come from municipios to the northeast. Grinding stones made in Nahualá, on the other hand, apparently supply the entire republic. Nor are these specialties always explicable in terms of geography: pottery-making, for example, stops at the boundaries of Totonicapán, and the closest neighbors on the other side of the line simply do not know how to make it. Likewise, a plenitude of maguey grows wild in all parts, but only in a few of the lake municipios is it worked to any extent, and although San Pedro la Laguna has great quantities of it, the leaves for the most part are purchased by Pobleños who twist the fibers into rope. Or, to take another example, the raw materials—most of the wool and the dyes—must be brought into Momostenango, yet the Momostecos are the experts who make the blankets.

Naturally, the distribution of all of these products is a matter of utmost importance in the economy of the region. Webster McBryde has recently completed a year's study of the trade routes, chiefly in this area, and a short article like this is no place to do justice to the intricate network, and the market system, that is found. The produce is carried on the backs of Indians, or occasionally on a mule. The producers themselves almost always take their products to the local market, or to neighboring markets: in some cases (such as with Momostenango blankets, Nahualá grinding-stones, and Totonicapán furniture) the Indians of the same municipios carry their own or their countrymen's products all over the republic. But there are Indians in certain municipios who make a business of carrying products from still other municipios from market to market, buying and selling. Thus their makers carry pottery and furniture into Chichicastenango, where Maxeños buy it and take it to the coast and as far as the capital to sell. In addition, Indians from Chichicastenango, Totonicapán, and San Cristobal buy factory-goods in the capital or in Quezaltenango and have regular routes on which they stop in various markets to sell their merchandise. Atitecos and Maxeños, and a few others, also have a coast-highland trade which carries them everywhere. In such commercial specializations it is difficult to find geographic justification. It would appear that trade (which is really cartage) is resorted to when people have no better resource at home or are especially ambitious but not so poor as not to have a little capital to engage in trade.

TABLE 1. Importance of various occupations in the different municipalities (Importance ranging from 1 to 10, 1 being the least important)

<i>Economic Pursuit</i>		<i>Panajachel</i>	<i>San Catarina</i>	<i>San Antonio</i>	<i>Tolimán</i>	<i>Artián</i>	<i>San Pedro</i>	<i>San Juan</i>	<i>San Pablo</i>	<i>San Marcos</i>	<i>Santa Cruz</i>
Agriculture, etc.	Milpa, mature maize and beans	XX	X	XXX	XX	X	XXX	X		XXX	XXX
	Milpa, green corn-on-cob					XXXX					
	Vegetables; onions and garlic	XXXX				X	X				
	Tomatoes			XX	X	XXX	X				
	Coffee	XX			XXX						
	Fruit	XX		X	X	X	X	X		XXX	XXX
	Sugar-cane									X	XX
	Anise			XXXX	X		XXX				
	Chick-peas				X	XX	XXX	X	XXX	XX	X
	Wild maguay	X					XXX		XX		
Fishing	Cultivated maguay								XX		
	Fishing		XXXX			XXX				X	
Industries	Crab-fishing		XXXX			X				X	
	Mat-making		XXXX							X	
	Maguay-working (rope, twine, etc.)						X	X	XXXX	X	
	Weaving (by women only)	XX	XX	XXX	XX	XXXX	XXXX	XXX		XX	XX
	Canoe-making			XX		XXXX	XX		X		
Commerce	Selling own produce	XXX	XX	X	?	XX	X		XXXX	XX	XX
	Buying and selling		X	X	?	XXXX	X				
Labor	In own or neighboring municipio	X	XXXX	?	?						
	In commercial plantations		XXXX	?	?	X		XXXX		X	

Almost any group of municipios presents its cases of specialization that are difficult to explain, but again the lake municipios may be taken as striking examples. Table 3 shows the economic basis of each of them; and it obviously raises more questions than it can answer. Why do Catarinecos engage in lake industries (they are expert canoers, swimmers, fishermen) while the neighboring Antoñeros are land-bound? Why are the Atitecos, rather than the Pedranos, the great merchants? Why are the Juaneros and the Catarinecos so much more bound to labor than the others? The answers to these questions, and others, are bound up with history and with the whole cultures of the municipios, even to religion. Such differences go far towards differentiating municipios: their technologies, their economics, and the trend of their cultural interests, even their standards of living differ with their particular economic specializations both as causes and as effects. And it must be pointed out finally that economic specialization occurs, of course, by *municipios*, or small groups of them, and not by groups related to language divisions or possible culture-areas.

Just as in economic matters the municipios are differentiated within a common (and far-reaching) system, in both politics and religion each achieves unity and relative independence within two complex and world-wide organizations. The division into municipios is recognized by the Constitution of the republic, and the general form of their internal organization is officially outlined: in like manner the Roman Catholic Church sanctions the elements of local religious organization which are, after a fashion, fitted to its ideology. Yet the peculiar formal combination of secular and sacred forms and functions in municipio organization are recognized by neither the Church nor the State. That organization follows a pattern general throughout the Midwestern Highlands, but in each municipio it is bound to the particular ecological and racial (ladino-Indian) composition, to local customs, to particular *santos* and the yearly ritual cycle, to the economic system and specialties, and even, in some clear cases, to the character of the people.

In brief summary, there is a hierarchy of secular officials in each municipio with functions ranging from those of a combined mayor and justice of the peace to those of janitors and messengers: there is a parallel hierarchy of sacred officials in charge of the important *santos* of the municipio. The officers are theoretically "elected" but actually they take turns, starting with the lower offices and gradually moving higher and higher: in the progression there is an alternation between the secular and sacred hierarchies, so that the two are effectively linked. Eventually, having passed through all of the offices, the individual becomes a principal,

an elder in the community exempt from further service to the town. There are almost as many variations of this scheme, in detail, as there are municipios; a few examples follow:

1. Where there are ladinos in the municipio, they are fitted into the political (not the religious) system in various ways. In the ladino large-towns there are independent organizations; the officially recognized system has ladinos in the higher offices, but parallel to it there is a complete Indian organization, unofficial where it conflicts with the other (i.e., in the higher offices) but official as regards the lower officers who are always Indians. In smaller-town municipios, and in the lake municipios, there is but one system with the higher offices alternating between ladinos and Indians; even here, however (in Panajachel, for example), the Indians have a complete roster of officials, those who conflict with ladinos existing in Indian custom alone.

2. In large municipios (such as Chichicastenango) there is added to the central vertical organization a series of officials chosen from and functioning in territorial subdivisions. The municipio (and in Atitlán the town itself) is divided into cantones, each with a local official subsidiary to the municipio officials, which function for administrative purposes. In such municipios the principales have territorial bases and functions, and in some cases there are groups of higher principales functioning for the whole municipio.

3. In small municipios every man has his turn at filling most of the offices. In large ones (where there are more men than offices) some selection (supervised by the principales) occurs for the higher offices, and the others give their service to the municipio in more menial tasks. There is thus more honor and prestige connected with the higher offices, both political and religious.

4. The functions of the secular officials are in varying degrees always partly religious as well; as a group they take part in the ceremonies supervised by the religious officials. In some cases (Panajachel, for example) the secular officials themselves have a santo in their charge, however, and in such cases they have religious functions in the same sense that the religious officials have.

5. Each municipio has its own roster of santos, and the name-day of each is the occasion of a fiesta. Therefore each municipio celebrates according to its own particular calendar. Furthermore, since each santo has its particular connotations and requires its celebration in its own particular manner, the details of fiestas differ with the municipios.

6. The importance of the fiestas themselves varies from municipio to municipio; each has its titular fiesta, which is celebrated in one way or another by the entire community, each has a number of other fiestas, but in some towns these are left to the officials in charge, while in others they are seized upon as an excuse by many people to leave their work and take to liquor and dancing. On the lake, for example, Santa Catarina and San Juan are notoriously "religious" and their economic positions are certainly tied up with their propensities for this kind of celebration.

7. The political and religious officials have, in different municipios, various

social functions. In Atitlán, for example, the members of the religious organizations have charge of funerals; in Chichicastenango the Indian secular officials carry out purely customary law in the misbehavior of couples non-legally married. Each municipio has its own customs more or less different from those of its neighbors and the politico-religious organization of course fits local law, custom, ritual, and belief.

Each municipio has, thus, a different and relatively independent social organization. They are interrelated in the modern political whole, but the larger organization does not, of course, take into account the pre-Columbian political divisions, nor any language groupings that may at one time have been coincident with them.

In birth and baptismal customs, in modes of courtship and ceremonies of marriage, in types of family organization, in kinship systems, in religious and magical beliefs, in the use of and the rituals of shamans and sorcerers, in what remains of pre-Columbian rituals and the calendar—in almost every aspect of the culture—the municipios differ from one another in greater or less detail. Contiguous municipios perhaps differ from each other less than those far separated geographically; much more research is required to determine to what extent this may be true. But enough has been said about the municipios as self-conscious social and cultural independent groups to show that progress in the study of Guatemalan ethnology depends upon a prior recognition of the municipios as the primary (and possibly final) ethnic units in which it is involved.

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SUICIDE IN NORTHEASTERN CALIFORNIA

By ERMINIE W. VOEGELIN

MODERN inquiry into the subject of suicide is revealing a growing number of tribes in northeastern California in which suicide was practised. Thus, Kelly mentions two Surprise Valley Paiute cases of suicide,¹ Du Bois cites a recent instance for the Wintu,² and in my own experience during a summer's ethnographic survey work,³ I found either aboriginal or recent cases of suicide acknowledged for exactly half of the groups visited (northern foothill Nisenan, McCloud and Upper Sacramento Wintu, Hat Creek Atsugewi, western and eastern Achomawi, Modoc, Klamath), while the other groups visited explicitly denied the practise (western and eastern Shasta, Hayfork Wintu, mountain, foothill, and valley Maidu, northern and southern mountain Nisenan). Out of this total of sixteen groups, the two Wintu groups are notable in their claim to an old and elaborately patterned form of suicide. Somewhat less clearly delineated procedures prevailed among neighboring groups.

The data collected from each of the groups acknowledging suicide practises are given, group by group and with relevant comments appended, in the main section of this paper. In this section I have also quoted in full Kelly's valuable material on Surprise Valley Paiute forms of suicide.⁴ Discussion of the suicide pattern for northeastern California as a whole, and some general conclusions, are reserved for the final section of the paper.

WINTU

*Case 1, McCloud Wintu.*⁵ This case concerns a man who gambled steadily for

¹ Isabel T. Kelly, *Ethnography of the Surprise Valley Paiute* (University of California Publications in American Archaeology and Ethnology, Vol. 31, No. 3, 1932), p. 168.

² Cora Du Bois, *Wintu Ethnography* (University of California Publications in American Archaeology and Ethnology, Vol. 36, No. 1, 1935), p. 50.

³ Acknowledgments are due to Professor A. L. Kroeber for permission to publish material gathered incidental to obtaining ethnographic element lists while Research Assistant at the University of California in 1936, and to my teacher, Professor E. Sapir, who first stimulated my interest in the subject of suicide among primitive groups.

⁴ The Surprise Valley Paiute are the most northeasterly of the California groups, they extend into northwestern Nevada and south central Oregon. My own field work stopped short with the Achomawi, western neighbors of the Surprise Valley Paiute.

⁵ Informant, Jenny Curl. Mrs. Curl gave her age as 72, but there is reason to believe that she is probably around 60-65 years of age. She was born and has lived all her life near Baird, on the McCloud River. Her father was a McCloud Wintu, born on Pit River; her mother a McCloud Wintu, born on McCloud River. Both of her paternal and one of her maternal grandparents were McCloud Wintu; her mother's father was an Upper Trinity Wintu. Mrs. Curl

four or five days and lost continuously, finally he had lost everything he had, so he returned home.⁶ During the protracted period of gambling the man had not eaten or drunk anything; when he returned home he was worn with fatigue and hunger. He entered his house and sat down to rest, his wife was working, but said nothing to him when he arrived, and made no move to bring him food or water. The husband sat quietly for some time, then asked his wife to bring him a basket of water so that he might drink and wash his face and hands. His wife did not answer him and kept on with her work; when he repeated his request she told him to get his own water, since he seemed to enjoy being away from home so much. The man said nothing, but rose, took his quiver and bow and arrows and left the house without saying anything. He went to a sacred spot (sauel)⁷ on the McCloud River and stayed there four or five days, praying for power. At intervals he dove into the sacred pool: gradually, with his protracted fasting, he grew weaker and weaker. One morning he swam in the river and dove into the pool for the last time; he never came to the surface again. All that the people found when they searched for him were his bow and arrows, lying on the rock where he had left them, before he dove into the pool for the last time.

Case 2, McCloud Wintu. This also concerns a man who had been gambling for several days, and who had lost everything he owned. Early one morning this man returned to his home; his wife was outside the house cooking acorn mush. She took no notice of her husband, but busied herself in taking some hot stones out of the basket of mush and placing them on a piece of bark to cool. Her husband was hungry; he had eaten nothing all the time that he was gambling. Picking up a mush-coated stone, he scraped off some of the mush on it between his thumb and first finger, meaning to lick the mush off his fingers.⁸ His wife turned to him angrily and said, "Take your hand out of that soup, go and gamble. You don't want to eat; go and gamble and get a bellyful." The man set down the stone, wiped the mush off his fingers instead of licking them clean, but said nothing. He went into the house, took his quiver and bow and arrows, and left the house, weeping. His wife watched him leave, and told her half-grown son that he had better follow his father to see what the latter meant to do.⁹ The boy trailed his father; the latter went to a sacred spot on the river where a power lived. When he saw that his son was following him the father told the boy it was no use to do so, because he was never going to return home, but that the boy had better do so immediately. However the son told his father that he had been sent by his mother to watch him, and that he meant

is in possession of all of her faculties and impressed one as a reliable informant. She often volunteered data, and her experience as an informant for Drs Du Bois and Demetracopoulou had made her thoroughly sympathetic toward ethnographic field work.

⁶ Cf. Du Bois' remarks on the importance of gambling among the Wintu (*ibid.*, p. 43).

⁷ For description of sacred spots cf. *ibid.*, p. 79 ff.

⁸ Cf. *ibid.*, p. 19.

⁹ Cf. Du Bois' remarks on the watch members of the group kept over a person when the latter showed signs of mental instability (*ibid.*, p. 50).

to stay with him. The father sat down on a rock and began smoking a pipe, making a prayer to heaven as he gazed at the sun.¹⁰ Finally he stood up, took off the deer-skin he wore over his shoulder, and after depositing his bow and arrows on the rock where he had been sitting, went swimming in the river. Finally he dove under water into the sacred spot, and stayed down some time, then came to the surface and swam about before returning to sit on the boulder above the pool. For five times, at intervals during the day, he swam and dove into the sacred spot thus, coming up to the surface each time after diving.¹¹ Throughout the day he frequently ordered his son to return home, but the boy merely sat and watched his father as he alternately swam and sat resting on the boulder beside the pool, smoking and praying. Finally the man went into the water for the sixth time; the sun was low in the west by then. When he came out, the man told his son, "There is no use in your staying here any longer. I am not going to come out next time; when I dive in again, you won't see me any more. If I don't come out after a while, you go home before it gets dark; don't stay here waiting for me. I was going to raise you, to teach you to be a good man; not to fight and to talk to people.¹² Now I cannot. I wanted to eat, but I guess your mother doesn't want to cook for me; that in future she would not feed me. You must go home alone; I am not going back with you." The young boy was weeping. His father dove into the sacred pool three more times, and after the third time again told the boy to leave because the next time he dove in he would not come to the surface again.

Finally the father dove and did not reappear; the boy stayed at the sacred spot for some time, watching the pool, then ran home, crying. When his mother saw him she asked, "Where is your father?" The boy told her what had happened; then all the men went to the spot on the river where the father had disappeared, and dove into the pool, but none of them could reach the bottom of the pool,¹³ so the man's body was never recovered.

Case 3, McCloud Wintu. This case concerns a man who called for an evil spirit¹⁴ in the earth lodge (Lut)¹⁵ during a shamanistic initiation dance;¹⁶ two or three nights after he had called for this evil spirit it came to him; the man then lay in the dance house for four or five days. At the end of that time the officiating shaman told all the seekers to leave the dance house and swim, and then return to their

¹⁰ Cf. *ibid.* , p. 73 ff., for discussion of prayers to the sun

¹¹ Cf. *ibid.* , pp. 80-82, for the procedure followed when visiting sacred spots.

¹² Cf. *ibid.* , p. 48, for the type of lectures delivered to boys.

¹³ Cf. *ibid.* , p. 80, for the difficulty sometimes encountered when diving in sacred pools

¹⁴ Described as a "sucker-like spirit, but not a real sucker [fish]"

¹⁵ See *ibid.* , p. 122, section on earth lodge. Mrs. Curl referred to the old type of earth lodge as Lut, but recognized that "the upriver people called it *olterestut* [the term Du Bois gives], because it was made with the *olterest* tree, something like a fir-tree" (white fir, according to my Upper Sacramento Wintu informant).

¹⁶ See *ibid.* , p. 89 ff., for an account of the shaman's initiation dance. Mrs. Curl's references to the procedure followed during an initiation ceremony differ in some respects from Du Bois' account of this ceremony.

homes and eat. But the man who had called on the evil spirit did not do this; he lay and sang in the dance house for about a week, not talking to anyone. After a week or so his wife came to the door of the dance house¹⁷ and asked him loudly why he didn't get up and eat; "You must have a 'mean' power, because you didn't want to wash your face and eat," the woman told her husband.

The next morning about sunrise the man went out of the dance house; he went off some distance and danced. People followed him and tried to persuade him to return home and eat. But he said, "No; my woman told me I must be a strong doctor, leave me alone; I must have an evil spirit."

For ten days people offered the man food, and entreated him to eat. They followed him about wherever he went,¹⁸ dancing in the woods. But the man refused to eat; he told the people that his wife had said that he must have obtained a strong spirit. Finally he climbed up on a digger pine and sang and danced out on the limbs of the tree for six days. The people kept watch over him, but they could not persuade him to come down from the tree. Then shortly before midday one day he dropped down dead, starved; he was all skin and bones. (My informant added, by way of comment, "That's the way some doctors died; they called on this spirit. People didn't like to call on it. But this man died because his wife talked loud and insulted him.")

Case 4, McCloud Wintu This case is said to have occurred "a long time ago." It concerns a man whose wife had insulted him. (My informant commented, "She may have insulted him because she was jealous of him; perhaps he had been going with another woman.") The man took his bow and arrow and went to a sacred spot on the river; when he arrived at the spot he dove in once, then later two or three times more, then again. He was seeking to pick up something on the bottom.¹⁹ But he failed to find anything, so he got out of the water and lay down by the fire; he wanted to sleep and dream. However he could not sleep because he was so angry. Before daylight he slept a little, but did not dream.²⁰ He woke up, and started the fire at daylight; by sunup he got up and dove in the water again. When he came out of the water, he lay down on his side, he napped, but had no dreams. So he kept sitting around, thinking, praying, smoking, he kept this up all day, going into the water at intervals and diving around on the bottom. He ate nothing all day.

That night he dove into the water again, four or five times, feeling around on the bottom. At the last dive he found a small hole under a rock, but obtained nothing from the bottom. So he came out of the water and warmed himself by the fire, lying on his side, resting his head on the palm of his upturned hand. This time he fell asleep and dreamt, he saw a black crow in his dream. The crow lit near the man's

¹⁷ Women were ordinarily debarred from entering the earth lodge, as Du Bois (*ibid*, p. 122) points out

¹⁸ See footnote 9, present paper

¹⁹ Any sort of charm (*xosi*), see *ibid*, p. 82 ff

²⁰ Cf. Du Bois' remarks on the significance of dreams during visits to sacred spots (*ibid*, p. 89)

foot and scratched the man's ankle with his claw. Then the man woke; it was nearly daylight. He pondered over his dream; "I wonder why a crow scratched my ankle?"

He went home early, about the middle of the morning. All his female relatives had acorn mush and salmon ready for him to eat (after his fast for power), but his wife had prepared nothing for him. The man's uncle was there talking to the mountains, praying for the man. All the people told him to come and eat, that he must eat now.²¹ But he refused to eat; he said, "Eat, you folks; I guess my wife doesn't want to see me eat; go on, you eat."

His mother and father coaxed him; they said that everyone ate after they came out of a [sacred] spring. His father told him, "You're young; you can go and get another woman; you shouldn't feel badly because this woman has treated you this way. You can take another woman; but now you must eat, my son."

But the man refused to eat; he went to his father's house and stayed there two days and nights, not eating anything.

Then people came from the south and told the people there to come south; that they were going to have a war dance. The man whose wife had insulted him wanted to go; he had an elkskin robe and cap,²² and a fisherskin quiver.²³ He took these out and left his father's house in the morning and swam in the water. The women told him to eat, and made a lunch of acorn bread²⁴ for him to take; but he would not eat. He was singing all the time.

The party of men he was with camped four or five times; still he would not eat. When the party arrived at their destination there was a big fight. The fighting went on, back and forth,²⁵ finally, the man's father and brother caught him, because the man was very nearly out of arrows. They advised him to return home.

"No, I'm not going back; I've come down here to die. You go back," the man said. Then he returned to the fight. Finally he was shot by the enemy in the ankle, where the crow had scratched him. His father said, "You're shot; you'd better go back." "No, I came to die," his son replied. So he let the enemy capture him, and kill him, and take his elkhide armor and fisher quiver.²⁶

Case 5, McCloud Wintu. This is a dubious case of possible suicide on the part of a young woman. After dancing all night with a grizzly bearskin,²⁷ a group of young women went up to a sacred place the next morning, as they had been told to do. This sacred place was a big cave, under a rock, the cave was dark, and had a narrow ledge on one side. When the young women came out of the cave, one of the girls was missing, the young women went to the entrance of the cave and called, but there was no answer. They concluded the missing girl had fallen into the hole, and

²¹ Cf. Du Bois' account of the procedure followed after visiting a sacred pool (*ibid.*, p. 81).

²² See *ibid.*, pp. 39, 124.

²³ See *ibid.*, p. 125.

²⁴ See *ibid.*, p. 19.

²⁵ Cf. *ibid.*, p. 39. Mrs. Curl is evidently referring to a formal combat.

²⁶ See *ibid.*, p. 38, for mention of valuables taken from fallen foes.

²⁷ See *ibid.*, p. 11, for description of this dance.

returned to the village and told the people (My informant commented, "Probably the girl didn't fall into the hole on purpose, but her parents said she must have been jealous over a man, that was what they thought.") The men from the village went to the cave and threw rocks into the hole; they thought there was a river at the bottom. Twenty years or so ago, two white men went down into the cave with a light, and found the bottom to be dry. They found the girl's bones lying there in a heap. When they came out they told EDC Campbell²¹ about it; she said that she had heard of a girl falling into the cave, so they buried the bones.

Wintu, Upper Sacramento group. My informant from the Upper Sacramento group of Wintu²² confirmed the first four of the five cases of suicide detailed above, and volunteered the interesting comment that any blame for the act of suicide attached itself to the wives of the suicides, rather than to the men themselves. The death of the young woman, detailed in Case 5, my Upper Sacramento informant said was entirely accidental, but his knowledge of this case was apparently limited, he asserted that the girl's bones had never been found, as it was impossible to descend to the bottom of the cave.

Both the McCloud and Upper Sacramento Wintu informants gave much the same information as noted by Du Bois concerning the recent case of suicide occurring among the Wintu. Both informants also agreed that treatment of the corpse of a suicide was the same as that for persons dying a natural death, provided of course that the body was recoverable. Only the parents of the deceased cried for the dead, however, at the burial of a suicide.

It is interesting to note, in connection with the discussion which follows, that when first questioned on the topic of suicide both McCloud and Upper Sacramento Wintu informants denied that the practice prevailed in aboriginal times. It was only in connection with another subject, and several hours after I had asked about suicide, that my McCloud informant related the first of the cases given above; when she realized my interest in the case she gave the other cases in the same succession in which I have presented them. When I went on to my next informant, among the Upper Sacramento Wintu, I again met with a point blank denial of any cases of aboriginal suicide, but when I briefly outlined the McCloud data this informant nodded immediate agreement and remarked, "Yes, that was what people used to do." There was no hint in his manner that he equated this behavior with suicide as it prevails today.

²¹ A Wintu half breed woman (according to Mrs. Curl) who is still living. See *ibid.*, p. 101. The cave referred to is, I believe, Potter's (?) cave.

²² Informant, John Towndolly, age 77. Towndolly's father was an Upper Trinity Wintu, his mother an Achomawi (Madesi group). Towndolly was born on the Sacramento River, in Shasta County, when about 4 years old he was taken to Trinity Center and lived there until 16 or 17 years old, then he moved to Dunsmuir, on the upper Sacramento, and has lived there for the past 60 years. The information he gave relates to the Upper Sacramento Wintu, he disclaimed any extended knowledge of Upper Trinity ethnography. Towndolly proved an excellent informant.

From her own ethnographic research among the Wintu, Du Bois feels that "Suicide was an unknown pattern in Wintu society. Only one was reported by informants. A Wintu under death sentence for the murder of a boy hanged himself in his cell."³⁰ The new material which I have presented belies, in my opinion, any such broad negation as Du Bois offers. On the other hand, this material is ambiguous in one respect. Are the McCloud cases Nos. 1-4 mythical, rather than historical, in character?

Fortunately we have an excellent collection of Wintu myths by Du Bois and Demetracopoulou³¹ to use as a basis for discussion of this point. In the myth collection there are two narratives which contain material strikingly similar at many points to the first four cases given by my McCloud informant. One of these Du Bois-Demetracopoulou narratives is purely mythical, the other anecdotal. The myth, which was recorded by Du Bois from Syke Mitchell, a McCloud Wintu informant, begins as follows:

Dentalium had been gambling. He gambled away all that he had. He was gone some time. He came home hungry and asked his wife for food. His wife answered "I haven't anything to eat. Why don't you eat what you gambled away?" This made him angry and he went north.³²

From this point resemblances to my material cease; Dentalium takes Coyote north with him, they undergo a series of adventures, die and come to life. There is no mention of either committing suicide.

The narrative of an anecdotal nature given by Du Bois and Demetracopoulou is titled *The Gambler*, and was recorded by Du Bois from Sara Fan, a Bald Hills Wintu informant. It concerns a famous gambler who lived near Ono. This man lost all he had while gambling; when he returned home his wife scolded him and refused to let him eat some of the seed meal she was grinding; the gambler left, and would not heed the people's entreaties to return. "My wife hurt my feelings and I am going away," he told the people. The next fall he reappeared in an emaciated condition and told the people that he had gone in underground at Mount Lassen and had come out at the Yolla Bully sacred place. The tale concludes, "Then he disappeared and was never seen again."³³ There is no mention of his committing suicide.

Demetracopoulou's comparative notes for *The Gambler* contain a reference to the distribution of the Underground Passage motif among tribes

³⁰ *Ibid*, p. 50

³¹ Cora Du Bois and Dorothy Demetracopoulou, *Wintu Myths* (University of California Publications in American Archaeology and Ethnology, Vol. 28, No. 5, 1931)

³² *Ibid*, p. 390.

³³ *Ibid*, p. 375.

neighboring to the Wintu, and a reference to the similarity of openings between The Gambler, and the Dentalium and Coyote myth. The lack of any further comparative material for The Gambler accords with inquiries I made for any such material among the Klamath, Modoc, Maidu, and Nisenan, groups which I visited after securing the Wintu data.

From a consideration of Du Bois and Demetracopoulou's myth material I see no convincing reason for believing that my four accounts of Wintu suicide are merely a heretofore unrecorded combination of more widely distributed motif elements. It is true that informants could not give the personal names of any of the principals concerned in the four accounts, but neither could my McCloud Wintu informant give the name of the young woman in the case of dubious suicide, although this case is definitely linked with the present. It is also true that my four accounts, plus The Gambler narrative in Du Bois and Demetracopoulou, show a marked tendency toward stylization—so marked in fact that one hesitates to accept them as purely factual recitals. However, the accounts themselves consistently reflect so many aspects of Wintu religious and social life, as I have tried to indicate briefly in footnotes, and so consistently lack references to animal actors, that they seem to pass out of the domain of mythology and to belong rather to a shadowy, but nevertheless actual, past phase of what is now a badly disintegrated culture. One fact emerges with satisfactory clarity; the concept of suicide was not unknown to the Wintu. How often this concept was actualized, under what circumstances, among how many Wintu groups are some of the questions connected with this topic which must await further field research.

ATSUGEWI-ACHOMAWI

Two cases of suicide were reported by my Hat Creek Atsugewi informant;²⁴ the pre-white case concerned a woman, who jumped off a cliff with her child because she had lost her husband. The more recent case had to do with a youth who shot himself with a gun because his mother "cussed" him. My informant had never heard of any cases of suicide which were motivated by quarrelling with one's mate or jealousy; she denied that men or women had ever hanged or drowned themselves, or had eaten wild parsnip in order to take their lives. Among the Atsugewi the corpse of a suicide was buried in the same manner as were the bodies of persons dying natural deaths. Cases of suicide were said to have been rare, and the act to have been disapproved by the community.

²⁴ Informant, Julie Bob, age 79. Mrs Bob's father was a Hat Creek Atsuge, her mother an Achomawi (Maidu group). Mrs Bob had been born and lived all her life on Hat Creek, near Cassel, Calif. She was by no means an ideal informant, and is definitely considered "queer" by Indians who know her, but for a gossipy topic like suicide I believe her data are reliable.

Among the Achomawi proper,³⁵ suicide was reported as having been common in pre-white days. My informant said there had been a "good many" cases formerly among both men and women, arising chiefly from disappointment in love and from jealousy. In recent times a mother's whipping her child caused the latter to kill himself by shooting with a gun. During pre-white times men and women drowned themselves or ate wild parsnip in order to commit suicide; death by hanging was not resorted to. The corpse was buried, following the usual procedure for disposal of the dead; however, only the parents of the suicide wailed. Suicide was disapproved by the community. Further investigation of suicide among this group of Achomawi might yield valuable data as to pattern; the culture of this group is less disintegrated than is Wintu culture, for example.

For the Hammawi, an eastern Achomawi group,³⁶ suicide was denied as a pre-white custom, but two recent cases were recalled. One was of a man who committed suicide, being motivated to the act because of quarrels with his wife; he took his life by eating the root of wild parsnip. A woman hanged herself. The corpse of a suicide is treated in the same manner as that of a person dying a natural death, but only the parents of the deceased cry. Death by drowning or shooting with a gun were denied. Cases of suicide were said to be rare, and the act disapproved by the community.

SURPRISE VALLEY PAIUTE

Kelly says of the Surprise Valley Paiute:

Suicide was rare; the accepted mode seems to have been by eating the roots of wild parsnip. A person might kill himself if rebuked. Sometimes a deserted or abused wife took her own life. One case of this came to my notice. There is also an instance of a young girl's committing suicide because of continual quarrelling between her mother and stepfather. Suicides received the same burial as others.³⁷

MODOC

One specific case of suicide was recalled;³⁸ a Modoc, Charlie Hood, shot himself

³⁵ Informant, Ben Bridge (or Bainbridge, as his hunting license reads), age ca. 70. Ben Bridge's father belonged to the Achomawi proper, his mother to the Ilmawi group of Achomawi. He was born in Round Valley, Mendocino County, but was brought to Glenburn, in Achomawi territory, when 2-3 years old, and has lived near Glenburn ever since except for trips made into the upper Sacramento Valley in the fall to pick prunes and olives. He proved a very good informant, being intelligent and interested, there were a few topics however which he did not enjoy discussing with a woman and suicide was, unfortunately, one of them.

³⁶ Informant, Sam Fox, age 74. Sam Fox's father was a Hammawi Achomawi, born in West's Valley; his mother a Kosalektawi Achomawi, born at Sugar Hill, Sam Fox was born in West's Valley and has lived there and at Alturas all his life. He is in poor health, but was a good informant and extremely cooperative.

³⁷ Kelly, p. 168.

³⁸ Informants, Jennie Clinton, age 78, and Dolly Lawver, age ca. 65. Both informants are full bloods, and claimed affiliation with the Paskanwas group of Modoc. They were born at Tule Lake, Calif., and lived there until removed to Oklahoma after the Modoc War (1873). Mrs. Clinton returned to Oregon in 1903; Mrs. Lawver in 1926. Mrs. Lawver was willing but her knowledge was limited, Mrs. Clinton was better informed.

with a gun, "because he was in love with a girl and didn't want to go to Oklahoma³⁹ and leave her." The Modoc were on their way to Oklahoma when the suicide occurred, so they buried Hood's body, instead of cremating it, as would have been customary. In connection with a discussion of marriage another reason for committing suicide, under aboriginal conditions, was mentioned. A man might kill an adulterous wife, because he was jealous of her, and then kill himself, "because he loved his wife so much." Unfortunately no accounts of any specific cases of this sort were secured.

As contrasted with death in war, or accidental death, suicide was not considered "respectable" or "honorable." No cases of suicide by drowning, hanging, or eating wild parsnip were remembered.

KLAMATH

A Klamath informant⁴⁰ recalled two post-white cases of suicide, both of which arose from disappointment in love. One concerned a woman who hung herself, the other a man who drowned himself. Eating wild parsnip was denied as a method of committing suicide. My information agrees with Spier's regarding disposal of the bodies;⁴¹ the corpse was cremated, as was usual also for persons dying natural deaths. At the cremation of a suicide all adult members of the community wailed for the dead. Suicide was disapproved by the community.

One of Spier's informants stated that a girl, married against her will, might hang herself,⁴² another of his informants knew nothing of such a practise. My informant's discussion of child betrothal indicates he also was ignorant of this matter. Dr Spier informs me that both of his references to suicide (cremation and marriage) are to aboriginal conditions.

NISENAN

The only information from Nisenan and Maidu groups concerns a recent case in which a northern foothill Nisenan man, married to a heavy drinking, quarrelsome wife, shot himself with a gun.⁴³ No instances of suicide by hanging, drowning, or eating wild parsnip were remembered by Nisenan-Maidu informants.

SHASTA, HAYFORK WINTU, MAIDU

For the two Shasta groups visited, for one Wintu groups (Hayfork) and for all

³⁹ See footnote 38, present paper.

⁴⁰ Informant, Antone Marrett, age 75 (?) Marrett's father was a Klamath of the Klamath Marsh subgroup, his mother was quarter Klamath, quarter Achomawi, half Modoc. Marrett was born at Klamath Marsh, he spent his boyhood there and has lived on the Klamath reservation all his life. Although blind, he was in many respects very satisfactory as an informant.

⁴¹ Leshe Spier, *Klamath Ethnography* (University of California Publications in American Archaeology and Ethnology, Vol. 30, 1930), p. 71

⁴² *Ibid.*, p. 45

⁴³ Informant, Lucy Thompson, age 86, of Stanfield Hill, Calif. Mrs. Thompson was born and has lived all her life at Stanfield Hill; her parents both belonged to the same vicinity. She was a good informant, very reliable.

Maidu groups except the northern foothill Nisenan, informants⁴⁴ did not recall any instances of Indians committing suicide, either in aboriginal or in recent times. It is of course possible that the practise existed; negatives are here apt to be untrustworthy evidence, and it remains a moot point whether or not these groups practised any form of suicide

SUICIDAL ATTEMPTS

Attempts on the part of a widow to commit suicide as the body of her deceased husband was being cremated or buried, were not uncommon throughout northeastern California. Among the Modoc, Hammawi Achomawi, northern foothill and mountain Nisenan, all of which groups regularly practised cremation, informants stated that widows frequently tried to jump on the burning pyre and had to be restrained. Among the Nisenan three old women were specially appointed to watch the widow during a cremation, in order to restrain her should she make such an attempt. My Klamath informant did not know whether widows ever tried to jump on the pyre.

Among groups regularly practising burial, the Atsugewi, Achomawi proper, three Wintu groups, and the foothill Maidu reported that widows sometimes tried to jump in the grave, to be buried with the deceased, but were restrained from doing so. The practise was denied for the western Shasta and Valley Maidu, who likewise buried; my informant for the eastern Shasta was uncertain regarding this matter. No instances of widows ever succeeding in any of these suicidal attempts were obtained.

In his description of Shasta girls' puberty customs, Dixon states that should a girl dream evil dreams during her puberty rites, she would have to confess such dreams and would, at the end of the puberty observances, be burnt alive.⁴⁵ Her confession would seal her own death warrant, presumably; the fact that she did confess might then be viewed as an indirect way of committing suicide. However, as Kroeber several years ago surmised,⁴⁶ there seems to be little evidence to support Dixon's statement that girls were ever actually burned. Sargent Sambo, my western Shasta informant (whom Dixon also mentions by name as having been his best informant for the Shasta)⁴⁷ laughed when this section of Dixon's monograph was read to him and explained that a girl, in case she had dreamt bad dreams during

⁴⁴ Informants. Western Shasta, Sargent Sambo, age 72, of Horse Creek, Calif.; Eastern Shasta, Emma Snelling, age 67, of Yreka, Calif.; Hayfork Wintu, William ("Whiskey Bill") George, age ca. 70, of Hayfork, Calif.; mountain Maidu, Dick Smith, age 73, of Indian Valley near Greenville, Calif.; foothill Maidu, Kitty Williams, age ca. 95, of Cherokee, Calif.; northern mountain Nisenan, Polly Ann Hamburg, age ca. 65-70, of Nevada City, Calif. (but born and raised at Chicago Park, Calif.); southern mountain Nisenan, Susie Kessler, age 65, of Placerville, Calif.; valley Maidu, Amanda Wilson, age ca. 75, of Chico, Calif.

⁴⁵ Roland B. Dixon, *The Shasta* (Bulletin, American Museum of Natural History, Vol. 17, 1907), p. 458.

⁴⁶ A. L. Kroeber, *Handbook of the Indians of California* (Bulletin, Bureau of American Ethnology, 78, 1925), p. 300.

⁴⁷ Dixon, *ibid*, p. 383.

her puberty ceremonies, was decked out with a few beads and made to pass through a fire. This was done in order to burn the evil effects of the dreams off the girl, but she was never burnt alive.

CONCLUSIONS

Assuming from Kelly's data that suicide is an old practise among the Surprise Valley Paiute, we find that there is a practically continuous line of distribution for the aboriginal occurrence of suicide procedures from the Surprise Valley Paiute westward through the Achomawi proper and the Hat Creek Atsugewi, to the McCloud and Upper Sacramento Wintu. To the north among the Modoc and Klamath suicide was also practised under aboriginal conditions. As regards the Hammawi Achomawi who are situated between the Surprise Valley Paiute and the Achomawi proper, and who disclaim aboriginal suicide practises, the negative statement of a single informant cannot be taken as the final word on the subject, especially when this informant admits to a recent case of suicide being accomplished by eating wild parsnip root, which was elsewhere an aboriginal procedure.

In the cluster of groups mentioned above, three disparate suicide patterns are encountered. Of these three patterns that of the Wintu has already been discussed in detail. As regards the second pattern, found among the Klamath and Modoc, we lack at present many specific details, but at least one notable fact emerges from our various references to suicide in these two groups. For both the Klamath and Modoc suicide is a romantic gesture, motivated by disappointments in love and, indirectly, jealousy. Women hang themselves, men in some instances drown themselves.

The third pattern, found among the Atsugewi, Achomawi, and Surprise Valley Paiute, may be briefly summarized as follows. Suicide was usually motivated either through jealousy or quarreling; eating wild parsnip root was one of the more generally accepted modes of committing suicide; the bodies of suicides were accorded the same disposal as the bodies of persons dying natural deaths, but only close relatives wailed; suicide was regarded with disapproval, and among most of the groups occurred only rarely. If we were seeking for a more extended areal distribution of this latter, or characteristically northeastern California suicide pattern, we would first of all turn eastward to the Great Basin tribes of Nevada, since among the Modoc and the Klamath to the north there is a definite change in pattern, among the Wintu to the west the pattern is also of a different order, while among the Maidu-speaking people to the south all suicide practises are consistently denied.

GRELNCastle, INDIANA

INTRODUCTION

THIS article, dealing primarily with the results of archaeological expeditions during 1935-1936, supplements the two previous reports in the *AMERICAN ANTHROPOLOGIST*.¹ In compiling these data special efforts were made to secure the more recent results obtained by expeditions mentioned in our previous article and also to procure information from new areas, particularly Siberia. The general arrangement of the material has been treated geographically under the headings Georgia and Abkhazia, Azerbaidzhan, Armenia, Daghestan, North Caucasus, Crimea and Black Sea Region, Ukraine, European Russia, Turkestan, and Siberia. In order to facilitate the location of sites the former as well as the modern names have been added. The maps, drawn at Field Museum by Richard A. Martin and published in our previous report, can also be utilized.

Soviet literature in the libraries of Field Museum of Natural History and the Oriental Institute of the University of Chicago were examined by Eugene Prostov, who selected many passages for inclusion and supervised the transliterations.² V. N. Rimskey-Korsakoff selected material from the Soviet publications in the library of the Peabody Museum of Harvard University. In August, 1936, A. M. Tallgren³ published two important articles in *Eurasia Septentrionalis Antiqua*. Since the wealth of information therein contained is in English it is available to every student and no extracts have here been incorporated.

The following abbreviations have been used:

ANU	<i>Akademiia Nauk Ukraini</i> (Ukrainian Academy of Sciences, Kiev, formerly VUAN)
ESA	<i>Eurasia Septentrionalis Antiqua</i> : journal for East European and North-Asiatic Archaeology and Ethnography published by Archaeological Society of Finland, Helsinki
GAIMK	<i>Gosudarstvennaia Akademiia Istorii Material'noi Kul'tury</i> (State Academy for the History of Material Culture, Leningrad)

¹ E. Golomshtok, *Anthropological Activities in Soviet Russia* (Vol. 35, pp. 301-27, 1933), Henry Field and Eugene Prostov, *Recent Archaeological Investigations in the Soviet Union* (Vol. 38, pp. 260-90, 1936).

² The Congressional Library system of transliteration, with minor modifications, was used for all proper nouns in Russian. The U. S. Geographic Board spelling was retained for the generally known geographic names.

³ A. M. Tallgren, *Archaeological Studies in Soviet Russia* (ESA, Vol. 10, pp. 129-70); *Problems Concerning the Central-Russian Gorodishche Civilization* (ESA, Vol. 10, pp. 171-85).

IAE	<i>Institut Antropologii i Etnografii</i> (Institute of Anthropology and Ethnography of the State Academy of Sciences, Leningrad)
MOGAIMK	<i>Moskovskoe otdelenie Gosudarstvennoi Akademii Istории Material'noi Kul'tury</i> (Moscow Department of the State Academy for the History of Material Culture)
Sovetskaia Ethnografiia	Soviet Ethnography: a bi-monthly journal published in Leningrad by the State Academy of Sciences in cooperation with the Russian [R. S. F. S. R.] Commissariat of Education in Moscow
UZKOM- STARIS	<i>Uzbekistsanskii Komitet po Okhrane Pamiatnikov Stariny i Iskusstva</i> (Uzbekistan Committee for the Preservation of Monuments of Antiquity and Art), currently known as <i>Uzbekistsanskii Komitet po Okhrane i Izucheniiu Pamiatnikov Material'noi Kul'tury</i> (Uzbekistan Committee for the Preservation and Study of Monuments of Material Culture, Tashkent)
VOKS	<i>Vsesoyuznoe Obschestvo Kul'turnykh Snoshenii</i> (All-Union Society for Cultural Relations with Foreign Countries)

The two institutions IAE and GAIMK conduct most of the archaeological research within the Soviet Union and for this reason their reports and publications have been the main sources of information. For example, during 1935 GAIMK⁴ organized seventeen expeditions which were conducted by ninety-three members of its staff. In addition to its own expeditions GAIMK cooperated with the following: (1) the Altai Expedition of IAE, (2) the Lower Volga Expedition of IAE; (3) the Sukhum Expedition of IAE; (4) the Tripolie Expedition of ANU.

During the past two years the staff of IAE⁵ has investigated numerous anthropological problems throughout the Soviet Union. The Institute is preparing for publication a reference handbook⁶ which will include brief data on the distribution, statistics, history, ethnography, and socialization of the population. The volume dealing with Central Asia is ready for printing, and materials have been collected for the northern Asiatic and European sections of the U.S.S.R., and, to some extent, for the Caucasus.

The most important project of the year, which dealt with the "genesis and development of the primitive communist society," included fifty theses on the prehistoric development of society based on archaeological data,

⁴ *Gosudarstvennaia Akademiia Istории Material'noi Kul'tury imeni N. I. Marr, Kratkii otchet o rabote v 1935 g.* [Brief Report of the Work of GAIMK in 1935] (Leningrad, 1936)

⁵ Cf. K. V. Viatkina, *Rabota Instituta Antropologii, Arkheologii i Etnografii L. V. S. S. S. R. za 1935 g.* [The Work of IAE of the Academy of Sciences of the U. S. S. R. in 1935] (Sovetskaia Etnografiia, 1936, No. 2, pp. 139-41).

⁶ *Narody S. S. S. R.* [The Peoples of the U. S. S. R.].

the history of prehistoric economics, the history of marriage and the family, the origin of the tribe, of primitive religion, and of art. Paleolithic specimens from the Caucasus and materials dealing with the ancient history of metallurgy in the U.S.S.R., etc., were studied in connection with the project.

Among the 1935 publications of the Archaeological Section of IAE not referred to in the footnotes are:

1. A. Gorodtsov, *Timonovskaia paleoliticheskaia stoianka* [Timonovo Paleolithic Station] (Trudy IAE, No. 3), embodying the results of the 1933 excavations.

2. D. N. Lev, *K'istorii gornogo dela* [A Contribution to the History of Mining] (Trudy IAE, No. 2).

The following are in course of preparation:

1. V. A. Gorodtsov, *Bronzovyi vek na territorii S.S.S.R.* [Bronze Age in the Territory of the U.S.S.R.].

2. P. P. Efimenko, a work on the burial grounds of eastern Finns.

3. N. I. Berezin, a comprehensive survey of archaeological monuments bearing on the history of the domestication of animals.

4. S. N. Bykovskii, a history of the origins of animal husbandry.

Among 1936 publications of IAE are the following:

1. L. A. Dintsès, *Russkaia Glinianaia Igrushka* [Russian Clay Toys] (French resumé: Trudy IAE, Vol. 12, No. 2, Arkheologicheskaiia seriia, No. 3, 1936).

2. N. A. Kisliakov, *Sledy pervobytnogo Kommunizma u gornykh Tadzhikov Vakhio-bolo* (*Vascijo Bolo*) [Signs of Primitive Communism among Mountain Tadzhiks of Vakhio-bolo] (Trudy IAE, Vol. 10, Etnograficheskaiia seriia, No. 2, 1936).

At the present time the Institute, enriched by the transfer of specimens from the Geological Museum of the Academy of Sciences, from the Russian Museum, and from the Hermitage, etc., has in its collections three-quarters of all the Paleolithic materials available in the museums. A comprehensive exposition of the Paleolithic periods is being prepared for exhibition in the IAE Museum.

GEORGIA AND ABKHAZIA⁷

1. The Abkhazian Expedition of IAE (S. N. Zamiatnin, leader) con-

⁷ The results of earlier explorations in Abkhazia (A. I. Lukin, V. I. Strazhev, M. M. Ivashchenko, etc.) have been published by M. M. Ivashchenko, *Issledovanie arkhaiskikh pamiatnikov material'noi kul'tury v Abkhazii* [Investigation of Archaic Monuments of Material Culture in Abkhazia] (Izvestia Nauchno-Issledovatel'skogo Instituta Kavkazovedeniia [Caucasian Research Institute of the Transcaucasian Branch of Akademii Nauk], No. 3, 1935). The antiquities include dolmens with bronze objects of more archaic type than those of the Koban culture, comparable with those from the North Caucasus dated by A. V. Schmidt (ESA, Vol. 4, pp. 9-21) between 2300-1600 B.C., urn burials, with Koban type of bronze inventory, associated with Greek and Hellenistic ceramics (sixth to second centuries B.C.), and stone circles of unknown date, locally known as "Dwarfs' fences" (*Atsan-guara*).

tinued the archaeological survey begun in 1934. A geological expedition of the Academy of Sciences, under the leadership of Mirchenko, Gromov, and Paretskiĭ, cooperated. According to Zamiatnin,⁸ as a result of three months' work about twenty Paleolithic sites were discovered, representing mainly Acheulian, Clactonian, Levalloisian, and Mousterian types, while Upper Paleolithic implements were also excavated.

Listed below are some of the new sites:⁹

Acheulian

- a. Byrts, southeast of Yasktukh Mountain
- b. Gvard, southeast of Byrts
- c. Apiancha Mountain, near Tsebelda, east of Sukhum
- d. Yagish Mountain, on left bank of Madzharkl River

Mousterian

- a. Akhabiuk near Mikhailovskoe, north of Sukhum
- b. Kelasuri on Kelasuri River, five kilometers southeast of Sukhum
- c. Esheri, on right bank of Gumista River, seven kilometers west of Sukhum
- d. Okum, near railroad, seven kilometers southeast of Ochemchiri
- e. Chuburishindzhi, twelve kilometers southeast of Gali near Sandzhio Mountain

In addition Lichkop, two kilometers north of Sukhum, contained Mousterian and Upper Paleolithic deposits. At Gali there were Acheulian, Mousterian, and Upper Paleolithic levels. The stations of Achikhvari, north of Ochemchiri, Kolkhida, five kilometers southeast of Gagry, and Barmish, fifteen kilometers east of Kolkhida, contained undetermined Paleolithic deposits. In many cases it was possible to establish the connection between the Paleolithic implements of various periods and the corresponding sea terraces. At several sites, for the first time in the U.S.S.R., Lower Paleolithic *coups-de-poing* were found.

2. The Sukhum Expedition of GAIMK (A. A. Jessen, leader) discovered remains of Medieval settlements, fortifications, and burial grounds in the valleys of eastern and western Gumista. Two Bronze Age sites near Sukhum and Ochemchiri yielded textile-impressed pottery. In 1936 an expedition from Tiflis (Museum of Georgia ?) explored the remains of ancient settlements, discovered by GAIMK in 1935, which are to be demolished during construction of the port of Ochemchiri. Regions embracing the villages of Esheri and Yashtukh, near Azanta and around Lake Amtkel, were examined.

⁸ In a letter dated June 28, 1936

⁹ N. I. Berezin, *Spravochnik po paleolitu S.S.S.R.* [Handbook of U.S.S.R. Paleolithic Periods] (Trudy IAE, Vol. 12, No. 1, Leningrad, 1936), p. 3.

At the invitation of the Georgian Committee for the Preservation of Monuments, a GAIMK expedition also explored the Colchian lowlands, where were found seventeen monuments representing remains of settlements from the time of the first appearance of metals down to the Roman period. A plan for further exploration of that region was formulated and submitted to the Committee.

3. The Terek Expedition of GAIMK (A. P. Kruglov, leader) completed the exploration of the Terek Valley and continued in the area between Kazbek village and the confluence of the rivers Terek and Arm-Khi, including the ancient cemetery near Chmi. Fifty sites were studied, bringing the total since 1934 to about two hundred.

A Medieval settlement, seventeen fortifications, fifteen burial groups, consisting of vaults and stone boxes, fifteen religious monuments including ten churches and five non-Christian sanctuaries, dating from the sixth to the fifteenth centuries, were investigated. Part of the Darial fortifications yielded dwellings built of roughly hewn stone, as well as many sherds, glass vessels, and animal bones. The greater number of the finds belonged to the ninth or tenth century. The interrelation of ancient fortifications nearby was also studied.

4. An expedition sponsored by the Georgian Geographical Society¹⁰ made ethnographical collections and explored the ancient monuments in the western part of the Caucasus range. The route of the expedition included the Ardon Valley, the Klot Pass, the Urtskh Basin, the Shtuluvtsekh Pass, the upper portion of the Cherekha River, the Sharivtsekh Pass, the Zagorskiĭ Pass, Ingur, the tributaries of the Galizga River, and Sukhum.

AZERBAIDZHAN

During 1935 an expedition¹¹ from the Azerbaidzhan Branch of the Academy of Sciences studied the ethnology of the former nomads, the Talyshs, Orans, and the Alars. Ancient burial monuments, "Shikh Zakeria" and "Dzhabir," near Larik village were investigated.

Mountain Karabakh Autonomous Area. A summary of earlier explorations, together with an account of the 1924 prospecting expedition sponsored by the Transcaucasian Orientological Association has been published by S. Ter-Avetisian.¹² The archaeological exploration of this area, once

¹⁰ Headquarters in University of Georgia, Tiflis. There is a fine library and map collection under the care of A. Djawachi-chwili (H F).

¹¹ I. M. Lekontsev, *Obzor etnograficheskikh rabot po Sovetskoiu Soiuзу za 1935 g. po gazetnym soobshcheniiam* [Survey of Ethnographical Studies in the Soviet Union] (Sovetskaiia Etnografiia, 1936, No. 3, pp. 101-105).

¹² S. Ter-Avetisian, *Pamiatniki drevnosti Karabakha i Skifskaia problema* [The Monu-

closely associated with the ancient region of Albania, was begun by N. Marr and Emil Rösler.¹³ The results were published as brief notes in the reports of the Imperial Archaeological Commission, 1892-1898. In 1926 the Society for the Study of Azerbaidzhan sent an expedition under the leadership of I. I. Meshchaninov to excavate the tumuli of the Khodzhalu area. Only a brief preliminary report was published by Meshchaninov;¹⁴ this led to the publication of Rösler's materials from Karabakh stored in the Hermitage Museum, Leningrad.¹⁵

Ter-Avetisian, studying the distribution of the tumuli, found them from the northeast part of the Albanian (now Evlakh) plain south into the Karabakh region and also southward in the Khochen, Karkar, and Terter Valleys. The latter extend to Lake Sevan in southern Azerbaidzhan. Along the entire southern frontier of Azerbaidzhan there exists a chain of tumuli between Lake Sevan and Lenkoran. The Khochen Valley tumuli are concentrated in the Arachadzor region. The line of tumuli along the Karkar Valley goes through the Askeran Pass and part of Kurdistan,¹⁶ into the Zangezur region, and disappears near Karaklis. The most important Bronze Age group of tumuli, partly excavated by Rösler in 1894, is near Khodzhalu village in the Karkar Valley.

In general the tumuli, twenty-five to thirty meters in height, belong to the Hallstatt period, although some have been attributed to later periods up to the Urartian epoch. Meshchaninov (*op. cit.*, p. 104) found five distinct types of tumuli near Khodzhalu. In shape they do not differ from South Russian tumuli. In the Khochen Valley the tumuli are faced with large, unhewn stones. In Karabakh there were stone box burials containing skeletons associated with bronze, occasional flint, gold, and iron objects from Khodzhalu, Varanda, and Dashalti near Shusha. Other burials,

ments of Antiquity in Karabakh and the Scythian Problem] (Izvestiia Nauchno-Izledovatel'skogo Instituta Kavkazovedeniia [Transcaucasian Branch of the Academy of Sciences, Tiflis], No. 1, 1934).

¹³ Part of Virchow's *Über die kulturgeschichtliche Stellung des Kaukasus* (Abhandl. der Königl. preuss. Akademie der Wissenschaften zu Berlin, 1895, Phys. Abhandl., Vol. 1, pp. 1-66) was based on Rösler's excavations.

¹⁴ I. I. Meshchaninov, *Arkheologicheskaiia ekspeditsiia Obshchestva v Nagornyi Karabakh i Nakhkrai* [Archaeological Expedition of the Society to Mountainous Karabakh and Nakhichevan A.S.S.R.] (Izvestiia Obshchestva Obsledovaniia i Izucheniia Azerbaidzhana [Society for the Study of Azerbaidzhan, Baku] No. 4, pp. 104-107, 1927).

¹⁵ T. Passek and B. Latinn. *Ocherk doistorii Severnogo Azerbaidzhana* [A Sketch of Azerbaidzhan Prehistory] (*ibid.*, No. 3, pp. 112-57); *idem*, *Khodzhalinskii Kurgan No. 11* [Khodzhalu Tumulus No. 11] (*ibid.*, No. 11, pp. 58-66).

¹⁶ There are approximately a hundred tumuli near Arishtara in Kurdistan.

marked by circles of cobblestones, contained incinerated remains together with large jars and stone implements. Unpublished pottery is stored in the Hermitage and in the Moscow Historical Museum. The Khochen tumuli contained multiple burials, a skeleton in the center being surrounded by as many as eight flexed skeletons (of slaves?).

The bronzes from Karabakh tumuli are characterized by decorations using locally known animals. No influence of Assyrian art is discernible, as in southern Armenia.¹⁷

Virchow discerned in the Karabakh tumuli the sway of a northern culture which he termed North Khaldic industry. Ter-Avetisian thinks that a more precise term would be "Scythian." According to Rostovtsev¹⁸ Transcaucasia was never isolated culturally from the North Caucasus and South Russia. The Scythian invasions of Albania were recorded by Herodotus and Xenophon. Marr found Scythian elements in the geographical names of this area. According to Strabo, a Scythian tribe, the Sacae,¹⁹ conquered Bactria and invaded the choicest land of Armenia, which was named Sacassēnē [Σακασσηνή] as a result. Further exploration of the Karabakh tumuli is anticipated to result in valuable contributions to the present knowledge of the Scythian problem.

*Nakhichevan.*²⁰ In the Nakhichevan Autonomous S.S.R., a subdivision of the Azerbaidzhan S.S.R., A. Alekperov of the Academy of Sciences in Baku has conducted several archaeological surveys, which were continued during May and June, 1936. A study of the painted pottery found in this area is now being prepared. Although there is no record of the stratification, the following sites have yielded painted pottery, according to the notes of Alekperov:

Chahtakhti	39° 22' N., 45° 7' E.
Danagirt	38° 59' N., 46° 0' E.
Ibadulla	39° 29' N., 45° 2' E.
Kara-Kala	39° 17' N., 45° 30' E.
Khock	39° 23' N., 45° 12' E.
Kizilvank	39° 6' N., 45° 28' E.
Kul Tepe	39° 16' N., 45° 29' E.
Milakh	39° 15' N., 45° 49' E.

¹⁷ The only exception was an Assyrian bead with a cuneiform inscription found by Rosler in Khodzhalu tumulus No. 11. This was dated by Meshchaninov between 763-755 B.C.

¹⁸ M. I. Rostovtsev, *Skizny i Bosfor* [Scythia and Bosphorus] (GAIMK, 1925, p. 304).

¹⁹ Cf. under IAngiul Valley, p. 479.

²⁰ Information supplied by E. B. Reilly in a letter dated December 5, 1936. Cf. R. A. Bowman, *The Oriental Institute Report on the Near East* (American Journal of Semitic Languages, Vol. 53, No. 2, pp. 123-24, Chicago, 1937).

Nahagir	39° 16' N.,	45° 37' E.
Ulia-Norachane	39° 36' N.,	45° 5' E.
Zurnabad	40° 30' N.,	46° 10' E.
in Azerbaidzhan		

From Kizilvank on the Aras there is pottery which belongs to the Copper Age of Anatolia (Alishar III), handmade, with a burnished slip fired plum-red to yellow, and decorated with an inch wide band of light red paint around the rim. On the most common form, a shallow bowl, a few broad stripes of red paint run down from the rim and intersect at the base. A similar piece of pottery,²¹ with only the rim painted, comes from Hasankale on the Aras in Turkey.

A typical piece of wheel-made pottery from Nakhichevan and Armenia is illustrated by Frankfort.²² The same design appears on red-wash and wheel-finished ware. The *schnabelkanne* is also a popular form and closely related to the Iranian types. The same forms become almost exclusively black in the region nearer the Caucasus. Red painted ware has been found as far north as Zurnabad and a wet-smoothed *schnabelkanne* appeared in the Shirak steppe of eastern Georgia.

ARMENIA²³

In 1936 the Institute of the History and Culture of Armenia in Erivan discovered what are believed to be Neolithic remains at Shungavit, a suburb to the south of Erivan, and at Muchannat-Thapa near the railroad station in Erivan.

Both sites contained flint and bone tools and crude black pottery. At Muchannat-Thapa the Urartu period is also represented by some red painted pottery with black geometrical designs, molds, bronze knives and axe heads, and Urartu roll seals which are slightly tapered with the larger base cut for use as a stamp seal.

In recent years Urartu cuneiform inscriptions have been found at the following places:

Adyaman	40° 8' N.,	45° 15' E.
Aluchalu	40° 8' N.,	45° 30' E.
Elar	40° 16' N.,	44° 38' E.

Excavated 1928-31

²¹ Museum of Georgia, Tiflis, Cat. No. $\frac{61-17}{6}$.

²² H. Frankfort, *Studies in Early Pottery of the Near East Vol. I Mesopotamia, Syria, and Egypt and their Earliest Interrelations* (Occasional Papers, Royal Anthropological Institute No. 6), Pl. 5, fig. 1.

²³ See footnote 20.

Kolanran	40° 10' N., 45° 28' E.
Ordaklu	40° 31' N., 44° 56' E.
Pashakend	40° 20' N., 45° 7' E.
Tapa-Aramavir	40° 5' N., 44° 3' E.

Excavated in 1893 and 1936, the recent work disclosing houses and a temple, as well as about twenty cuneiform inscriptions

Zagalu	40° 11' N., 45° 37' E.
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In 1936 at Karaklis, where de Morgan worked, many Bronze Age objects were associated only with black pottery.

DAGHESTAN

In 1924 an expedition under the auspices of the Daghestan Republic (A. S. Bashkirov, leader)²⁴ continued investigation of ancient monuments in southeastern Daghestan. Numerous fortifications, burial grounds, huge tumuli, etc., were examined in the vicinity of Derbent and parts of an ancient wall were excavated. A minaret of the fourteenth to fifteenth centuries in Darvag village, ruins of an ancient mosque near Zil', and burial grounds near Gemeidi, with keel-shaped tombstones, were studied.

In the Kaïtag-Tabasaran region were examined about eighty reliefs depicting lions, either attacking a wild boar, a deer, or some other animal, or two facing each other rampant. The expedition made a brief survey of many burial grounds and fortifications bearing reliefs and inscriptions, noting in many instances sculptured, "console-like" fragments on the turrets. One unique tombstone presented an eagle and two lions on the front, the figures surrounded by geometric ornamentation composed of different elements, while the back side was embellished with decorative designs and Arabic graffiti. In the Kala-Koreïshsk mosque two doors bear plant and animal figures.

Itsari village, built on the ruins of a castle, was investigated. Here a round, Medieval turret is well preserved. On the ancient structures, as well as in the newer buildings constructed from the material of the former, were found: (1) remains of engraved stone very similar to those discovered in the Avar and Andiïsk regions in Daghestan in 1923; (2) reliefs of human and animal heads of the Amuzgi and mountain Chechnia type; (3) sculptured monuments similar to the Kubachinsk structures; (4) many ancient inscriptions. In the Makhach-Kala (former Petrovsk) region tumuli, fortifications, and numerous tombstones of peculiar shape with highly artistic ornamental designs were investigated.

²⁴ A. S. Bashkirov, *Izuchenie Pamiatnikov Stariny* [Investigations of Ancient Monuments] (Daghestanskii Sbornik [Daghestan Collectanea], 1927, pp. 233-49).

NORTH CAUCASUS²⁵

1. The Manych Expedition²⁶ of GAIMK (M. I. Artamonov, leader) continued work near the Manych canal, in the location of Khutor Veselyĭ, Mechet' district, Azov-Black Sea region. The excavation of a tumulus, begun in 1934, was continued in 1935. Some flexed burials were unearthed inside the tumulus above the level of the plain, while below were several others of the catacomb type. Both kinds of burial were also found in a smaller tumulus. Two catacomb burials with pottery of new shapes and types contained deformed crania, one of the children's graves having a human head modeled in red paint. Metal (bronze?) and stone implements were also found.

2. The Kuban Expedition²⁷ of IAE in cooperation with several other scientific bodies (V. A. Gorodtsov, leader) continued excavation at the fifth century B.C. to the first century A.D. site of Elizavetinskaia on the Kuban River,²⁸ eighteen kilometers west of Krasnodar, during 1935 and 1936. Burial grounds here were examined in 1935; grave furniture included pottery, rings, pendants, and coins. Several *gorodishches*²⁹ of the same

²⁵ The Muzeĭ Gorskikh Narodov Severnogo Kavkaza [Museum of Mountain Peoples of North Caucasus], Budionovskiy 60, Rostov-on-Don, contains neither Paleolithic nor Neolithic objects but there are important local ethnological and historical materials. A local linguistic map is being prepared (H.F.).

²⁶ Cf. ESA, Vol. 10, 1936, p. 144.

²⁷ D. N. Lev, *Arkheologicheskaiia sektsiia Instituta* [Archaeological Section of IAE] (*Sovetskaia Etnografiia*, No. 2, 1936, pp. 144-46).

A summary of the 1928 excavations at the Mousterian site at Il'skaia, forty kilometers southwest of Krasnodar in the Kuban region, was published by S. N. Zamiatnin, *Résultats des dernières fouilles à la station paléolithique d'Il'skaia* (Transactions, II International Conference of the Association for the Study of the Quaternary Period in Europe, fasc. 5, pp. 213-24, Leningrad, 1935).

²⁸ The preliminary report of 1934 excavations mentions the find of many pieces of classical ceramics, including anochoes, Alexandrian jars, and a Megara cup with the effigy of Cybele and a hunting motif, well-preserved Panticapean coins, fourth to second centuries B.C., many stamped handles of amphoras, of third to second centuries B.C., originating from Rhodes (25), Sinope (17), Cnydos (1), Thasos (1). M. V. Pokrovskii identified this site with the ancient city of Gargas (*Рёгоруа*) mentioned by Diodorus. V. A. Gorodtsov, *O rezultatakh arkheologicheskikh issledovanii Elizavetinskogo gorodishche i mogil'nika v 1934 g.* [The Results of Archaeological Explorations of Elizavetinskaia *Gorodishche* and Burial Ground in 1934] (*Sovetskaia Etnografiia*, No. 3, 1935, pp. 71-76).

²⁹ In his translation of S. S. Magura, *Early Slavonic Pottery Dag up at Kiselevka Hill, Kiev, in 1932* (Journal, Royal Anthropological Institute, Vol. 65, 1935, p. 114), Ellis H. Minns suggests "town sites" as a suitable rendition of this specific term, meaning "sites inhabited continuously." As the term seems to be used currently to designate many types of sites, characteristic of eastern Europe and parts of Asia, we are following the example of ESA and consistently retaining the Russian form of the original (E.P.).

period were explored by the expedition in the neighborhood of the burials, as well as part of the great *gorodishche*, two cemeteries, and a round barrow previously explored by N. I. Veselovskii, who thought it to be a Scythian tumulus. Among the specimens were gold and silver ornaments, bronze and iron objects. Panticapean copper coins, many clay vessels of various forms bearing Greek stamps, net and loom weights. There were also many remains of domesticated animals and fish. In the *gorodishche* the expedition unearthed an altar, pottery kilns, and stylized clay figurines of local manufacture but copied apparently from Greek models. The round hill yielded restored dwellings of the fourth to third centuries B.C., of construction similar to modern Kuban houses since clay fragments bearing the impression of twigs and reeds were found. One clay fragment retained traces of white pigment. Among the fauna were the bones of a rooster.

3 The Mozdok Expedition of GAIMK (M. A. Miller, leader) explored Scytho-Sarmatian settlements and cemeteries in this region.

CRIMEA³⁰ AND BLACK SEA REGION

1. The Crimean Expedition of IAE (S. N. Bibikov, leader) worked in cooperation with the Soviet Section of the International Association for the Study of the Quaternary Period. The Shan-Koba³¹ rock-shelter excavations, begun in 1927, were continued. In 1935 three basic cultural horizons, yielding about eight hundred flint artifacts, were determined in this Azilian-Tardenoisian site. In addition were found a hoard of unworked flints in the sixth layer; a storeroom filled with snail shells (*Helix vulgaris*) in one of the lateral crevices of the shelter, and hearths at various levels. Of special note were several bone knife handles, scraper handles, etc. An ornamented knife handle was the first of this type ever found in the Soviet Union. Caspian affinities of the Crimea are demonstrated by comparing bone and stone implements from the lower layers of Shan-Koba with those of the upper horizons at Siuren, an Aurignacian grotto thirteen kilometers southwest of Bakhchisaraï excavated from 1926-1929 by G. A. Bonch-Osmolovskii.³² Especially significant in this connection is the presence of nuclear types of flint implements and elongated blades.

³⁰ See *Iz istorii Bospora* [Contribution to the History of Pontus], a collection of articles published by GAIMK, 1935.

³¹ S. N. Bibikov, *Tezisy doklada "Itogi raboty krymskoï ekspeditsii v skalistom naveshe Shan-Koba"* [The Theses of the Report, "The Results of the Work of the Crimean Expedition in the Rock-shelter of Shan-Koba"] (Otchetnaia arkheologicheskaiia sessiia IAE, Leningrad, February, 1936).

³² G. A. Bonch-Osmolovskii, *Itogi izucheniia Krymskogo paleolita* [The Results of Study of Crimean Paleolithic Period] (Transactions, II International Conference for the Study of Quaternary Period in Europe, 1934, fasc. 5).

2. During the summer of 1936 an expedition from IAE (S. N. Bibikov, leader) began an archaeological survey of the valley of the Chernaia River in the Sevastopol District. E. V. Zhironov, physical anthropologist, found two nearly complete "Crô-Magnon" skeletons badly crushed under heavy stones near the entrance to the small cave of Murzak Koba. They were not buried in a pit. The male skeleton lacks only the left leg, the right tibia and fibula, and the phalanges. The following measurements and observations were recorded:³³ age 40-55; stature about 1800; undoubtedly dolichocephalic, although part of frontal bone absent; bizygomatic breadth 147; morphological face height 70; typical deep Crô-Magnon notch at nasion; pronounced Crô-Magnon type tibia, sharp and compressed laterally; orbital height only 27; orbital breadth, very wide, 47; very heavy supra-orbital crests; superior oblique ridges on occiput; all teeth present but much worn, especially upper teeth, which were worn past pulp chamber in several instances; upper teeth nearly all abscessed, pyorrhea being present; and a distal occlusion of the upper jaw.

The female skeleton appears to be complete except for part of each hand and foot. This young individual possessed orbits which were not so wide laterally in proportion to the vertical diameter. All teeth back to and including the first molar showed considerable signs of wear but the second molars showed very little wear and the third molars none at all. Although their food must have been abrasive this must have been eaten outside the cave where only remains of snails and fish were found.

These skeletons were excavated from a rich cultural deposit containing stone and bone implements attributed to the Azilian-Tardenoisian period. At the woman's right hand lay a knife made of a rib of some large animal. Three bone harpoons were found near the skeletons.

This is the second Crimean site containing human remains belonging to this transition period. Bonch-Osmolovskii uncovered a skeleton, neither Crô-Magnon in type nor in manner of burial, buried in a pit in a crouching position.

Since the deposits at Murzak Koba grew richer as the excavations proceeded, plans were made to continue work there during 1937.

3. The Kerch Expedition³⁴ of GAIMK (L. Slavin,³⁵ leader) continued

³³ In a letter from R. F. Barton of IAE dated December 17, 1936.

³⁴ Report transmitted in a letter dated November 15, 1936, from Leo S. Berg of the Institute of Fisheries, U.S.S.R. Academy of Sciences, Leningrad. He adds that the ancient Pontine fisheries have been described in an article by M. Turpaev, *Ocherki po istorii posola ryby v drevnii period* [Essays on the History of Fish-salting in Antiquity] (Rybnoe Khoziaistvo S.S.S.R., No. 4, 1935, pp. 42-49).

³⁵ Chief of Kamysh-Burun Section of the Kerch Expedition and Scientific Secretary of GAIMK.

examination of the ancient city of Tiritaka-Dia, in a locality now known as Kamysh-Burun,³⁶ about eleven kilometers south of the modern Kerch, formerly Panticapea, the capital of Pontus. During 1935 six Roman tanks, with total capacity of about thirty-three cubic meters, constructed of close-fitting limestone slabs (180 by 180 by 15-25 centimeters), were found. The inner walls of the tanks were coated with a mixture of lime and sand and pulverized brick. All angle-joints and walls fitted well. The remains of stone walls and the mass of tile fragments indicate that the plant was probably sheltered by a tile roof. Compressed layers of small fish bones and scales, mainly of Kerch herring (*Clupea caspialosa*) were found in some of the tanks. Near a defensive wall, three meters thick, lay indications of economic life: fish-salting cisterns of the Roman period, millstones in a building of the early Middle Ages, a granary with wheat grains, animal bones, many coins, and painted pottery.

At Myrmikia work was continued and two wineries discovered, each including a well, one of them with potable water. The exceptional strength of the concrete and the waterproof qualities of the coating were especially noted. The excavations, which will be continued during 1937, disclosed Myrmikia in the role of a large wine-producing center of the Bosphorus.

4. The Olvia (Olbia)³⁷ Expedition of GAIMK (F. A. Kozubovskii, L. M. Slavin, F. N. Molchanovskii, leaders) was sponsored by GAIMK and ANU. The staff consisted of twenty-six members, including archaeologists, architects, a geologist, an artist, a chemical expert, and an osteologist. This site was one of the most important Black Sea colonies. The excavation of an entire city quarter of the Hellenistic period was completed, as well as the main street leading from the city gate to the port. The northern defensive wall was explored disclosing a hundred meter clay substructure of the city wall. Many objects attributed to the Greek and Roman periods were brought to light. Seven hundred square meters of the harbor area were examined, especially a Roman stratum, two to three meters in depth. Two large buildings, one of them a bakery, were unearthed. Two Roman vaults excavated in the necropolis yielded a golden wreath, buckles, golden earrings, etc.

5. The Crimean Expedition of GAIMK in cooperation with the Military Engineering Academy of the Red Army studied the ancient fortifications of Kafa, Sol'dag'ia, Arabat, Chersonessus, Heraclea, Kala-

³⁶ Cf. ESA, Vol. 10, 1936, p. 146.

³⁷ A Parthian amphora from the earlier excavations at Olbia was published by A. Strelkov, *Eine parthische Amphora in Moskau* (Arch. Anz., Vol. 50, pp. 58-70, 1935). Cf. American Journal of Archaeology, Vol. 40, p. 355, 1936.

mita-Inkerman, Eski-Kermen, Mangup-Kale, and Chembalo. Some archaeological objects were obtained.

6. The Theodosia Expedition of GAIMK (V. V. Danilevskii, leader) examined the water supply of the ancient Crimea. The sites of Tepe-Oba and Dzhan-Kutaran Creek and of an ancient Armenian monastery were excavated and an archaeological map of the region prepared.

7. The Kharak Expedition of GAIMK (IU. V. Sergievskii, leader) excavated a Roman fortress.

8. The Tardenoisian skeleton from Fatma-Koba discovered by Bonch-Osmolovskii was placed in the Quaternary Hall in the Geological Museum of the Academy of Sciences, Leningrad.

UKRAINE³⁸

1. The Gontsi Expedition of ANU, assisted by MOGAIMK³⁹ (A. IA. Briusov, leader), excavated a Paleolithic (Magdalenian?) site.

2. The Tripolie Expedition of ANU, assisted by MOGAIMK (T. S. Passek,⁴⁰ leader), continued excavations of sites yielding Tripolie culture.

3. The Dnepropetrovsk Expedition⁴¹ of ANU (T. T. Tesla and L. A. Lepikash, leaders) examined during 1935 the Mousterian site, discovered by Tesla in the previous year, near Staryi Kodak village, seven miles south of Dnepropetrovsk. The fauna included *Elephas trogontherii*, *Rhinoceros tichorhinus*, *Bison priscus*, *Cervus megaceros*, *Cervus elaphus*, and *Rangifer tarandus*.

4. The Chernigov Expedition of ANU (F. A. Kozubovskiy and T. G. Pydoplychka, leaders) during 1935 excavated a Magdalenian site near Chulatovo, eight kilometers south of Novgorod-Seversk, on the right bank of the Desna River.

EUROPEAN RUSSIA

1. The greatest single enterprise of the MOGAIMK⁴² was the study of the antiquities found during construction of the Moscow Subway.

³⁸ S. S. Magura, *Early Slavonic Pottery Dug Up at Kiselyvka Hill, Kiev, in 1932* (Translated by Ellis H. Minns, *op. cit.*, pp. 113-21 and Pls. 4 and 5), contains an account of the oldest Slavonic pottery known (before the sixth century A D.), associated with some pottery showing Roman influence, discovered by an expedition from ANU.

³⁹ The Moscow branch of GAIMK is a separate institution under a different government department.

⁴⁰ See *La Céramique tripolienne* (in French) in the *Izvestiia* of GAIMK, No. 122, 1935.

⁴¹ L. A. Lepikash, *Zamitka pro geologichni umozy znakhudky davnogo paleolitu v okoliakh s. Staryi Kodak na Dni propetrovshchyni* [A Note on the Geology of the Lower Paleolithic Site in the Neighborhood of Staryi Kodak Village in the Dnepropetrovsk Region] (*Chetvertynnyi Period*, No. 10, 1935).

⁴² Collection of materials, *Metro pervoi ocheredi*, is now being prepared for publication.

2. In 1935 the Sarkel Expedition of GAIMK (M. I. Artamonov, leader) continued excavations in the Khazar city of Sarkel on the Don near Tsimilianskaia. Beside the river were uncovered houses and storage pits which yielded many important objects including a bone tool with a carved culture's head, metal buckles, sickles, axes, and other implements, many sherds, and several complete jars. North of the city the upper stratum was removed over an area of 200 square meters and a trench was sunk to bed rock. Remains of buildings with brick ovens, mud walls and floors containing straw and more ancient crushed baked brick were uncovered. The latest period of this settlement dates to the twelfth century. Nine small tumuli with horse burials and a large tumulus used as a sepulcher for 124 individuals were excavated. Objects from this large eleventh century tumulus included earrings, pendants, crosses, beads, small bells, buckles, textile fragments, daggers, etc.

In a Bronze Age site near Krasnyĭ IAr, five kilometers from Sarkel, was found a mud hut pitted for roof poles, a fireplace, many sherds, flint and bone tools including a perfect harpoon and several blunt awls, *tupik*, a bronze awl, and a small chisel.

3. The Western Province (*Oblast*) Expedition of IAE (K. M. Polikarpovich,⁴³ leader) continued excavation of the Magdalenian site of Eliseevichi, on the right bank of the Sudost' River, forty-five kilometers west-southwest of Briansk and fifty kilometers east of Mglin. The station is situated in a loess deposit contemporaneous with a lower terrace of the Upper Dnieper region. Twenty thousand worked flints were collected. Under a pile of mammoth tusks were found a fifteen centimeter female figurine differing considerably in style from any existing Paleolithic "Venus," a representation of a fish, and fifteen oval tablets of mammoth ivory, the latter delicately engraved with complicated designs. For the first time a significant series of Magdalenian art objects was placed in the hands of Soviet scholars. The fauna included *Elephas primigenius*, *Canis lupus*, *Vulpes* sp. (*?lugopus*), etc. The finds were divided between the Museum of the White Russian Academy of Sciences in Minsk and the IAE Museum in Leningrad.

4. The Kama Expedition of GAIMK (N. A. Prokoshev, leader) continued work near Lake Griaznoe, where four Neolithic dwellings were found, in the vicinity of Turbino, where they excavated 250 square meters, and near Konets Gor. No complete burials being discovered in Turbino, it

⁴³ For report on 1930 excavations see K. M. Polikarpovich, *Paleolit na r. Sudas'tsi* [Paleolithic Remains on the Sudost River] (Pratsy, Belorus Akad. Nauk, Sekts. Arkheol., Vol. 3, 1932).

was concluded that the group of tombs, first discovered by A. V. Schmidt,⁴⁴ was exhausted. Turbino, however, yielded many nephrite rings, knives, scrapers, and flint flakes for insertion in bone handles. The excavations during 1935-1936 at Rodanov *gorodishche* nearby brought to light a small hoard of coins which verified the dating of this site in the eleventh century. Remains of a dwelling including a fireplace and several stove emplacements were also investigated. Further, the expedition explored along the Chusovaia River, where a score of new *gorodishches*, settlements, and burial grounds were discovered.

5. The Kola Expedition⁴⁵ of GAIMK (B. F. Zemliakov, leader) was organized in cooperation with the Soviet Section of the International Association for the Study of the Quaternary Period in Europe. The expedition prospected for the sites of the so-called Arctic Paleolithic in the regions of the Rybachii Peninsula, especially in the neighborhood of Motovskaia Guba (Bay) near the Finland frontier. Three of these sites were found on the eastern shore at heights of sixty, forty-two, and thirty-five meters above sea level respectively. The implements, buried in rough gravel twenty to twenty-five centimeters below the surface, are similar to those from Finland and Norway. These are the oldest sites found in this territory.

Several Arctic Neolithic sites were also found at a height of from eleven to fifteen meters above sea level. Flint implements, occurring at only two stations, included crudely flaked points, scrapers, and flakes. At one site quartz and polished implements were present along with flint tools and some chalcedony points.

6. During 1935 and 1936 the Karelian Expedition⁴⁶ of IAE and the Ethnographic Museum in cooperation with GAIMK (V. I. Raudonikas,⁴⁷ leader) studied seven hundred petroglyphs on the shores of the White Sea and near Lake Onega.

7.(a) The Novgorod Expedition was organized by the local museum under the direction of GAIMK (V. I. Raudonikas, leader). Excavations were continued in Rurik's *gorodishche*, exposing a log dwelling complex with the fireplace and an accumulation of cultural remains of ninth to

⁴⁴ A. V. Schmidt, *Die Ausgrabungen bei dem Dorfe Turbina* (Ethnologische Anzeiger, Vol. 8, 1926).

⁴⁵ Cf. ESA, Vol. 10, 1936, p. 145, *Kolskii Sbornik* [Collected Works on Kola] (Materialy Komissii Ispeditsonnykh issledovanii, Akad. Nauk S.S.S.R., Seria severnaia, 1930).

⁴⁶ Cf. ESA, Vol. 10, 1936, pp. 140-42, also GAIMK Annual Report for 1935.

⁴⁷ V. I. Raudonikas, *Naskal'nye izobrazheniia Onezhskogo ozera i Belogo moria* [Rock Engravings Near Lake Onega and the White Sea] (French resumé Trudy IAE, Vol. 9, Arkheologicheskaiia Seria, No. 1, 1936).

tenth century feudal Novgorod. The clay substratum yielded Neolithic finds belonging to the "matriarchal gens society" of the third to second centuries B.C.

At Slavna,⁴⁸ one of the oldest parts of Novgorod, were uncovered Byzantine frescoes, numerous human skeletons, charred grains of wheat, a fifteenth century silver coin, colored glass bracelets, heavy spurs, tools of various trades, eating utensils, and remains of food.

(b) Members of the Novgorod Expedition of the Leningrad Russian Museum are restoring frescoes (one at Arkzha of a boy digging the earth with a pickaxe) in many of the ancient churches of this region.

8. The Davydkovskii Expedition of MOGAIMK (K. IA. Vinogradov, leader) excavated the Davydkovskii burial grounds near Moscow.

9. The Vaulovo Expedition of MOGAIMK (D. A. Kraïnov, leader) examined cemeteries containing objects of the Fat'ianovo culture in the Ivanovsk region.

10. The Kolomna Expedition of MOGAIMK (N. P. Milonov, leader) uncovered a *gorodishche* which contained objects apparently of the Fat'ianovo culture.

11. The Kalinin Expedition of MOGAIMK (N. P. Milonov, leader) excavated a *gorodishche* near Kalinin (formerly Tver).

12. The Istrinsk Expedition of MOGAIMK (K. IA. Vinogradov, leader) investigated a burial ground containing objects of the Fat'ianovo culture.

13. The Volga Expedition of GAIMK (P. N. Tret'iakov, leader) continued excavation of a fourth to fifth century *gorodishche* near Yaroslavl, which is to be submerged by a hydroelectric plant, and other sites. Many houses, mills, and tombs were brought to light.

14. The Uglich Expedition of GAIMK (P. N. Tret'iakov, leader) explored the Upper Volga region. Two Neolithic stations and two *gorodishches*, of the third or fourth century A.D. and of the middle of the first millennium B.C. respectively, were discovered.

15. The Kaluga Expedition of GAIMK (M. M. Gerasimov, leader) prospected a 120 kilometer tract between Kaluga and Belev. About one hundred late Slavic tumuli, three Neolithic stations, five *gorodishches*, and other ancient monuments were reported. A well-preserved *gorodishche* of the Dyakov type was discovered near Spassk.

16. "The upper part of a human skull 'said to date back to the last phase of the Glacial epoch' has been found near the Moscow-Volga canal, on the banks of the Skhodnaia River, at a depth of twelve feet in an al-

⁴⁸ *Sovietsland* (in English. Leningrad, November, 1936), pp. 24, 25, 32; see plates.

luvial deposit of the Quaternary Period. It is described as having protruding browridges and a very low forehead, thus resembling skulls of the Neanderthal type."⁴⁹

17. The Komi Expedition of IAE (A. K. Supinskii, leader) began an ethnographical study of the Pinega region including the ancient settlements and *gorodishches* and their modern counterparts among the present dwellers in the Komi (Zyrian) Autonomous Area. V. N. Chernetsov explored the regions of Severnaia Sosv' and Liapina, where he discovered several *gorodishches* belonging to various periods and collected stone artifacts and pottery.

18. In 1936 the Don Expedition of IAE (P. P. Efimenko, leader), cooperating with GAIMK, continued examination of the Paleolithic stations of Kostenki and Borshevo near Voronezh.

Tatar A.S.S.R. The Suvar Expedition of MOGAIMK (A. P. Smirnov, leader) excavated a Bulgarian *gorodishche*.

TURKESTAN

*Uzbek S.S.R.*⁵⁰ 1. During 1935 the Bokhara Committee for the Preservation and Study of the Monuments of Material Culture excavated a wall surrounding Bokhara.⁵¹ This wall, attributed to the eighth century A.D., had the character of an earthen rampart with watchtowers of unbaked clay and was constructed after the fashion of similar walls of pre-Moslem origin which were once erected around large areas such as Samarkand. Samanid Ismail, who exerted his sway over Bokhara from A.D. 874 to 907, was the first ruler to abandon maintenance of the wall surrounding the oasis of Bokhara. This great wall, known as the Kampyr-duval ("Old Women's Wall") was two hundred and fifty kilometers in length.

2. The earliest mosque⁵² in Soviet Central Asia was discovered during an archaeological survey, conducted by the Committee, in Khozara village, forty kilometers from Bokhara. On the basis of architectural and stylistic form this mosque is attributed to the eighth century.

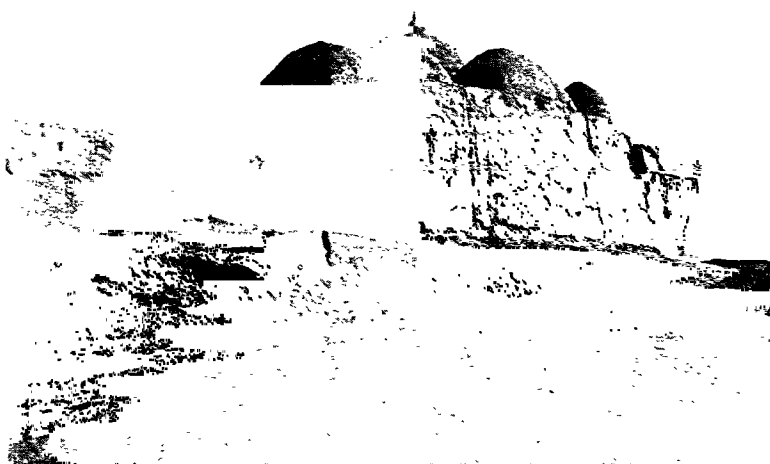
According to Eric Schroeder,

this mosque (if it was always a mosque or not is perhaps uncertain) is an invaluable piece of evidence as to the nature of pre-Samanid Islamic architecture in this region, formerly absolutely unknown. The earliest Islamic monument of Transoxiana was

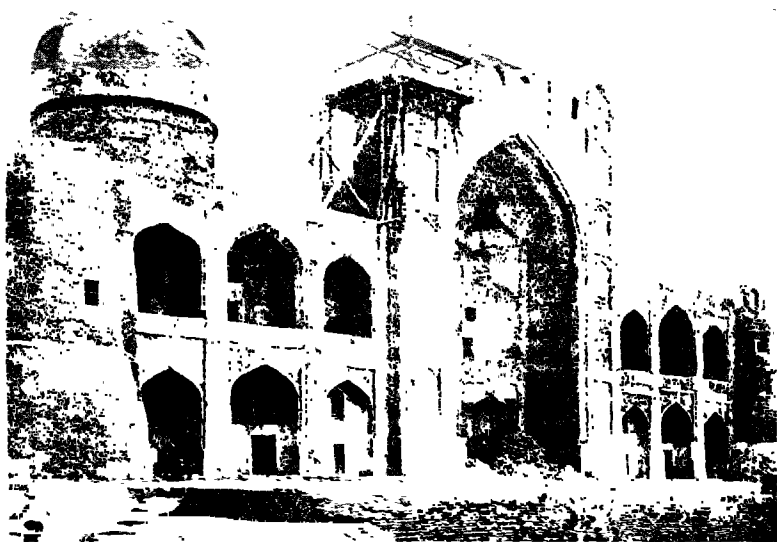
⁴⁹ *Antiquity*, December, 1936, p. 481, quoted from *Evening Standard*, August 18, 1936, London.

⁵⁰ In a letter from the Bokhara Committee dated July 17, 1936.

⁵¹ Cf. R. A. Bowman, *The Oriental Institute Report on the Near East* (*American Journal of Semitic Languages*, Vol. 53, No. 2, pp. 123-24, Chicago, January, 1937).



A



B

Mosques in the Bokhara region. A, Eighth century mosque in Khovara (40 kilometers from Bokhara). B, Mir-i-Arab in Bokhara (sixteenth century)



Vaulted arches in the Khozara mosque

hitherto the Mazar of Ismail Samanid at Bokhara. The new discovery is an index of the persistence of little modified Sasanian forms: the lay of the pier bricks, similar to that of *Tepe Hissar* and the *Tāri-Khāna* of Damghan, and the plan for its corridor of circumambulation.⁵² The outset string-courses barring the intrados of the arches appear to me an importation from India, through Buddhism.⁵³

3. During 1936 another expedition (Sudakov, leader) began preliminary excavations of the Romitan tumulus. This was one of the most ancient cities of Sogdiana, considerably older than Bokhara, and the official residence of the pre-Moslem rulers.⁵⁴

4. The Samarkand Expedition⁵⁴ of the Uzbek State Historical Museum (I. A. Sukharev, leader) explored the region south of Samarkand between the suburbs and the Kry-tau and Agalyk ranges. This region includes the Dargomskaia steppe now uninhabited, but populated until the fifteenth century. Twenty-four monuments, registered during preparation of an archaeological map of the region, include sites of settlements, remains of irrigation, canals, etc. Two pre-Moslem clay seals, with human effigies *en face*, were brought to light.

5.(a) A fragment of a Greco-Buddhist sculptured cornice was found in October, 1932, by a Government cutter in the waters of the Amu on the Afghan boundary, near the border post of Airtam, thirteen kilometers west of Termez.⁵⁵ This was forwarded to UZKOMSTARIS. The cornice is carved in high relief on a slab (100 by 38 by 47 centimeters) of marly limestone containing calcite, and weighing 320 kilograms. The technique is reminiscent of wood carving. The foreground of the design consists of acanthus palmettos; above these are figures of musicians, two female and one male, separated by three larger acanthus palmettos. According to S. F. Oldenburg, this is a portion of a group of five of the so-called *Pañca Mahāśabda*, "The Five Great Sounds," of the Buddhist art canon.

The richly ornamented dress of the musicians is reminiscent of the Greco-Roman decadent period. The jewelry includes bracelets, earrings, pendants, and necklaces set with large stones. The tiaras resemble the recent headdress (*bargak*) of the noble ladies of the Khivan court in Tash-

⁵² Cf. E. Herzfeld, *Archaeological History of Iran* (London, 1935), p. 93.

⁵³ Cf. *Eastern Art*, Vol. 3, opp. pp. 181, 191, and, for a later example, *Memoirs of the Archaeological Survey of India*, No. 33, Pl. 13.

⁵⁴ I. A. Sukharev, *Raboty po etnografii i arkheologii v Uzbekistanskom Gosudarstvennom Istoricheskom Muzei* [Ethnographical and Archaeological Work of the Uzbekistan State Historical Museum] (*Sovetskaia Etnografiia*, 1936, No. 3, pp. 109-10).

⁵⁵ M. E. Masson, *Nakhodka fragmenta skul'opturnogo karniza percykh vekov n.e.* [The Finding of a Fragment of a Sculptured Cornice of the First Centuries of the Christian Era] (Materialy, UZKOMSTARIS, Tashkent, 1933, No. 1).

kent Museum. The faces of the effigies, two of which are damaged, are full, rounded, with bilateral chins and small cheek bones, straight Grecian noses, with large straight-set eyes and narrow, well-pronounced, sharply arched eyebrows, and moderately broad foreheads. The musical instruments include: a small, thirteen-stringed harp with a rectangular resonance chamber, widened at the top; a lute-like, four-stringed, waisted instrument with four lunate sound holes; and a small, barrel-shaped drum of the type used currently in India. These differ from other musical instruments of pre-Moslem times as recognized from the terra cottas of Afrasiab, etc. The remains of red ocher on the palmettos and the costumes suggest that the cornice may have been gilded, for ocher was frequently used as a ground.

According to Chinese sources⁵⁶ Termez was the capital of the Ta Mi kingdom, and the home of a thousand Buddhist monks, ten monasteries, and many stupas and statues of Buddha. Another Chinese source⁵⁷ mentions that the numerous temples and obelisks (?) of the Ephthalite capital of Ba-Di-Yan (now Badakhshan), 300 kilometers east of Termez, were covered with gold. The heavy weight of the slab suggests its use in a large structure, probably a stupa. In 1926 a tower in Termez was identified as a stupa by an expedition from the Museum of Eastern Cultures, Moscow.

According to Masson,⁵⁸ the stylistic peculiarities of the cornice date it in the first centuries of the Christian era, and it belongs with the so-called Gandharan Greco-Buddhist art of western India and northwestern Afghanistan. Further study might well shed additional light on the early Buddhist antiquities of Hadda, discovered by the French expedition to Afghanistan, and now dated as between the third and ninth centuries.⁵⁸

Other Buddhistic objects from this region and from Samarkand include bronze and stone effigies of Buddha, Bodhisattva, and Devata;⁵⁹ a Gandhara type of capital was discovered in 1932 near Vakhsh. The literary sources indicate that Buddhism flourished here between the second and eighth centuries A.D.⁶⁰ There are also a few numismatic finds in the neigh-

⁵⁶ P. Pelliot, *Temps dans les textes chinois et tibetains* (Doklady [Reports], Akad. Nauk, Ser. B, No. 16, 1929), p. 297.

⁵⁷ Iakini, *Istoria Severnykh dvorov* [History of the Northern Courts] (Sobranie svedeniĭ o narodakh Srednei Azii, Pt. 3, St. Petersburg, 1851), p. 176.

⁵⁸ Cf. J. Bartoux, *Les fouilles de Hadda* (Gazette des Beaux Arts, March, 1929, pp. 121-32); J. Hackin, *Les fouilles de la délégation archéologique française à Hadda* (Revue des Arts Asiatiques, No. 2, 1929, pp. 66-76).

⁵⁹ A. S. Strelkov, *Dor-lamskie pamyatniki drevnego Termeza* [Pre-Islamic Monuments of Ancient Termez] (Kul'tura Vostoka [Collection published by the Museum of Eastern Cultures, Moscow], No. 2, 1928, pp. 41-46).

⁶⁰ V. V. Barthold, *Vostochno-Iranskiĭ vopros* [Eastern Iranian Question] (Izvestiia GAIMK, Vol. 2, 1922, p. 376).

hood: a gold stater of Kanishka, who convoked the fourth Buddhist Council at Kashmir; and Indo-Scythian copper coins, one of them, reputedly, of the Greco-Bactrian King Apollodotus (second century B.C.).

(b) Alabaster ornaments, fragments of frescoes, and coins dating from the second and third centuries have been discovered at Termez by an expedition sent out from the Hermitage of Leningrad and UZKOMSTARIS. One of the halls of a twelfth century palace was completely excavated.⁶¹

6. The results of the 1934 Tashkent Expedition (G. V. Grigor'ev, of UZKOMSTARIS, leader) to the IAngiul' region in Chirchik Valley, sponsored by UZKOMSTARIS, have been published.⁶² Monuments were located in the area between Tersak railroad station, six kilometers from Tashkent, and Chinaz *kishlak* (hamlet), five kilometers from the Syr-Darya (Jaxarthes). The escarpment of the second terrace of the Chirchik, west to the Dzhun irrigation canal was also explored. Nineteen *gorodishches* of "feudal" and "archaic" types were reported.

(a) *Feudal Sites*. Five settlements were characterized by walls, bastions, towers, and by the presence of citadels. The finds are dated after the Arab Conquest. Excavation of garbage-pits of Iski-Kaunchi⁶³ brought to light: Afrasiab types of glazed pottery, mainly white with black and red ornamentation, bearing Arabic inscriptions of the Samanid period (tenth century A.D.); bottles and thin glass vessels, some of them ornamented; a dark green glazed lamp; wheel-made pottery kettles of hard gray clay, painted black; spheroidal, yellow clay, wheel-made kettles, large (diameter 35 centimeters) and strong but with thin walls (2 millimeters) and projecting hook-like handles; baked bricks (25 by 17 by 4 centimeters and 25 by 8 by 3.5 centimeters); and ornamented ceramic disks (diameter 70 centimeters), probably used as covers for vessels.

The Chinaz and IUGon *Tepes* yielded sphero-conical vessels, which occur from Moscow to Egypt, a hand-mill, a mold for silver ornaments, etc.

Zengi-Ati, with pre-Islamic cultural levels evident in the escarpment, and Banunkent complete the list of Feudal sites.

(b) *Archaic Sites*. The most important site was associated with Kaunchi

⁶¹ Antiquity, December, 1936, p. 483, quoted from The Times, August 17, 1936, London.

⁶² G. V. Grigor'ev, *Otchet ob arkheologicheskoi razvedke v IAngiul'skom raione UZSSR v 1934 godu* [A Report on an Archaeological Survey in the IAngiul' Region of Uzbek S.S.R. in 1934] (Tashkent, 1935).

⁶³ Identified by V. V. Barthold with Shuturket or Ushturket of Arabic geographers (Al-Mukaddisi); Chinaz was certainly Djinandjiket of Al-Mukaddisi and Al-Istakhri; IUGon *Tepe*, "2 farsangs from Shuturket," Denfeganket.

Tepe where at a depth of five meters a cave settlement was unearthed. In the upper levels (20 to 30 centimeters deep) were white glazed sherds of V. L. Viatkin's "Sogdian type," while below was an ash pit (1.5 meters) containing bones of domesticated *Bos*, *Ovis*, and *Sus*. The pottery included: a fragmentary red bead; a red-wash cubical vessel (12 by 12 by 12 centimeters), a miniature pot (11 centimeters high); a vessel⁶⁴ with a ram-shaped handle (12.7 centimeters high), one of the horns of the ram twisting forward; a crude kettle with rounded base and lid ornamented with finger impressions; pottery fragments showing cloth impressions; and fragments of pythoses. This pottery, with the exception of the Sogdian ware, was not wheel-made. Stone implements included a fragment of a hand-mill, two pestles, a stone hammer, several slingstones, and a rubbing stone (*polissoir*) with well-worn facets. A remnant of an unbaked brick wall, 10 centimeters thick, the intervals between the bricks filled with clay mixed with chopped straw, was found nearby. Beneath the wall and the pit lay a stratum of firm loess, 15 centimeters thick, below that, another ash layer, and at a depth of three meters the floor of the dugout. An iron arrowhead, probably square in cross section, was found in the floor, together with a bead made from a boar's tusk. In the western wall of the dugout was discovered a small cave-like stove, while another was found in the cave below the dugout, which also contained many sherds of crude pottery, a scraper made from a *Bos* rib, a quartzite flake, and the bones of ram, bull, horse, and wild duck. As no floor level could be identified in the cave, it is assumed that the remains belong to the later dugout period.

The finds from the *gorodishche* associated with the cave included many hand-mills, sherds, some with impressions of loom-made cloth, slingstones, and the bones of pig, camel, sheep, and bull. Most of the pottery was not wheel-made. Few glazed sherds were found, the greater number near the central *tepe*, currently occupied by a Moslem cemetery.

Further cave settlements of the same type were found: five in the second site associated with the *tepe*; at three other *tepes*, Kugait *Tepe* (six kilometers from Tashkent), Shash *Tepe* (three kilometers southwest of Tersak village, between Tashkent and Chinaz), and Kafter *Tepe* (near Abjal-bob village). Each *tepe* occupied a promontory of the second terrace, with a watchtower, surrounded by a depression, a semi-circular *gorodishche*, the remains of walls terminating at the slopes of the terrace, and sometimes a second settlement beyond the moat. The contents of the cave *gorodishches* were, with small variations, similar to those of Kaunchi *Tepe*, red-wash

⁶⁴ A similar vessel attributed to the third to second centuries B.C. was found in the Bronze Age tumulus near Khutor Veselyi, on the Kuban River, by the Manych Expedition of G.M.M.K. in 1934.

pottery, hand-mills, etc., the most characteristic feature being total absence of bowl-shaped pottery. In *Shash Tepe gorodishche* were the remains of a large structure, consisting of parallel arched corridors, with walls 2.3 to 2.8 meters thick, built of clay faced by bricks (57 by 52 by 10 centimeters). These may be the ruins of a stupa-like temple, a type common in Afghanistan, and also of some Mesopotamian *tepes*, whose unbaked bricks, by Borchard's measurements, would approximate in size those in *Shash Tepe*.

According to Chinese and classical sources,⁶⁵ the territory around Tashkent was occupied by a Scythian tribe known as the Sacae (Greek Σάκαι), the earliest mention of whom is found in the writings of Ctesias (fifth century B.C.). The last reference to the Sacae occurs in Chinese reports of the first century B.C., which state that the Se (i.e. Sacae) were expelled by another people, the Yue-chih, who, in their turn, were replaced by the Huns. On the basis of this and other evidence, Grigor'ev tentatively dates the cultural layer of the *gorodishches* within the walls between the first appearance of iron and the fourth century B.C. Ptolemy states that the nomadic inhabitants of the land of the Sacae lived in caves. Hence the cave settlements on the right bank may well be the remains of the Sacae dwellings mentioned by Ptolemy.

After the debacle in Central Asia, the Sacae migrated to the south of the Hindu Kush, where the dwellings described by the tenth century Arab traveler, Al-Istakhri, and in recent times by G. Markham, are comparable to the dugout dwellings of the Samarkand *tepes*. Ten other *tepes* of the "archaic" type were recorded by Grigor'ev in the IAngiul' region.

7. The Kazak A.S.S.R. Expedition of GAIMK (A. A. Jessen, leader) was organized in cooperation with the Nickel and Tin Prospecting Bureau to discover the sources of tin used in the metals of ancient Russia. Seventeen deposits were explored in the region of the Kalba and Naryn ranges, and in the valley of Irtysh above Ust'-Kamenogorsk. The only data bearing on the technique of mining in the first millennium B.C. were collected, and two new sites were found. The ancient gold mines of Kazanchukur were also investigated, as well as the Dzhungarian fortress of Ablankit, leading to the discovery of many tumuli and other monuments.

SIBERIA⁶⁶

1. The archaeological monuments of the Amur Valley from the mouth of the Ussuri River to the Tatar Straits were studied in 1935. Prior to the

⁶⁵ Cf. E. Hermann, *Sakai* (Pauly-Wissowa Real-Encyclopädie, 2. Reihe, Bd. I-A, Pt. 1, Stuttgart, 1920, cols. 1770-1806 and map).

⁶⁶ General references: B. F. Petri, *Sibirskii paleolit* [Siberian Paleolithic Period] (Trudy Gosudarstvennogo Irkutskogo Universiteta, Vol. 5, Irkutsk, 1923); and *Sibirskii neolit*

Revolution explorations had been made by Sternberg, Laufer, Arseniev, and others, and since then by Kharlamov, Sternberg, Marikovskii, Zolotarev, Schneider, Anuchin, Shuchenko, and Okladnikov.⁶⁷ Because the data available was scattered and fragmentary, the 1935 IAE expedition determined: (a) to prepare an inventory of ancient monuments in this valley between Khabarovsk and Nikolaevsk; (b) to make an archaeological map; and (c) to classify antiquity of monuments.

Two hundred archaeological sites, including ancient settlements, earth huts, *gorodishches*, and petroglyphs, a Neolithic cemetery and Iron Age burials, as well as traces of ancient iron smelting, were investigated. The first archaeological map was prepared and the preliminary classification and relative chronology of the sites in the lower Amur Valley were made.

Stone Age. With the exception of the so-called "Amur Paleolithic" stations (? *ateliers*) near Khabarovsk, which at present remain geologically and stratigraphically uncertain as to date, the "Stone Age" stations can be divided into three groups:

(a) From Khabarovsk (Krasnaia Rechka, Pokrovka) to Nikolaevsk at several sites were found stone tools (some with hoe-like wedges convex on one side), grooved chisels (sometimes very small), hammerstones, hand-

[Siberian Neolithic Period] (Izvestiia Biologo-Geograficheskogo Instituta pri Irkutskom Gosudarstvennom Universitete, Vol. 3, No. 6, p. 77, Irkutsk, 1926, Trudy Komissii po izucheniiu chetvertichnogo perioda [Transactions of the Commission for the Study of the Quaternary Period] Akad. Nauk S.S.S.R., Vol. 1, Leningrad, 1932); B. E. Petri, *Dalekoe proshloe Pribaikal'ia. I. I: Drevne-Kamennyi perioda* [Old Stone Age of Baikal Region] (Irkutsk, 1928); H. Obermaier, *Sibirien: Paläolithikum* (Realexikon der Vorgeschichte, Vol. 12, Berlin, 1932); H. Enderlein, *Der älteste Mensch in Nord-Asien und seine Kultur* (Mitteil. der Vereinig. für Völkerkunde und verwandte Wissensch., No. 7-8, Berlin, 1929-1930); G. Merhart, *Neuere Literatur über die Steinzeit Sibiriens* (Wiener Prähist. Zeitschr., Vol. 11, 1924); A. Salmons, *Neue Material in Sibirischen Museen* (IPEK, 1930, pp. 127-29), *Die Kunst des Aurignacien in Mutha* (IPEK, 1931, pp. 1-6).

For a complete bibliography of Siberian Paleolithic Periods (63 sites) see N. I. Berezin, footnote 9, pp. 38-61. *Paleolit S.S.S.R.* (a collectanea published by GAIMK, Leningrad, 1935) contains important Siberian studies by N. K. Auerbach, K. K. Flerov, V. I. Gromov, and G. P. Sosnovskii, P. P. Efimenko, *Dorodovoe obshchestvo* [Pre-gens Society] (Leningrad, 1934) includes many hitherto unpublished materials from Siberia.

Other important recent studies. G. P. Sosnovskii, *Paleoliticheskaia stoianka Severnoi Azii* [The Paleolithic Stations of North Asia] (Transactions, II International Conference of the Association for the Study of Quaternary Period in Europe, Moscow, fasc. 5, 1934); G. Debets, *Opyt Vydeleniia Kul'turnykh Kompleksov v Neolite Pribaikal'ia* [An Attempt to Define the Cultural Complexes in the Neolithic Period of Baikal Region] (Izvestiia Nauchno-Issledovatel'nogo Instituta, Fiz.-Mat. Fakult., 1 Mosk. Univ., Vol. 3, No. 2, 1930).

⁶⁷ Quoted from A. P. Okladnikov, *Ancient Monuments on the Lower Course of the Amur River* (investigations of 1935) in Otchetnaia Arkheologicheskaiia sessiia Instituta Antropologii, Arkheologii i Etnografii Akad. Nauk S.S.S.R., February 1936, p. 5 *et seq.*

flints, and abundant small flint scrapers, arrowheads, flakes, etc. The pottery consists of remains of flat-bottomed, cylindrical vessels of various forms, whose commonest ornamentation is an incised pattern of parallel zigzags forming the background for wide curved lines. The "comb," "pit," and "carved" techniques of decoration are well represented. One important group of pottery is covered with a thin layer of bright crimson pigment, polished in, and forming background for an impressed black ornament of curved ribbons. In its general character the decorations are zonal (comb impressions), spiral, double-spiral, and zigzag, with variations of the meander, and finally rhombic meshed net.

The dwellings are sturdy structures, about two meters below the surface, still retaining traces of poles and storage pits. Grouped in villages, the dwellings each cover an area eight by ten meters or more.

(b) This group differs from *a* in the less developed character of its pottery, the use of appliqué belts (rolls) superimposed over the slip, and in the presence of bomb-shaped vessels with straight, narrow necks. *Mattenkeramik* appears at these sites. The usual pigment is black applied in straight lines, occasionally on the inside of the vessels. Microliths are rarer and there are many slate knives, arrowheads, spearheads, and daggers copied from bronze originals. Cylindrical "paste" beads belong to this period.

(c) Typical pottery included large *Mattenkeramik* vessels and vase-shaped jars with slender necks, having sculptured parallel belts, and exceptionally wide, saucer-like lips, ornamented with indentations and knobs in pairs. Stone implements are rare, especially scrapers. Bone arrowheads and awls occur, while clay rings and bracelets, shell pendants, and perforated boars' tusks comprise the ornaments. Fauna include dogs, *Sus* sp., etc. Many bones of fish and heaps of shells were unearthed.

At Malmyzh traces of copper smelting were found. In the settlement near Khabarovsk burials contained bone arrowheads and pottery similar to that in the earth huts. A separate group of settlements near Sredne-Tambovsk yielded Iron Age pottery, stone implements, and iron slag not encountered higher up near Ussuri.

Iron Age. (a) Near the Ussuri River were excavated remains of *gorodishches* and earth hut settlements containing polished gray pottery similar to that from the third Neolithic group. The inhabitants probably were agriculturists and practised animal husbandry.

(b) Near Nikolaevsk, in the neighborhood of Lake Kizi and Lake Ntyr, the characteristic pottery, standing on round bases, was richly ornamented with serpentiform designs or rosettes. Bone tools occurred in prom-

fusion. There were remains of an iron industry and decadent stone implements. Hunting and fishing were predominant.

(c) Other monuments in both regions consist of remains of dwellings with stone flues (*k'ang*) and pottery of the Sung or, more probably, Ming period.

On the basis of explorations at four sites, the petroglyphs may be divided into four groups, the latest associated with the Nanai (Samoyed) "horses" resembling birds. Other petroglyphs with incised effigies of horses are evidently more ancient. Those from Serachi-Al'iensk are probably related to the Bokhai, since they suggest Chinese influence. At Kalinkova, where the most ancient petroglyphs have been found, occur "halters" crossing the anthropomorphic faces of the effigies, as they do in the Karasuk stelæ.

The relative Neolithic chronology can be established by: (a) the degree of stone technique development; (b) the evolution of pottery; and (c) stratigraphy of individual sites, as for example at Bol'shoi-Dural, Zhelekhovo, and Bukola. Thus groups *a*, *b*, and *c* above are of concurrent periods, the latest of which belongs to the era before the "Yamato" culture in Japan and Korea. The data of Chinese chroniclers regarding the I Lou and the Su-Shên ancestors of the Manchurians may be utilized to complete the history of the Neolithic period of the Amur Valley.

The assignment of the Negidal division⁶⁸ of the Amur Expedition included a study of the ethnography, physical anthropology, folklore, and archaeology of the peoples of the basin of the Amgun, the lower left confluent of the Amur, and of the area near Lakes Udyi' and Orel' and the Bichi River. The archaeological work in this region, inhabited by Negidals and a completely unexplored group known as Orel' Tungus, was to be coordinated with that under Okladnikov in the lower Amur Valley.

2. The Altai Expedition of GAIMK⁶⁹ (S. V. Kiselev, leader) cooperated with TSUDOTRANS, the Central Bureau of Highway Transportation, to preserve monuments near the Chuisk Highway from Biisk to Kosh Agach. Seven hundred monuments were examined along a stretch of 570 kilometers. An archaeological map was made of the Kuraï steppe, where small stone tumuli, containing stone graves with flexed burials, were found. There were also several ritualistic structures in the form of stone circles,

⁶⁸ K. M. Forstein-Myl'nikova, *Negidal'skii Otriad Amurskoi kompleksnoi ekspeditsii Instituta* [Negidal Division of the Amur Complex Expedition of the Institute] (Sovetskaiia Etnografiia, No. 1, 1936, pp. 154-55).

⁶⁹ N. A. Mal'tseva, *Altaiskaiia Ekspeditsiia GAIMK* [Altai Expedition of GAIMK] (Sovetskaiia Etnografiia, 1936, No. 2, pp. 153-54).

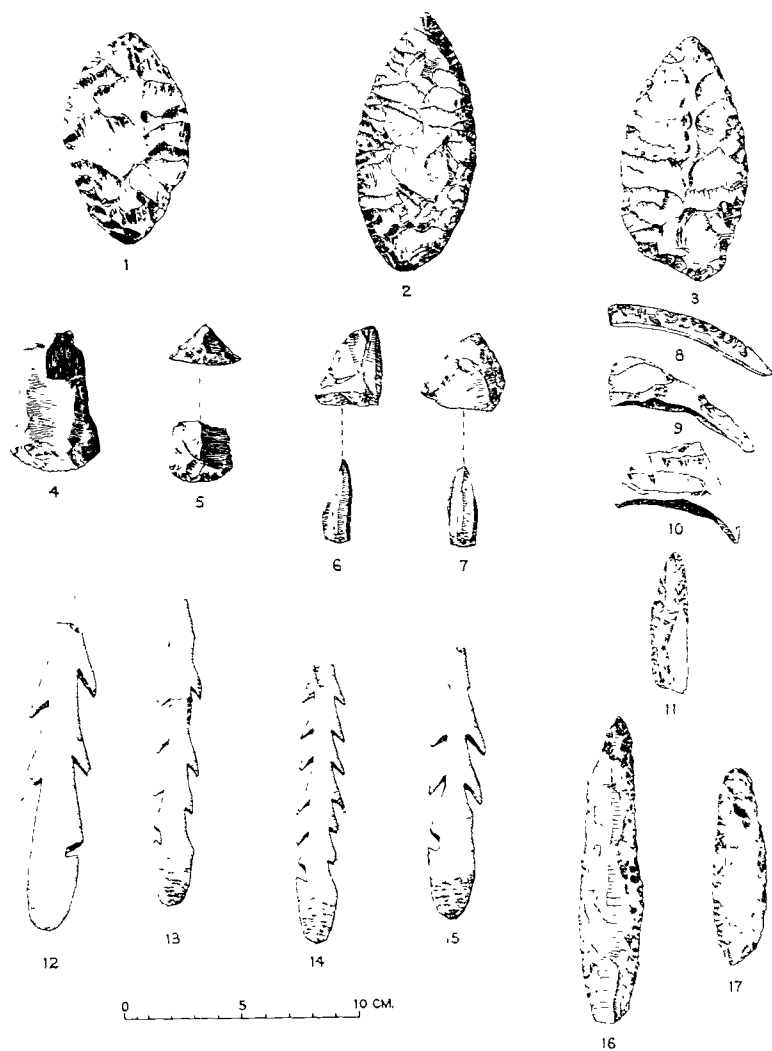


FIG. 1. Implements from Verkholskaia Gora, near Irkutsk, Siberia. 1-3, Laurel-leaf points (flinty schist), 4-5, Keeled scrapers; 6-7, Core scrapers with bilaterally retouched edges, made from truncated nuclei, 8-11, Graving tools (8, flat, 9-10, beak-shaped, 11, wedge-shaped and made from a core); 12-15, Double-barbed bone harpoon points (12-13, teeth asymmetrically opposed, 14-15, teeth symmetrical), 16-17, Elongated knives with flat lower surface and secondary retouch (flinty schist)

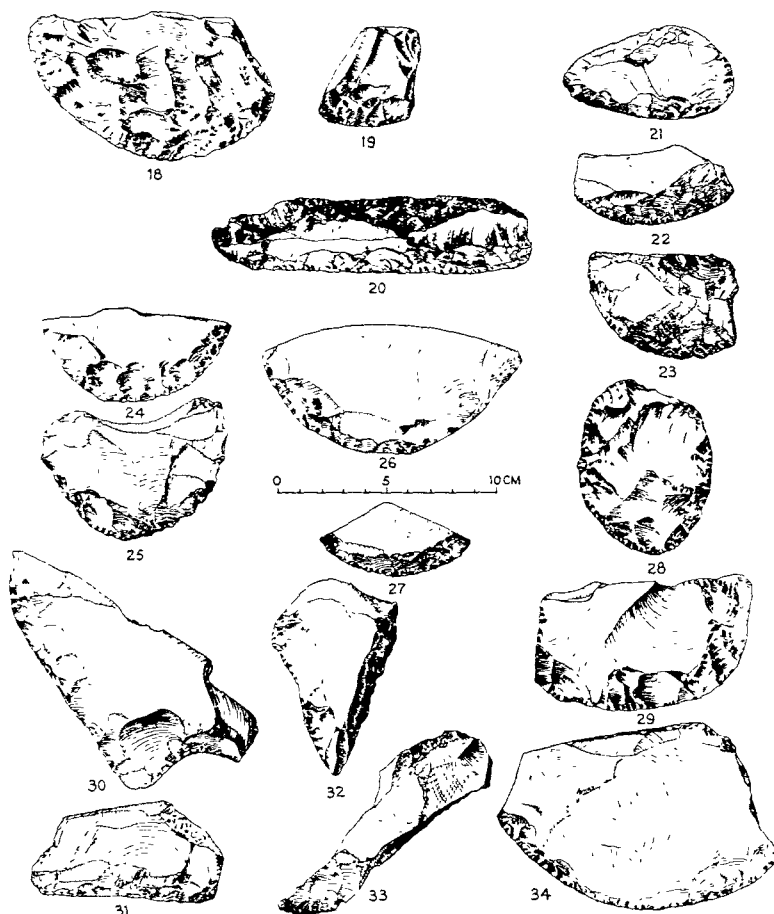


FIG. 2 Implements from Verkholenskaia Gora, near Irkutsk, Siberia. 18, 21-29, Side scrapers with convex scraping edges (flinty schist, except 28, a fine-grained quartzite) (21-23, 26-27, unilateral retouch; 18, 24-25, 28-29, bilateral retouch; 18, 23, asymmetrical point-shape; 24-27, crescent-shaped; 26-27, made of thin segments of flinty schist; 21, 28, almond-shaped); 19, Chisel-like scraper with bilateral retouching, 20, Elongated knife with flat lower surface and secondary retouch (very large; greenish flinty rock); 30-31, Straight-edged scrapers with unilateral retouch (30, made of a slab of flinty schist; 31, made of a large flake); 32, Unilateral point made out of a flint-like river pebble with cortex adhering to the butt, 33, Knife or chopper, trimmed on both upper and lower surfaces for use as a hand tool (flinty schist); 34, Convex side scraper with unilateral retouch (quartzite)

...teen meters in diameter, with burials in stone boxes, whose date is uncertain, standing on the surface of the ground. Several tumuli of the "Siberian-Scythian" Pazyryk type, with collective burials of 283 individuals, were excavated. A large tumulus, which unfortunately had been robbed, held three horses, one of them on the roof of the burial. The tumuli yielded considerable skeletal material. A wooden head of a griffin was also found.

Another important group of interments in small stone tumuli near ... village belong to the era provisionally known as the T'ang epoch. These male and female burials, accompanied by horses, contained seventh to ninth century pottery and one well-preserved bast quiver of "whistling arrows." Earrings and other jewelry were present in some of the female burials, in one case a necklace of Siberian stag (*maral*) teeth. Two mound burials held millstones forming hand-mills. This proved the contention of experts that the ancient irrigation system found in the steppe was used for agriculture, currently not practised in the region.

Three tumuli, excavated in the settlement of Tadila, yielded the Pazyryk, T'ang, and stone mound types of burials. A hoard of objects, probably the remains of the last stage of the funeral ceremony, were found above the center of the grave, only forty centimeters below the surface. These included a knife, a silver vessel with an Orkhon inscription scratched on the base, and a bridle set with a silver buckle. Another burial, near Tuiakhta, contained two horses with harness sets; a human skeleton, resting in a hollowed log, was wrapped in a green silken robe, decorated with a belt held together by buckles of stamped gold. A small bag carried magical objects and a bone whip or knife handle elaborately decorated. In addition there were golden earrings, bone inlay fragments of a Mongolian bow, and a bast quiver with "whistling arrows." An iron riveted kettle together with a sheep's bones lay near the feet of the skeleton.

3. The Siberian Expedition (B. E. Petri,⁷⁰ leader) began excavations at Verkholskaia Gora in 1923. During the removal of 156 cubic meters of loess-like silt loam deposit he found 4029 objects (figs. 1 and 2). Among the fauna identified by A. A. Belynitiskii-Birulia were: *Rhinoceros antiquitatis*, *Cervus megaceros*, *Equus hemionus*, *Equus caballus*, *Bison priscus*, *Alces alces*, *Cervus elaphus*, *Ovis ammon*, *Canis lupus*, and *Cervus tarandus*.

Two new Paleolithic sites were found, one on the slopes of Kaïsk Mountain on the outskirts of Irkutsk, the other in the Ushkanka depression five kilometers below Irkutsk on the right bank of the Angara River.

⁷⁰ B. E. Petri, *Sibirskii Paleolit. Atlas* [Siberian Paleolithic Atlas] (Irkutsk, 1927); cf. also footnote 66.

The Katsk site, discovered by M. M. Gerasimov,⁷¹ yielded flint implements associated with the mammoth, suggesting an earlier culture than that of Verkholsenskaia Gora. It may, however, be more closely related to the Aiontov Mountain culture of the Yeniseisk region brought to light by G. P. Sosnovskii and N. K. Auerbach. Fauna included *Elephas primigenius*, *Cervus tarandus*, *Alces alces*, *Equus caballus*, and *Bos* sp. (? *primigenius*). The Ushkanka site, found in 1926 by I. A. N. Khodukin, shows cultural deposits similar to those of Verkholsenskaia Gora, which Petri attributes to the late Magdalenian period.

Petri reports⁷² the discovery of a new Paleolithic site which is as much older than the Verkholsenskaia Gora [Mountain] as it is younger than Mal'ta. Preliminary excavations yielded fifteen excellent tools, not counting small items. . . . I shall proceed very shortly to the excavations on Lake Baikal, on the Angara River and the Kudu, where is my recently opened Paleolithic site. . . . M. M. Gerasimov, who opened the Neolithic graves on the Selenga River, has now left for a study of the Paleolithic sites on the Belaia River. A. P. Okladnikov leaves shortly for the excavation of the Neolithic cemeteries down the Angara River. From Leningrad has come Kazakevich, an Iron Age specialist, who is studying my excavations at Tunka [Tunke?], graves from the Genghis Khan epoch.

4. The Altai Expedition⁷³ of the State Ethnographical (formerly Russian) Museum (L. P. Potapov, ethnographer, and G. P. Sosnovskii, A. T. Kuznetsova, archaeologists) worked in the Uspensk, Elikmonarsk, Ust'-Kansk, and Ongudaï *aimaks* (territorial divisions) of the Oirat Autonomous Area and in the Biisk region. An Upper Paleolithic station near Srostki on the right bank of the Katun' River, thirty kilometers from Biisk, yielded twenty quartz implements, many flakes, nuclei, etc. This site, discovered in 1929 by S. M. Sergeev, was excavated in 1935 by G. P. Sosnovskii, who in addition to the artifacts unearthed remains of *Equus caballus*. The stratigraphy and the geological section of the deposit prove that this station is the earliest found up to the present time. In western Siberia, near Tomsk, N. F. Kashchenko⁷⁴ in 1896 discovered an Upper Paleolithic station but no stone implements.

An Upper Paleolithic site near Fominskoe on the right bank of the Ob'

⁷¹ M. M. Gerasimov, *Paleoliticheskie nakhodki u pereselencheskogo punkta v Irkutsk* [Paleolithic Finds at the Colonization Post in Irkutsk] (*Kraevedenie v Irkutskoi gub.* [Regional Investigations of the Irkutsk Province], No. 3, 1926).

⁷² In a letter from Irkutsk dated July 28, 1936, to E. H. Minns and E. J. Lindgren, published in *Man*, Vol. 37, p. 14, No. 9, London, January, 1937.

⁷³ L. P. Potapov, *Ekspeditii v Gosudarstvennogo muzeia etnografi* [The Expeditions of the State Ethnographic Museum, Leningrad] (*Sovetskaia etnografiia*, 1936, No. 1, pp. 158-60).

⁷⁴ N. F. Kashchenko, *Ein vom Menschen verzehrtes Mammuth* (*Correspondenzblatt der Deutschen Gesellschaft für Anthropologie, Ethnologie, und Urgeschichte*, 1896).

River, twenty-five kilometers north of Biisk, was discovered by I. D. Kopytov in 1913. The first excavations, begun in 1935 by G. P. Sosnovskii, confirmed the earlier identification, and among animal remains were *Diphas primigenius*, *Rhinoceros tichorhinus*, *Equus caballus*, etc.

At Eniseiskoe on the Biia River Upper Paleolithic implements were also found. These two collections were studied in the Biisk Museum.⁷⁵

A burial ground was investigated in the vicinity of Shibe, in the Ongudal *aimak*, Oirat Autonomous Area (Mountainous Altai), where a large stone tumulus with finds dating from the first century B.C. was previously excavated. A group of eight large stone tumuli of the "Scytho-Sarmatian" period was examined in the Talda Valley near Shibe and a chain of six large stone tumuli, one of them sixty meters in diameter and two meters high, larger than the famous Pazyryk tumulus, near Kurota. The large tumulus is to be excavated by the Oirat Museum. Burial mounds of Scythian and Turkish epochs were studied near Tuekta.

5. The Sale-Kharda⁷⁶ (former Obdorsk) Expedition of IAE (V. S. Adrianov, leader) excavated a settlement of the fourth century of our era near the Polui River in the Yamal National Territory, where twelve thousand objects of an unknown culture were found: 1500 stone, bone, and bronze artifacts, testifying both to a higher culture than that of the modern inhabitants, the Nentsi (Samoyeds) and the Khanta, as well as to a different type of climate; spoons, figurines, and animal heads of worked bone; ornamented combs evidently intended for high headdresses; other artifacts of bone, iron, and bronze, a copper epaulette-like clasp of the type of the so-called Pianoborskaia culture; and agricultural implements suggesting that a warmer climate once prevailed in this frigid region.

Ethnographical expeditions from IAE planned to continue work among the peoples of the Far North. In 1936 the staff concentrated on the Evenks (Tungus), Ulchis, Oroks, etc., in the basin of the Amur River and on Sakhalin and among the Voguls in the north.

6. In April, 1934, N. A. Gur'ev, the first mate of the fishing trawler "Krasnoarmeets," presented to IAE a collection of objects found in 1932 in the remains of a dugout on the shore of Tar'ia inlet of Avachinskaia Guba, three kilometers from Petropavlovsk in Kamchatka.⁷⁷

⁷⁵ N. I. Berezin, *Spravochnik po paleolitu SSSR* [Handbook to the Paleolithic Periods in the U.S.S.R.] (Trudy IAE, Vol. 12, No. 1, 1936).

⁷⁶ Cf. ESA, Vol. 10, 1936, pp. 183-84; also Fig. 4, p. 180; V. S. Adrianov, *Rezume doklada "Razvedochnaia raskopka . . . u g Sale-Kharda"* [Resumé of report, "Preliminary Excavations at the Mouth of Polui River, near Sale-Kharda, 1935"] (Otchetnaia arkhelogicheskaiia sessiia IAE, February, 1936).

⁷⁷ D. N. Lev, *Novye arkhelogicheskie pamiatniki Kamchatki* [New Archaeological Finds from Kamchatka] (Sovetskaia Etnografiia, Nos. 4-5, 1935, pp. 217-24).

According to Gur'ev⁷⁵ fifteen objects were retained by his companion, Ichthyologist K. I. Panin, and a chalcedony arrowhead, a slate hatchet, and an effigy were presented to the third mate of a Japanese boat. In the remains of the dugout Gur'ev noticed many flint fragments, potsherds (?), and bones. There were no remains of wood or charcoal. He adds:

Walking from the trawler base toward the fisheries, I found on the ground a stone resembling an arrowhead. I examined the surrounding territory and found a rectangular excavation made for the foundation of some structure. The hole, dug in the slope of a hill, was two meters deep on the mountainside. There was a worked stone imbedded in the wall, one and a half meters below the surface. Digging revealed the remains of bones, which crumbled at the least contact, and many fragments of stone of the same kind as that used in the implements. The bottom of the hole was clayey, with peat intermingled with sand at the surface. The Stone Age implements were imbedded in peat.

The objects consisted of forty-three knives, scrapers, adzes, chisels, arrowheads, spearheads, and flakes of flint, slate, quartz, and obsidian, as well as two small dark obsidian, stylized human figurines.⁷⁶ Another obsidian figurine made by pressure flaking, represents a stylized seal with well-modeled head and flippers. Similar human figurines have been found at the Neolithic station of Volosovo near Murom, at the Borki dune station near Kazan,⁷⁷ and at Kubenino on the Onega River, four kilometers from Kargopol. These together with a flint seal figurine acquired by Zenger at Zolotitsa on the White Sea are like those from the Mississippi and Ohio Valleys.⁷⁸

A number of leaf-shaped obsidian knives with angular raised backs and slightly curved blades were also found. V. I. Jochelson⁸² recorded a similar asymmetric knife in Kamchatka during 1910-1911. In 1912 such a knife (No. 1942-16), found by A. A. Shchennikov in Old Iurtishche on Kamchatka, is now in IAE. Fur cutters in Leningrad today use this type of cutting tool. Stone implements were still in use in Kamchatka at the end of the seventeenth century: Karl Ditmar, who traveled there in 1851-1855, saw a stone axe considered as a sacred object by its owner. Some of

⁷⁵ Quoted in IAE Accession No. 5180. Other details supplied by S. N. Zamiatnin, Curator of Department of Archaeology, IAE.

⁷⁶ Cf. N. K. Kourich, *Figurnye podelki iz kremnia* [Figured Flint Artifacts] (Zapiski Russ. Arheol. Soc., Old Russ and Slav Arch., Vol. 7, No. 2, St. Petersburg, 1907), pp. 242-43.

⁷⁷ In a letter dated November 21, 1934, to D. N. Lev from V. A. Gorodtsov.

⁷⁸ Cf. T. Wilson, *Arrowpoints, Spearheads and Knives of Prehistoric Times* (Annual Report, Smithsonian Institution, Washington, 1899), p. 943.

⁸² V. I. Jochelson, *Arheologicheskie issledovaniia na Kamchatke* [Archaeological Explorations on Kamchatka] (Izvestiia Gos. Russ. Geogr. Obshch., Vol. 12, No. 4, 1930), p. 359.

the older people still remembered the time when stone implements were in general use. According to other travelers crude pottery, hand made and poorly fired, was used up to the seventeenth century but was discontinued when the Cossacks introduced metal. The presence of pottery with the stone implements dates them as definitely prior to the arrival of Russians. The stone figurines are typical of the Upper Neolithic period in Volosovo. These remains may well belong to the end of the Neolithic period in Kamchatka.

SUMMARY

This report summarizes some recent results obtained by archaeologists at several hundred sites by fifty-six Soviet expeditions. Grateful acknowledgment must be made to the Directors of IAE, GAIMK, MOGAIMK, UZKOMSTARIS, and VOKS, and particularly to S. N. Zamiatnin, R. F. Marton, Leo S. Berg, L. M. Slavin, and E. B. Reilly, who have contributed the information herein contained. At Field Museum we are also grateful for the assistance of Richard A. Martin, who edited the sections dealing with pottery, and also to Martin Wilbur, who checked the Chinese translations. We wish to record our thanks to Hallam L. Movius, Jr., of Peabody Museum of Harvard University who called our attention to new discoveries in Siberia, which have been incorporated.

Since this article went to press additional reports have been received from S. N. Zamiatnin, S. S. Magura, and B. E. Petri, and summaries of recent discoveries by ANU in the Ukraine and by UZKOMSTARIS in Turkestan are expected in the near future.

The first issue of *Sovetskaia Arkheologiia* (Moscow and Leningrad, 1936), a periodical collection sponsored by IAE "to publish . . . archaeological materials, historical researches on ancient monuments, critical articles, archaeological news, and bibliographical references," has been received too late to include extracts in the present article. The current issue contains three long articles: V. I. Raudonikas, on the Onega petroglyphs; M. V. Voevodskii, a study of the technique of primitive pottery from the forest belt of European Russia; and B. Rabinovich, on the dating of Scythian tumuli of the middle Dnieper. The "Materials" section contains the reports of 1935 excavations: in the Elizavetinskoe *gorodishche* (V. A. Gorodtsov); in Shan-Koba, Crimea (S. N. Bibikov); in the Gandzha-Chai Bronze Age tumuli, Azerbaidzhan (I. A. I. Gummel'); and the Kama Expedition sites (N. A. Prokoshev). Among the important reports hitherto unpublished are those on the 1927 excavations at Pashkovskaia, Kuban region (M. V. Pokrovskii), the Bronze Age tumuli at Kuznetsovka, near Moscow (S. V. Kiselev), 1930 excavations, Medieval tumuli in the Novgorod region

(A. V. Artsikhovskii); 1933-34 excavations in the Kalmyck region (P. S. Rykov); Neolithic stone fish from eastern Siberia, a study by A. P. Okladnikov. The news notes comprise accounts of the 1932 excavations in eastern Sakhalin by A. M. Zolotarev, as well as 1935 results from Salekharda, Amur Valley, Novgorod, Saian Altai, and Koban culture sites in North Ossetia. An extensive summary of recent Central American archaeology is given in the bibliographical section. The volume is extensively illustrated, and includes brief resumé's in French.

The authors hope that this method of presenting the recent archaeological results according to geographical areas will inform the student of the work being done in the U.S.S.R.

FIELD MUSEUM OF NATURAL HISTORY
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THE Hopi regard life as predetermined in all important respects. All that has ever happened or will happen has been known from the beginning. This is so fundamental that even the most recent innovations in material culture are considered in the same light, and one is assured that their grandfathers predicted the coming of these things. But the important activities, those which are essential in following the Hopi road, are the ceremonies which play such a large part in their lives. They include various magical techniques for obtaining rain, crops, health, happiness, and long life, and have been described at great length in the literature.

All these things which are desired are not separable, but are linked into one essential whole. This is made quite explicit in the speeches made to the departing katchinas at the end of a dance.

We, here, have worked all day for you and you have correctly spoken. When you arrive yonder, at your homes, these our words you will carry to them. From there you will soon come to make us drink. Around here our different kinds of plants are doing poorly, so you will come soon to make them drink. You, having brought our plants drink, they will drink, their children [corn ears] will grow, they will have eyes [kernels], our children the little ones will eat them and surely will be happy. Then all the people will live happily. Then our lives reaching to old age among our children will happily be fulfilled.

In every ceremony, the spirits of the dead are involved, whether as katchina, clouds, or those living in the underworld. The year is divided by the calendar into two halves, with six named months of the winter season repeated in the summer.² If the Snake Dance is to be held in the August moon, it is believed that it will be performed in the underworld in the January moon of the same name. Consequently, there is a one night meeting of the society in January to participate in the rites of the underworld, and those in the underworld will reciprocate in August. This holds good for every ceremony, and in every one a hole is opened in the floor of the kiva to represent the place of emergence and to provide means of communication with the underworld. The climax of the initiation of the boys into the four societies generally referred to as the Wôwotcim occurs the night on which all the roads to the village are closed and the spirits of the dead

¹ This paper is based on field work conducted under the auspices of the Department of Anthropology of Columbia University during a period of nine months in 1934-35.

² J. W. Fewkes, *Tusayan Katchinas* (Fifteenth Annual Report, Bureau of American Ethnology, pp. 245-313, 1897), p. 258. The system does not balance exactly, but in theory it should.

return. All the women and children gather in houses on one side of the village and dare not come out at all. It is a dangerous period. At the conclusion of every rite a discharming occurs to break the contact of the living with the dead. Even the weaving of wedding garments for a bride is to provide her with clothing for the future life.³

Despite this preoccupation, there is the greatest reluctance to speak of the dead, the underworld, or even to identify the *katsina* with the dead. A man is buried with a "cloud mask" of cotton batting, but discussion of the subject is avoided.⁴

The psychological reason for this reaction is related to ideas concerning the role of will in human life. In the literature so much emphasis has been placed upon the magical devices which produce an automatic response that the subjective state of mind has been neglected. It is imperative that there be no quarrel, nor dissension during any ceremony, since all must concentrate their will upon obtaining the desired result. The word *na'wakna* means not only "to want," but "to pray," "to wish," and "to will." Thus, it is often said that it is unnecessary to speak one's prayer, since mere willing or transfer of the wish by means of the breath to a prayer stick or prayer meal has the same effect. The essential idea seems to be that by concentration the collective will is projected so that the powers are compelled to comply. Should there be even one whose "heart is not good" the efficacy of the whole is destroyed. Hence, the admonition of the crier in announcing every ceremony is to cease all quarrels. That this was no empty form was impressed upon me by the fact that a man refused to discuss an important matter with the village chief after the Winter Solstice had been announced for fear that he might disturb the chief's heart.

All these ideas of unanimity of will directed to attaining health, long life, happiness, and holding fast to the Hopi road are made explicit insofar as they concern the people as a whole. But they are equally true of individual lives, which are the whole writ small. Every individual has his own road to follow, his own will which he concentrates upon keeping happy, healthy, and arriving at old age. He, too, must live without mental conflict, worry, or trouble since these destroy his will and consequently lead to unhappiness, sickness, and death. A man who thinks of the dead or of the future life instead of being concerned with worldly activities is thereby bringing about his own death. In folklore are tales of people who wonder

³ P. Beaglehole, *Ceremonial Data from Two Hopi Villages* (American Anthropologist, Vol. 37, pp. 41-54, 1935), p. 48.

⁴ E. and P. Beaglehole, *Hopi of the Second Mesa* (Memoirs, American Anthropological Association, No. 44, 1935).

About what happens to the dead, and are given the opportunity to visit the underworld by magical means, and return to life to tell the people about it.⁵ In such tales the living are urged not to be lonesome and not to long for the deceased.

Go back and tell them in the village that we are living here, and here we live in the dark, and tell them that no one should wish to come here. For some it is not yet at all time to come, but if their hearts are not good and they are angry they will come here sooner, so tell them no one should wish to travel this way. . . . Now you must not think about that any more. You must go home and live there strong.⁶

These tales are a reflection of a rather common experience. Many men have dreamed of visiting the underworld, but they always phrase the experience as dying and coming to life again. The conclusion is always the same. Since then they have been strong and have not thought about such things. The Hopi word *öqa'la* is translated "strong," but it refers to strength of will, mind, or spirit, and is never used in reference to physical characteristics alone. A strong man always gets his wish and is able to set aside all trouble, and concentrate upon following his road in this life to the end.

The essential unity of these ideas in individual lives is clearly indicated in the following advice from a father to his son, which I have translated quite literally:

My child, tomorrow morning you will go for a bath. Just as the sun comes out you will pray (*na'wakna*) that your life shall be good. Then the sun will come out and give you good life. And you shall live happily. Here you happily will work for me and I'll eat those good things. With them I will grow strong. I shall continue to live well. I won't be sick. Going on [my road] I shall always be happy. And these people having something to live by will think [only] of continuing their good lives. And having made the good life for them, don't ever be mean [angry]. Whoever is not mean will live long. Whoever is mean will surely die. Therefore anyone who is happy always sings. And so go take your bath. If you do that you will be strong, and your mothers and fathers [clouds, katchinas, the dead] will be happy when you work for them. They will be parents to your plants. One who lives thinking this way has a peaceful (*ho'pi*) life and is always happy.

An example of a man who was able by his strength of will alone to obtain what he desired was told to me by his nephews and nieces. He was the former village chief of Shipaulovi, and all during the last years of his life he prayed that an eagle would nest close to the village. At that time

⁵ H. R. Voth, *The Traditions of the Hopi* (Publications, Field Museum of Natural History, Anthropological Series, Vol. 8, 1905), pp. 109-19.

⁶ *Ibid.*, p. 119.

the eagle trapping grounds of the Bear clan were at Canyon Diablo, thirty miles east of Flagstaff. But now his prayer has been answered and for the last few years since his death, eagles have nested just two miles east of the village, and his descendants no longer need make the long trip for eagles before Home Dance. "He was really strong in those things."

Within this system, the idea of chance or accident is virtually ruled out of existence. For every sickness there is a cure, and death following illness is attributed to the will or lack of strength of the individual rather than to any deficiency in the curing techniques. When an old person dies he is regarded as having reached the end of his road. This is best illustrated by the manner in which the old are treated while still living. In one instance, an old man asked his nephew about the new Hopi Constitution, which was being discussed on all sides. His nephew replied, "You don't have to think about that. It is for us young people. You are going to die pretty soon, so you don't have to bother yourself about it." On another occasion a teacher reprimanded a young man for permitting his old father to carry a heavy load up the mesa without making a move to assist him. The answer was, "He's an old man. He has to die sometime." The same attitude toward death occurs in a tale in which an old man sees a Navaho approaching and believes that he intends to kill him. "After all, it can't be helped. After all, I am an old man."⁷ In all of these cases there is a tacit recognition of the fact that "the appointed time was near" and acceptance of it.

However, there is no calm acceptance of an unavoidable reality when one who is young or in active middle years dies. The prescribed reaction is: "You don't really feel bad about it because you know that he is all right and that he is happy. For the first year, when you think of him, you throw aside a little food, but after that you don't think about it any more." Actually they are not able to adjust themselves to a difficult situation so easily, and the reaction of any individual to a relative's death will depend upon his interpretation of the essential cause of the death. He can regard it as caused by some trouble, a conflict stronger than the will to overcome it, or as the dead man's deliberate premeditated willing of his own death.

The role of the will and its importance in situations of internal conflict can be best illustrated by a case in which no death occurred. A woman had been suffering from hemorrhages following a miscarriage. The American doctors at the government hospital were unable to stop the flow of blood and at her request permitted her to visit a Hopi doctor at First

⁷ Both sentences in the Hopi are introduced by the particle *paipi*, which I have here translated "after all." Actually it cannot be translated, but indicates resignation to the inevitable.

Mesa, who was successful. The doctor was surprised to find her alive when he met her a month later, but she had never had the slightest doubt of the outcome since she was at that time "strong" and happy. Yet the same person later lived in a constant state of anxiety for seven months, when there was apparently nothing wrong with her physically, fearing that she was going to die. Her husband to whom she was very much attached had left her and she was unable to adjust herself to the new situation. She had no desire to die, and her whole struggle was to put all that trouble out of her head. One day I was reading a text which she was translating. It was a tale about Ma'sawi, the deity associated with death. Suddenly, she burst out crying and told me not to read any more. When I rose to leave she begged me to read her something else, to talk to her—anything to get her mind away from the subject of death. Many months later when her husband's name was brought into conversation she remarked, "See, I am all right now. I can talk about him and it doesn't trouble me. I've put all that aside."

When a woman died whose house had been used as a meeting place for one faction in a dispute which had agitated the village for years, her death was attributed to all that trouble being too much for her. In another case a young man died who had been trained for chieftainship since childhood. His sister believed that he was too young and that thinking about all those serious things had been too great a responsibility and had killed him. Another instance involved a separation of husband and wife after a rather violent dispute, in which he refused to leave the house since he had no relatives to whom he could go. The wife moved out and in the course of the next two years she was married successively to two other men. Then their young son, who had been ill for some time, told his father that he thought he would get well if they lived together again. His mother returned immediately to her first husband, believing that if the trouble between herself and her husband was the cause of his sickness he would get well. "So I came back to L, but he died anyway."

Examples of this type of interpretation are common, but those in which an individual actively wills his own death are more difficult to obtain. Such a person is called *qövi'sa*. The Hopi describe him as sensitive or thin skinned and quick to take offense. Consequently when offended he wills to die in order that the relative who has hurt him will be bereft. He is conceived to be motivated by the desire for revenge and spite. And the invariable reaction to the death of a *qövi'sa* is anger associated with ideas of personal reference. One woman slapped the face of a corpse in her anger and cried, "You are mean to do this to me." Another case concerned two

brothers who had been chums all through their childhood and school days. While they were in school, they made many plans about what they were going to do when they returned to the village. Shortly after their return, the younger brother drowned while swimming in a lake near Flagstaff. The other stood on the shore weeping and said, "All right, damn you, go ahead and die. You just have to spoil everything we've planned together." The third example of a *qóvi'sa's* death occurred during the summer of 1936. A woman who had been ill for months died on the dance day of the Shipaulovi Niman Katsina ceremony. That was enough to spoil the whole dance when everyone was supposed to be happy, but, what made it infinitely worse, she was affiliated with the kiva putting on the dance and her brother was the chief for that day. The universal reaction was anger. The clearest explanation came from a man who said that she was the meanest person he had ever heard of. "She could have died yesterday or waited until tomorrow, but she deliberately chose that day to spoil the dance."

These examples illustrate three distinct attitudes toward death. Yet they are all intelligible and consistent with the great emphasis that Hopi culture places upon the role of individual will in what is otherwise a deterministic view of the course of human life.

NEW YORK CITY

³ This was told to me when the living brother was drunk, and there was no possibility of mistaking the affect or that he believed that his brother's death was intentional.

NAMES AND NAMING CUSTOMS AMONG
THE MASHONA IN SOUTHERN
RHODESIA

By HEINZ WIESCHHOFF

DURING my work among the Mashona in southeast Africa I had the opportunity to collect some data concerning personal names and their meaning, as well as to investigate the customs involved in the naming of newly born children. In general this domain of ethnography has been neglected in field work, although it is rather interesting in itself and somewhat important for general ethnological questions. We know from the study of our own names that they sometimes represent relics of older customs and manners and that name-etymology may throw some light on past cultural periods. The discoveries are not very exciting but nevertheless of some significance for detailed research. Thus some names, giving, as they do, invaluable hints as to religious and other social attitudes of the Mashona, are a genuine stimulus to ethnological investigation among these interesting people.

I would like, accordingly, to present some notes made among the following tribes, all of them subdivisions of the Mashona group:

1. Baduma, living under their chief Makorre in the Chikwanda Reservation, some thirty miles northeast of Fort Victoria in Southern Rhodesia.

2. Wahungwe, living in the Makoni district under their chief Makoni, about forty miles north of Rusape in Southern Rhodesia.

3. Babudja, living in the M'toko district in the northeastern parts of Southern Rhodesia. Their chief is called M'toko.

4. Wataware, the northern neighbors of the Babudja, living in the eastern parts of the Mt. Darwin district of Southern Rhodesia. Part of the tribe extends into Portuguese East Africa.

5. Barue, inhabiting Portuguese East Africa around the junction of the Gaeresi and Ruenya Rivers. Their chief is Macombe.

6. Wawesa, the western neighbors of the Barue, living west of the Ruenya River in Southern Rhodesia in the northern parts of the Inyanga district.

NAMING CUSTOMS

All these tribes are culturally very closely related. Their social organization and material culture are also remarkably similar, but there are some differences in naming regulations. It is a common custom among these tribes for all individuals, men and women, to acquire at least two names during their lives, sometimes even three and more being bestowed. Only in a very few exceptional cases is one name retained throughout

life. The first name is generally given immediately after birth, the second name when maturity is reached: customarily for boys when reaching about twelve or fourteen years of age, and girls when they marry. There are, however, a few exceptions to this rule.

The Baduma and Wahungwe give the first name to the newly born child, whether it is a boy or a girl, on the third day. This first name is chosen by the wet-nurse and only occasionally by the mother herself. No ceremony whatsoever takes place with this naming, although the birth itself is considered an event of great importance and is celebrated in different ways. The second name is either given by the father or chosen by the individual himself. This naming also is done without any kind of celebration.

In contrast to this, among the Babudja the father alone is allowed to select the first name. After the birth, the wet-nurse (any old woman of the village who assisted at the birth) calls on the father who is waiting for the message with his friends at a certain place in the village. She asks him for the name selected by him and returns to inform the mother of his decision. Thus the name is given immediately on the day after birth, at the latest on the second day after. The second name, on the other hand, is mostly selected by the mother in later years. Only in rare cases is the first name kept throughout life. I did not find any such example, but some of the Babudja assured me that it happens occasionally. This second name is accepted at the age of about twelve or thirteen years.

Among the Wataware the father selects the first as well as the second name. Sometimes, however, the child upon reaching maturity chooses the second name by itself. The mother is never allowed to select the name. Similarly, among the Wawesa the mother has nothing to do with the naming of the child, but the father has only the right to give the second name, the first one is casually given by the wet-nurse. The first naming takes place in both tribes immediately after the birth, the second in the manner just described.

The Barue do not give the first name before the child is six months old. They are particularly strict in this respect. For the first half year they call the male baby marumbra, the female ntsiye. After this the father gives the names to the boys and the mother to the girls. The second name is selected by each individual for himself, or is a nickname adopted as a result of frequent use by friends.

MEANING OF NAMES

Names are of no little concern for the Mashona: names are closely connected with the individuals and they regard them as part of their ex-

ance. The Babudja believe that if a child cries habitually it is a sign that the name is inadequate to satisfy some demand of its existence. Thus the spirits indicate through this crying that the name must be changed and another selected. Sometimes many name changes are necessary. From the Wawesa and Wahungwe I heard that the names are also changed if a person becomes seriously sick. The nganga (doctor) who treats the patient selects the new name as often as is necessary, because it is believed that misfortune is connected with the name.

The naming of the children of the M'toko, the paramount chief of the Babudja, was regarded as very important. As the chief is dependent in many respects on the Charewa, a kind of high priestess, it was her task to select the names of all the royal heirs.

NAMES OF THE BADUMA

Examples of first names:

1. Vumba (g): "The mother gave this name to her daughter because all her former children died." Literally ku fumba is a verb meaning "cause to die." It is especially used by women who lose many children through death.¹

2. Gunyei (g): "At the birth one did not know whether the child was alive or dead."

3. Pedzi (g): "The mother said at the birth: 'This is my last child'." Literally ku pedza means "to finish"; pedzi is a derivative noun meaning "the finisher."

4. Kapera (g): "As the child was born, many children died in the village." Ku pera means "to be finished, to come to an end." The name is derived from this verb, indicating "This child will also come to an end."

5. Morombe (g): "It is the child of a poor man." The word is formed from rombe meaning "poor man."

6. Balala (b). "At birth the child was so thin that everyone thought it would die." This name derives from ku la meaning "to eat" and ku la-la meaning "to eat much." Thus the name indicates: "You must eat much in order to grow."

7. Kokudza (b): "The child shall not live very long." There seems to be no suitable analysis at hand.

8. Mosamura (b): "As all the previous children of the mother had died, she believed that this child would also die." Ku samura means "to lift hand to strike." Thus the name may denote: "The spirit of some kind or other will lift his hand to strike it."

9. Senwe (b): "The child was very feeble when it was born." Literally senwe means "a dry stalk of grain," hence the thin child was compared with such a stalk.

10. Matope (b): "This shall be the last child." No further explanation was offered.

¹ The explanations given by the natives are given in quotations; g indicates the name of a girl, b the name of a boy.

11. Megotsu (b): "The nurse said at the birth: 'I got it from the mother'."

12. Musenda (b): "The name means a nightmare." According to the belief of the natives the spirit of an ancestor enters a person and tells him that this or that should be done. This happened to the mother of this child shortly before its birth. Therefore the name "nightmare" was chosen.

13. Maturuke (b): "During the birth a heavy rain commenced." This name derives from ku turuka meaning "to set in," especially used of rain or the rainy season.

14. Muneva (b): "It was a very thin child." Muneva is a kind of net bag. The child was compared with it.

Examples of second names:

1. Makadisa (g): "She was always very selfish." The name derives from ku disa meaning "to like very much." It was given because as a girl she always wanted everything.

2. Mushongo (b). The father gave this name to his one and only son. The meaning is: "In spite of all kinds of medicine no further son has been born." The literal meaning is "medicine."

3. Vembo (b): "The boy wanted in his youth to eat only mushuku" (that is the fruit of the loquat tree). Vembo means literally "mushuku-juice."

NAMES OF THE WAHUNGWE²

I. 1. Dzifamba (b). Ku famba means "to go." The reason why this name was given is unknown. 2. Dina: "He sat down wherever he went." Dina literally means "a nice sheltered place." This name was given because he was always looking for a comfortable resting place.

II. 1. Masiyiwa (b). "My father left me." The father died shortly before the birth of the child. Literally ku siyiwa means "to be left behind." 2. Shilling: English coin.

III. 1. Dzaramba (b): "He shall become strong." The word derives from ku ramba meaning "to persist." 2. Matemba: It is the name of a steep mountain.

IV. 1. Chivodzi (g): "It is a delicate child." This word seems to derive from ku vodza meaning "to make bad." The literal translation would be "the bad-maker." 2. Kadenere. "She was born under a tree." Denere means thicket.

V. 1. Chiriga (g): "She is the girl of poor parents." The name derives from ku riga meaning "to rob of everything." 2. Kademo: Demo simply means a "hatchet." There is no explanation as to why this name was given.

NAMES OF THE BABUDJA

I. 1. Kakosora (g). "The girl was almost blind after birth." During pregnancy the mother had killed a snake, an act which is taboo and causes the child to be afflicted, according to the native belief. After the nurse had washed the eyes with warm water, they were cured. Ku kosora literally means "to cough." Apparently, Kakosora

² Among the following tribes I collected both names for single individuals. 1 indicates the first name, 2 the second.

derives from this word. 2. Seli: This is the name of the grandmother. The meaning is unknown.

II. 1. Chuma (g): The name is the translation of "beads." 2. Makandipei: "The girl was very avaricious in her youth. She never gave anything away; she always asked, 'What did you give me?' " It is a form of ku pa meaning "to give." The translation is, "What did you give me?"

III. 1. Shangwa (g): "At the time of birth, food was very rare in the village." Literally shangwa means "famine." 2. Maya: Meaning unknown.

IV. 1. Kamali (b): "At the time of birth many children died in the village. This child lived because the midzimu (spirits) helped him." Kamali is the name of such a spirit. 2. Mappa: Meaning unknown.

V. 1. Kamoto (b): "It was a difficult birth, the mother cried and said: 'It burns like fire.'" The name derives from moto meaning "fire." 2. Kadenga: "He has always been a feeble boy." Ku denga-denga means "to be feeble."

VI. 1. Kaseko (b): "All the women in the village laughed about his mother because she had no children, but now she has a son." The name derives from ku seka meaning "to mock, to ridicule." The second name is not yet given.

VII. 1. Katura (b): "'I feel better now,' said the mother after the birth." Ku tura means literally "to take a load from the head or shoulder." 2. Goso: Meaning unknown.

VIII. 1. Siko (g) "The girl cried the first day." No explanation could be found. 2. Magara: "She was always despairing, she always said: 'We are standing here and do not know what to do'." The name derives from ku gara meaning "to sit, to stay."

IX. 1. Muhende (b): "At the birth of the child the mother said: 'Perhaps this child will also go away.' Many children died at that time in the village." It seems to be a derivative of ku enda meaning "to go." The second name not yet given.

X. 1. Dambudso (g). "The mother had many troubles and pains before the birth of the girl." Dambudso is, literally, "affliction, trouble." 2. Makuwere: "The white people enter the country."

NAMES OF THE WATAWARE

I. 1. Chipei (g): "The mother says: 'What shall I give the child?' There was a famine in the country." The word derives from ku pa meaning "to give." 2. Kalioga: "She is alone." This is a form of oga meaning "alone."

II. 1. Mandipedza (g): "At the birth of the child, the period in which so many children died came to an end." The name is a compound of ku pedza meaning "to finish." 2. Amadika: "At first the husband loved me, but later ceased to." The name derives from ku dikana meaning "to be beloved."

III. 1. Karombe (g): "She is poor; she has no child. People spoke in this way about the mother of this child." The word derives from rombe meaning "poor man, poor person." 2. Ndoro: Ndoro is a round, white ornament worn on the head and breast. Originally it was a mussel shell and only chiefs and priests were allowed to wear them.

IV. 1. Shopo (b): Shopo is a kind of spear with which oxen were sacrificed on special occasions. They no longer have this spear. 2. Nyambewu: "He always had many kinds of seeds for his garden." Mbewu means "seeds."

V. 1. Siti (b): The name of an uncle, who died shortly before the birth of the child. The meaning is unknown. 2. Kagora: "Name of an animal, which was given, because he was poor and had to work all the day in his garden." Gora is a large wild cat.

VI. 1. Marara (b): This name means "dirt." No explanation was given for using this appellation. 2. Chadamunda: "The monkeys always came into his garden and ate all the fruits." Munda means "garden."

VII. 1. Kambalame (g): Name of a bird. 2. Sikweno: "If she came to another family, she was shy and timid, because she did not know them."

VIII. 1. Nyengera (b): "When a man has much to eat, one must ask him to give something to others." The literal translation of ku nyengera would be "to cheat." Whether there is a relation between the meaning of both is doubtful. 2. Matakaira: "He is always running around." Literally ku nekaira means "to walk to and fro."

NAMES OF THE WAWESA

I. 1. Nyamadsawu (b): "One child after another died. The parents thought that an evil spirit had bewitched them and therefore they said to the spirit: 'This is your food.'" 2. Musappa: It is the name of an uncle, who died shortly before the birth of the child. This name was given in his remembrance. The meaning of the name must be "soil without stones."

II. 1. Huma (b): "It has such a large head." Literally huma means "forehead." 2. Mushoshomu: "When he came to a river, he said: 'I see a river.' When he had seen a house, he said: 'I have seen a house.'" The meaning is "talker." Literally a derivative of ku shoshomu meaning "to be talkative."

III. 1. Nyanzangusa (b): "I am poor, as wild animals have killed all my brothers." 2. Chimivenda: "He always could walk nicely and fast. He had an accident and since then he cannot walk well." This name is a form of ku wenda meaning "to walk."

IV. 1. Chisingwe (g): "Perhaps this child will die." 2. Amulima: "She is poor."

NAMES OF THE BARUE

I. 1. Muchese (b): "The father of this boy was a very able smith." Literally chese means "blade of knife or hatchet," which the father probably made with great skill. 2. Kudangilana: "Friends do not like me; friends always have fights and discussions with me."

II. 1. Tawurani (b): "You may speak; I do not give you an answer." This name derives from ku tawurirana meaning "to converse." 2. Muteye: "He is always hunting animals." The name derives from ku teya meaning "to catch animals in a trap."

III. 1. Mutsunwa (b): "The father thought that this feeble child would soon die and said: 'I shall not see it very long.'" 2. Umtali: A town in Southern Rhodesia

IV. 1. Muhamba (b): "Now you die. A son and a daughter had died and therefore the father thought that this child would also die." No further explanation is possible. 2. Makombe: The name of the paramount chief.

V. 1. Muganiva (b): "Somebody tried to bewitch the child. The father said: 'Bewitch it; I cannot help it; I have no medicine to prevent it'." 2. Topwe: "This is the name of a special vegetable, which he liked to eat."

SOME OBSERVATIONS

In summarizing the material presented, we can say that the first names among all the Mashona divisions are predominantly connected with events which took place at the time of birth. These events give the stem for the personal name: there was a rain-storm during the birth; the birth was difficult; the child was apparently blind; the child was feeble, etc. But the most significant feature is that most of the names express more or less an attitude of fate and resignation: the child will die, we cannot help it; the food is scarce, how shall we feed the child; the child is thin, it will not live very long; all the previous children died, so this one will also die; a spirit has bewitched the child, he may have it. Such is the prevailing spirit of the first names. It is really astonishing to what extent the possible death of little children is predicted in the names. Only occasionally are the first names governed by other circumstances in the individuals' environment: a relative died and his name is given in remembrance of him, or the names of tools and implements are selected. Although the personal name is regarded as very important and once acquired is believed to be a very important part of the individual, the name association is not involved with any kind of spiritual meaning other than those mentioned in connection with sickness, in which case the name is regarded as ill-omened.

The second name normally reveals individual behavior: he sits down wherever he goes; friends do not like him; he talks too much; she is selfish; she has no child, etc. These names are willingly accepted and applied with frankness, although they are not as a rule flattering according to Mashona understanding. Yet the names indicate at the same time a latent understanding of good social form among these people. It is not considered good social form to sit down wherever one goes, to talk too much, to be selfish, etc. Although the sobriquet-names are not flattering, they are used publicly and repeated without embarrassment to the feelings of their owners.

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PUEBLO SOCIAL ORGANIZATION AS
A LEAD TO PUEBLO HISTORY

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IT is surprising to note how many anthropologists not specializing in studies of the modern Southwestern Pueblos still consider them all to be characterized by the group of traits collected in the late 1800's by early students of the Western Pueblos. After the detailed work of Parsons, Strong, and Eggan, all of whom have pointed out the differences between the Eastern and the Western Pueblos and who have suggested not only specific centers of origin for the various traits of social organization but also the changes made in these traits as they were borrowed by one group from another, it is surprising to find any student still complacently cataloguing all Pueblos as matrilineal, democratic, and peaceful. The misfortune in this is not merely one of misinformation, it is rather that in the differences apparent in the social structure of the various Pueblo groups, correlated with their differing linguistic identities, we find a key to the diverse history of these groups.

In 1924 Parsons¹ made the first division of the Southwestern Pueblos into an Eastern and a Western group on the basis of social structure. She reiterated this division in 1929² and again in 1936.³ She makes a point of the Tewa center of moieties, the Hopi and Zuni center of clans, and the Keresan concentration on societies, and points out that the eastern clan is a marginal trait rather than decadent, that clanship and some ideas of society were borrowed from the south by the Tewa, and that some things, such as masks and prayer sticks, were borrowed by the Tewa from the western center of the kachina cult. No attempt was made to formulate periods of prehistoric development of any of these traits nor of the borrowing of these traits, and no suggestion was made regarding the original source of the Pueblo peoples of the Eastern or Western divisions.

In 1927 Strong,⁴ interested in the history and outside relationships of the Southwestern Indians, charted the presence and absence of a number of the social factors characterizing their tribes. These plotted traits were: warlike tendency, moiety system, group fetish, group priest, group house, non-localized clan, localized clan, moiety descent, clan descent, lineage descent, moiety exogamy, and clan exogamy. From his survey he restated Parsons' two general divisions of the Pueblos, but he made no further

¹ Parsons, *Tewa Kin, Clan, and Moiety*.

² Parsons, *Social Organization of the Tewa of New Mexico*.

³ Parsons, *Taos Pueblo*.

⁴ Strong, *An Analysis of Southwestern Society*.

divisions. He did attempt to work out the periods of a localized common history for the two groups of Pueblos, and he postulated a close connection between the Indian groups of southern California and the Southwestern Pueblos.

Since I do not agree with Strong's reconstruction, it is only fair to outline it here. The two Pueblo groups, Eastern and Western, were thought to have developed in the Southwest from a hypothetical early prehistoric period when their only social organization consisted of the bilateral family. From this stage they developed recognition of lineages and of the group house-fetish-priest complex. Patrilineal moieties were universal. Strong believes that at this time the Pueblos were probably connected with and influenced by the Shoshonean and Hokan groups of southern California and with the Pima-Papago of southern Arizona, who are still marked by these traits.

In the succeeding undated period the lineages are supposed to have fused into clans and to have grouped themselves into towns, the men of the Western Pueblo group, including the Hopi Shoshoneans, the Zuni, and the Western Keresans, gallantly settling down to the custom of female house ownership and care of the group fetish. The men of the Eastern group, including the Tewa, Tiwa, probably the Towa, and the Eastern Keresans, more masculinely developed the custom of male ownership of houses. Strong is undecided as to which sex then held custody of the Eastern Pueblo fetishes. The fetish-priest-clan house combination and the type of house ownership are given as shaping the Southwestern clan patterns, which he thinks go back to a multiple rather than to a single origin.

The important item in the hypothetical reconstructed history at this point is the differentiation between the matrilineal system of the west and the patrilineal system of the east. This he makes more apparent in the next period, when, in the west, the females continued to own the family residences and proceeded to develop matrilineal clans, a system which is suggested to have spread from these Pueblos to the adjoining Apache and Navajo groups.

In the east, the Rio Grande Pueblo men clung to their house ownership and to their patrilineal moieties. In contrast to Parsons' idea of the eastern clan being a marginal trait, he believes that the clans which had developed in the east faded into their present state of decadent insignificance. In the next period the two kiva system was evolved by the male-conscious Easterners, while the Westerners concentrated on strong matrilineal clans and on societies closely connected with the clans. These societies were under the inherited leadership of men chosen by the oldest woman of the

clan by which the society was led. This was the period of large towns, elaboration of fraternities, masks, altars, town governments, and ceremonies; a period in which the rituals of the recently arrived Navajo and Apache received considerable elaboration from Pueblo influence.

At best, reconstruction of prehistory is dangerous, in that a consideration of every item of culture make-up is necessary for valid conclusions, and yet our only data on ancient social organization and its history must come from modern peoples. Obviously, however, the student must consider every known factor and should weigh the reconstruction with regard to each item which has not been considered or which appears to contradict the hypotheses. Two important factors which might bear evidence for or against chapters of reconstructed Southwestern Pueblo prehistory, and which were not considered by Strong and but slightly by Parsons, are physical anthropology and linguistic grouping and the correlation of these with detailed studies of Pueblo social organization. This correlation suggests a history of the modern Pueblos different from that given by Strong and going back farther than that given by Parsons.

Unfortunately we have barely been introduced to the skeletons in the closets of the Southwestern past. In his work on Pecos Pueblo,⁵ Hooton found a number of physical types represented, and Hrdlička agrees that the modern Pueblo Indians are not a homogeneous group but are made up of what appear to be considerable accretions from various stocks.⁶

Grouping of the Pueblos on linguistic grounds and on those of social organization has been worked out in sufficient detail for our use in historical reconstruction, and these two, correlated, suggest a picture of Pueblo history significantly different from that of local development of the social organization of both Eastern and Western Pueblos from a prehistoric Southwestern prototype. Briefly, this history is one of divergence of Pueblo origins, of immigration of the various groups making up the Eastern division into the Southwest at a period when their social organization was already highly developed and in a form not far different from that seen in the fundamentals of their organization today, and of influence upon these groups from the western Zuñi and Hopi, who are probably older in the Southwest and whose origins were probably in the regions farther to the west or to the south. In support of this theory it is necessary to briefly set forth the linguistic grouping and the fundamental elements of social organization of each of the Pueblos involved.

The language of a people is perhaps their most stable characteristic,

⁵ Hooton, *Indians of Pecos Pueblo*

⁶ Hrdlička, *The Pueblos*

as is evidenced when one notes that the Tewa village of Hano, built to adjoin the Hopi village of Sichimovi about 250 years ago, shows a considerable measure of Hopi acculturation but a stubborn tenacity in holding to the Tewa language. These Tewa speak Hopi to their next door neighbors, yet as a foreign tongue. A similar situation is seen among the Laguna colonists in Isleta.

In his scholarly paper on *Social Organization of the Western Pueblos*, accepted as a thesis by the University of Chicago in 1934, Eggan suggested that three and perhaps more distinct divisions might be made in the Eastern Pueblo group on the basis of social structure. This suggestion brought up three problems:

1. How many of these distinct divisions do exist in the large Eastern division, all of which align themselves on the basis of patrilineity, prominence of the ceremonial father, importance of a central figure commonly known as the cacique, two kiva system, moiety system, importance of societies, and lack of importance of clans?

2. How do these divisions, based upon social structure, correlate with the divisions based upon linguistic affiliation in the Eastern group?

3. How does the new grouping bear on Strong's reconstruction of Pueblo prehistory?

With these problems in mind, a catalogue was made of the culture traits described in the numerous, more or less detailed reports which cover all of the Eastern Pueblos with the exception of Picuris and Sandia. The headings under which the main characteristics of the social organization of each pueblo were listed are: family, kinship principles, sibs, phratries, governmental system, religious organization, and secular government. Under each of these topics were the sub-headings of organization and function.

Questions one and two answered themselves together.

THE EASTERN PUEBLOS

The Tewa pueblos⁷ are marked by a two kiva moiety system as their principal social institution. These moieties are patrilineal with a tendency toward endogamy. They function with seasonal dichotomy: the Winter people with their officers have charge of the government of the pueblo and of rites for weather control during the winter months from November to March, the Summer people taking charge during the succeeding warmer season of March to November. Two complete sets of officers, each con-

⁷ A full discussion of the Tewa social organization may be found in Parsons, *Social Organization of the Tewa of New Mexico*

sisting of the cacique and of his right and left hand men, are required. The dual feeling extends to classification of witches, food, and various objects of nature.

Tewa sibs are insignificant, frequently consisting merely of a patrilineal group name inherited with the family name but without any apparent function.

The eight Tewa societies are second in importance to the moieties. They function in curing and in conducting the village ceremonies, from secret katchina dances to war dances.

The secular government functions as the executive arm of the religious government. The officers are a governor and his two assistants, the outside chief and his five assistants, and the fiscales.

The presence of secular officers in the Pueblos is the result of a Spanish decree in 1620. The conquerors found it difficult to deal with priests and medicine men; hence at their suggestion, the King of Spain, sending a decree to the Custodian of New Mexico, ordered an annual election to be held in every Indian pueblo on the first of January. At this time "a Governor, alcaldes, fiscales, and other ministers of the Republic"⁸ were to be chosen by the Indians themselves, with no Spanish officers present. The Pueblos, accustomed to a religious government, obliged the Spaniards as best they could by retaining their own native system for actual government and by acquiring a set of secular officers to deal with outsiders and to act as an executive arm of the religious group. The method by which these secular officers gained their positions in all but two of the pueblos reflects the old system by which the religious officers were selected: appointment through the cacique, through society heads, or through an organized group. Only two Pueblos, Isleta and Laguna, ever comprehended and adopted the election system. For acculturation studies, the secular government offers excellent material.

The Tewa family is of the single, elementary type. Houses are customarily owned by the men.

Coming south down the Rio Grande one descends from the plateau into the Eastern Keresan country. Cochiti, Santo Domingo, and San Felipe, close neighbors, share one general type of social system, although outside the most important and universal traits, certain differences distinguish each of the three.⁹

⁸ White, *Pueblo of Santo Domingo*, p. 42.

⁹ Data on these three Pueblos is taken from White, *Pueblo of Santo Domingo*, Goldfrank, *The Social and Ceremonial Organization of Cochiti*; White, *Pueblo of San Felipe*; Parsons, *Notes on San Felipe and Santo Domingo*.

In contrast to the Tewa moieties, the Keresans have developed the society as the basis of their socio-ceremonial system. The religious government of the Eastern Keresans is under the control of the Flint society.

The cacique must be a Flint man; he is selected for life service by two war captains, representatives of the mythical Twin War Gods. In turn, he annually selects the two war captains.

In Santo Domingo and in San Felipe he also appoints their assistants, but in Cochiti the assistants are named by the principales.

In San Felipe, the cacique, representing the Flint society, is almost a monarch. Although assisting the cacique in selection of officers is a function of the societies, he actually makes all appointments in the secular government, which acts in dealing with outsiders. Secular officers are governor, lieutenant governor, eight captains, six fiscales, and the principales.

In Santo Domingo the secular government is under the direction of the Cikame society, which annually appoints the officers, governor, lieutenant governor, six captains, and six Bikari or fiscales.

In Cochiti the Giant and the Cikame societies cooperate in selecting secular officers. Governor and lieutenant governor are appointed by the Giant Society, the fiscale and his lieutenant are selected by the Cikame society, and the six little fiscales are chosen by the principales, who consist of all ex-officers plus the officers presiding at that time.

In all three Pueblos the functions of the societies are to control the old social structure and to oppose innovations, to cure the sick, to control the weather, and to select or to aid in selecting some of the officers. In all three the principal curing societies are the Flint, the Giant, and the Cikame. In Cochiti two minor societies, Snake and Fire, are connected to the Flint society, and each of the three main societies has a so called "managing society" tied to it; the Koshairi being tied to Flint, the Kurina to Cikame, and the Curdzi to Giant. In Santo Domingo the Boyakya society forms a fourth and less important major curing society, there are four minor curing societies and a Hunters' and a Warriors' society. In San Felipe there is only one society outside the three principal curing societies, and that is the Koshairi, tied, as at Cochiti, to the Flint society.

Moieties are not lacking among the Eastern Keresans. As in the Tewa country, the moieties are patrilineal with a tendency toward endogamy. The two kivas used by the moieties are known by the terms Squash and Turquoise. At San Felipe the Yacstca is a small third group which meets in a house, as it has no kiva of its own. This group may be the result of a fairly recent split in one of the two kiva groups. Each kiva is directed by a kiva head whose function is chiefly in connection with the actual perfor-

mance of ceremonies. The kiva groups put on the masked katchina dances, make and keep costumes, masks, and katchina dolls.

As among the Tewa, clans exist, but they lack importance. In these clans we find our first hint of matrilineal inheritance. A child takes its mother's clan affiliation. The chief function of the clan is regulation of marriage, as the clans are nominally exogamous, but endogamous marriages are frequent. The ceremonial father of a child is chosen from his clan; the duty of this sponsor is in the ritual of society initiations. A person chooses which society or societies he shall join. In both these functions Western influence is reflected.

The Eastern Keresan family is elementary. Houses are usually owned by the men.

Reports on the two more isolated Keresan villages, Zia and Santa Ana,¹⁰ on the Jemez River, are brief and miss many points, but the outline remains generally similar to that of their relatives. In Zia, as at San Felipe, the cacique is the theocratic head of the pueblo and annually appoints the two war captains and the secular officers. These officers are the governor, the lieutenant governor, and the war chief and his assistant. The societies, as reported by Mrs Stevenson, fall under three headings. Six curing societies, two societies for rain and for fertility, and a warriors' society are listed. There is a two kiva system. Clans, which were formerly exogamous, exist, but they have little importance. Decrease in village population necessitated the breakdown of exogamy. The study of Zia Pueblo now being made should do much to answer some of the questions concerning this friendly village.

The cacique of Santa Ana is chosen for life by all the medicine men, although he need not be a medicine man himself. He may be a member of any clan. He formerly had an assistant. The societies function in curing and in weather control: they are listed as the Flint, Fire, Eagle, and Shiwanna, with a hint that the Cikame is a now extinct group.

The cacique selects all civil officers. These are the governor, his lieutenant, two pishales or fiscales, two ditch officers, six captains, and two leaders.

Fourteen clans are reported, of which eight are in one moiety and five in the other. One clan is grouped with either. These moieties function only in the pattern of alternating dance groups.

For the Tiwa we are forced to concentrate on a study of Isleta and Taos,¹¹ as reports on Sandia and Picuris are lacking. It is apparent that

¹⁰ Material on these two towns is drawn from Stevenson, *The Sia*; Parsons, *Notes on Isleta, Santa Ana, and Acoma*, and from my own field notes on Zia Pueblo.

¹¹ See reports by Parsons, *Taos Pueblo; Isleta, Notes on Isleta, Santa Ana, and Acoma*

both have social systems which differ considerably from those of the Tewa and of the Keresans, although they show elements of each. The moiety is the primary organization in both Taos and Isleta, but in Taos each moiety is made up of three kiva groups and their associated "branches," ceremonial divisions within the kiva organization. No reason was gathered from informants why a certain kiva or branch was chosen by a boy or by the parents for the boy, and no indication of inheritance of membership appears. Membership comes through dedication in infancy. In Isleta if both parents belong to one moiety the children are dedicated to that moiety. If the parents belong to different moieties the children are dedicated to the one and to the other, alternately. By proper ceremonial procedure, a child may be dedicated to the moiety of a friend. The Isleta moiety has its own kiva and its own ceremonial room. The moieties here function as the chief units of ceremonial organization and provide the cacique. The Taos caciqueship is hereditary and is considered to be a Spanish office. The cacique functions only in making nominations and as the sun watcher.

The leadership of Taos is in the hands of two men, respectively heads of the most important kivas of the north and south side divisions. The leader or house chief of the north side group nominates two men for governor and one for war captain. The house chief of the south side and the cacique nominate two others. The council, composed of all the kiva chiefs, the governor, the ex-governors, the head war captain, the head fiscal, and the sacristan, chose the governor-elect from among the nominees. The runner-up and one other are voted on for lieutenant governor. The defeated man becomes the war captain. Staff officers are appointed by the governor and the war captain.

The cacique of Isleta is the leader of the pueblo. Theoretically he is taken from each of the six corn groups in succession. These groups are entirely ceremonial in function. Children are usually dedicated to the mother's group. Neither medicine societies nor women's societies are known for Taos, but in Isleta there are two medicine societies which function in the solstice ceremonies and which claim powers of magic and of curing. Members join by resolution or by a vow during illness.

Isleta is one of the two Eastern Pueblos in which we find elections of secular officers actually being held according to the old Spanish decree. The men of the pueblo elect a governor, a lieutenant governor or right hand man, a second lieutenant who is left hand man, six war captains (three from each moiety), three Laguna war captains who appoint the Laguna dancers and take charge of the Laguna colonists, two sheriffs alternating each week, two majordomos or ditch bosses, a town crier, and a council of twelve men who serve for several years. Ceremonial officers such

as the War chief, Katsina chief, Laguna father, Hunt chief, and various assistants to each also serve for life.

Clans do not exist in Picuris or in Taos: the term "clan" is used for the ceremonial group. In Isleta, likewise, there are no clans, and Parsons suggests that the matrilineal system of the corn group has been taken over from the Western Pueblos. The Isleta corn groups and the Taos "branches" appear to be generally comparable. Emphasis in membership in Tiwa organizations is not on descent but on dedication. Parsons characterizes the Tiwa as of bilateral descent but favoring the patrilineal.

This concludes a summary of present knowledge of the social organization of the Eastern villages. The most obvious general characteristic of the Eastern group, as a whole, is the patrilineal tendency. At first glance one might suspect this to be the effect of Spanish and of American acculturation, but when these Pueblos are seen to be patrilineal not only in a custom so subject to change under modern influence as house ownership but also in those organizations fundamental to their social existence, such as moieties and societies, the institution of patrilineity is seen to be too deeply ingrained to be the result of contact with European groups. Membership regulations in these basic organizations would not appear to be liable to change, either from within or by acculturation. Sibs are usually patrilineal and of little importance and few functions. It is apparent that whether the lack of importance of sibs is due to the institution having spread in a more or less attenuated form from the western clan centers to the eastern peripheries or whether it is due merely to Spanish-Mexican, and American influence, they are at present losing even what little importance they may have once held. The young people frequently do not know their own sib, and even the older people rarely have any idea of the sib affiliation of anyone outside the immediate family. It might be pointed out, however, that whatever the origin of the Eastern sibs, their importance could scarcely have been equal to that of societies, moieties, or corn groups, since those organizations have continued to function forcefully in spite of the long foreign contact. Where clans and phratries are important for the Western Pueblos, the Eastern group substitutes a dual division, important ceremonially and in government.

It is the difference in concentration of importance in one organization or in another which distinguishes the three subgroups of the Eastern division; the Tewa social organization being based upon the dichotomous moiety system, the Eastern Keresan on the two kiva-society system, and the Tiwa on the ceremonial group-moiety complex. All three are using the moiety principle in one form or another, with varying functions, but this

shift of function and the consequent rise in importance of Keresan societies and of Tiwa corn groups indicates a fundamentally different conception of the balance of power in these three linguistic groups.

THE WESTERN PUEBLOS

As one leaves the Rio Grande and moves westward, the pueblos of Jemez, Laguna, Acoma, Zuñi, and finally Hopi with its Tewa neighbor of Hano are found to make up a second large division, that of the Western Pueblos. Of these six, Laguna and Acoma represent linguistic extensions of the Keresan pueblos of the East, and Hano an extension of the Eastern Tewa. In these Western Pueblos lie two problems, that of their culture as it stands and functions today, and that of the amount and kind of acculturation which has been received from their Indian neighbors and incorporated into their own structure.

In Jemez,¹² on the border line between the Eastern and the Western Pueblos, clans are recognized as a more important element of social structure than they are farther east. Jemez clans are not only matrilineal and exogamous, but marriage into the father's clan is also disapproved. Clans likewise carry a religious function in owning the corn ear fetishes, which are considered the most sacred possession of the pueblo. They function politically, in that the cacique must be selected from a certain clan. Clan importance in Jemez is apparently an item of acculturation from the West, where clans among the Hopi figure as the most important division of the pueblo structure.

The proximity of Jemez to the Eastern villages might lead one to expect a functioning moiety system, but here the only approximation is the presence of two clown societies, each serving ceremonially one half year, and a patrilineal two kiva system reflecting a feeling for endogamy, in that a woman joins her husband's kiva at marriage. Unfortunately reports are vague on these matters and details lacking.

Government is by a religious hierarchy consisting of the cacique, two assistants chosen from the Coyote clan, and the Fathers, a group composed of the chiefs of all the ceremonial groups and the ex-governors. The presence of the cacique is an Eastern trait, but his council of priests is Western. The secular government, which is directed by the hierarchy, is composed of a governor and lieutenant governor, two war captains, their six assistants, fiscales, and a sacristan, chosen annually by the hierarchy. This reflects the Rio Grande system.

¹² For the only report on Jemez see Parsons, *Pueblo of Jemez*.

Jemez is a borderline pueblo with about an equal amount of influence from the East and from the West.

In Laguna¹³ the matrilineal clan is the outstanding feature of social life. Its functions are economic, judicial, and ceremonial. The clan head holds a position similar to that in Hopi, he has charge of the fetish figures, the katchina masks, and the medicine bowls. There is no phratry or moiety system, the only hint of dual division of clans being in the two sets of dancers who perform in the War and in the Santo dances.

The native government has been that of a religious hierarchy headed by the cacique, but that office has recently lapsed. The cacique was chosen by the Cheani, members of the medicine societies. This was made up of those who had been cured by a society and apparently about equal to the clans in importance. Ten orders of Cheani or medicine societies formerly existed at Laguna, but the split which occurred when a large section of the pueblo moved to Isleta left but two curing societies in Laguna.

The secular government consists of a governor, two lieutenant governors, and three war captains. As in Isleta, these officers are actually elected annually by a council composed of all the men. Contrary to the usual custom, these officers have ritual as well as secular functions.

The Katchina cult holds the entire pueblo population as members. Formerly a clown group and a warriors' society existed also.

In Acoma,¹⁴ the other Western Keresan pueblo, clans are matrilineal and exogamous, with economic, ritual, and political functions. The cacique must be a member of the Antelope clan. There is specialization of clan duties, as in the Hopi system. The only approximation to a dual division is in their conservative and liberal political parties.

The native government is a religious theocracy. The cacique is the religious and political head. He selects the principales who function for life as his council. Other councilors for the governor are the one war chief and his two lieutenants. The war chief, the ten little chiefs, and the three ceremonial cooks are appointed by the cacique. The secular officers appointed annually by the cacique are the governor, his two lieutenants, three fiscales, and a ditch boss. The four medicine societies function in curing, in special ceremonies, and in holding the power of veto over the cacique's choice of officers.

The katchina cult, which was carried on by the six kivas (only five exist at present), functions in bringing rain and in increasing crops. Kiva membership is patrilineal.

¹³ Parsons, *Notes on Acoma and Laguna*; Dutton, *The Laguna Calendar*; Eggan, *Social Organization of the Western Pueblos*.

¹⁴ White, *Summary Report of Field Work at Acoma, The Acoma Indians*

The patrilineal kiva membership is Eastern and the selection of the cacique from a certain clan reflects the importance of clans in the West. The presence of but one war captain contrasts with the Eastern system, and there are likewise differences in the linking of societies. Acoma is definitely Keresan in cultural background, but with a strong superstructure of Western traits.

In Zuñi¹⁵ the economic structure is based on the women of the household and the ceremonial structure on the men. The clan is matrilineal and exogamous. The Zuñi system differs from the Hopi in lacking a central clan house, clan head, clan council, and clan organization. The clan function is primarily in relation to the Katsina cult, as certain clans kept certain of the most important masks. The relation of the clan to the ceremonial organization as a whole is through control of a fetish which is kept in one of the clan households.

The phratries, which are named for the directions, are primarily ceremonial. They were formerly exogamous but are not at present.

The basis of Zuñi ceremonial life and organization is the cult of the ancestors, who are more or less confused with the Katsina and with the rain makers. There are six major cults with overlapping functions and personnel: the Sun cult in charge of the Pekwin and with the whole pueblo as members; the Uwanami in charge of twelve rain priesthoods; the Katsina cult carried on by the six kivas; the Katsina Priest cult, made up of men who have impersonated the principal katsinas in their ceremonies; the War God cult which functions as the secular arm of the religious hierarchy; and the Beast God cult, in the hands of twelve curing societies.

The governing hierarchy consists of a council of priests, the Pekwin and two other Sun priests, the two Bow priests as executives, and the heads of each kiva division of the Katsina society as advisers. There is no cacique, an omission which contrasts with the Eastern Pueblo system. The functions of this group are making changes in the ceremonial calendar, deciding questions of tribal policy, supervising initiations, appointing those who shall impersonate the gods, and selecting secular officers.

The secular government consists of a governor, lieutenant governor, and eight assistants, who are appointed by the council for an indefinite period and who may be removed from office at any time. They function in settling quarrels and property damages, in organizing cooperative work, and in dealing with outsiders.

¹⁵ Among the numerous references available on Zuñi, some of the best are Stevenson, *The Religious Life of the Zuñi Child: The Zuñi Indians*, Cushing, *Zuñi Social, Mythical, and Religious Systems*; Kroeber, *Zuñi Kin and Clan*, Bunzel, *Introduction to Zuñi Ceremonialism*

It is difficult to say whether the clan or the household is the basis of Zuñi social organization, for this pueblo has a much more closely integrated social structure than the pueblos previously examined, and the balance of power is so distributed that no focus of concentration is immediately visible.

The six Hopi pueblos,¹⁶ while never united, are all of one type of social organization, which may be taken as representative for the West.

The Hopi family is compound and bilateral, matrilineal, and with the household as the important economic unit. Clans are matrilineal and composed of one or more non-selfconscious matrilineal lineages. Each clan has specialized functions in the village organization, owns land and springs, and recognizes a clan house containing ceremonial paraphernalia which pertain to the ceremonies owned by the clan. The position of clan leader is hereditary in the chief household of the clan and is handed down to brother or to sister's son.

This clan leader is likewise chief priest of a society or fraternity which carries the same name as the clan but whose membership is not identical with that of the clan. One joins through a ceremonial father or mother selected from within or from without the clan by one's parents. This fraternity puts on the ceremonies owned by the clan of which the leader is a member. The ceremonies are given in a kiva owned by the clan and usually bearing the clan name, but with a membership drawn, through ceremonial fathers, from various clans.

The clans are linked into exogamous phratries whose chief function is marriage control.

The Hopi type of government is the hierarchy, consisting of a body composed of the chief priest of every clan. As the old Spanish order did not extend westward to Hopi, there is no secular government.

The kinship system is the most important integrating factor in Hopi society. With two small exceptions, there is precise correlation between kinship terminology and behavior. Terminology is basically reciprocal except for relatives by affinity. Father's and mother's parents are not given different terms, but their ritual duties are different. Grandparent terms are extended to include all old persons. The kinship structure is based on vertical grouping of relatives in the matrilineal and patrilineal lineages.

Hano,¹⁷ the Tewa neighbor of Sichimovi, is made up of people whose

¹⁶ Modern references are Lowie, *Notes on Hopi Clans*; Eggan, *Social Organization of the Western Pueblos*.

¹⁷ Friere-Marreco, *Tewa Kinship Terms from the Pueblo of Hano, Arizona*, Parson, *Ceremonial Calendar of the Tewa of Arizona*, Eggan, *op. cit.*

ancestors moved from the Rio Grande at the time of the Pueblo rebellion more than 250 years ago. The kinship system of Hano is now the same as that of Hopi. Hano clans are similar to Hopi clans in structure and in function. They have clan houses, masks and paraphernalia, legends, and land ownership. In place of the numerous Hopi phratries of linked clans, three clans are grouped into one phratry and the other clans are single. They also use the Hopi system of clan control of ceremonies, but initiation into the fraternities is on the Eastern basis of curing or of trespass rather than through the Hopi system of a ceremonial father.

Following the Rio Grande system, Hano has two kivas, which are more important than Hopi kivas in that many of the duties of Hopi fraternities are assumed by the Hano kiva groups. These kivas are the center of all major activities. Membership is by clan affiliation.

The governing hierarchy is similar to that of the Hopi, but reflections of the Eastern system are seen. The governing body is made up of the clan heads plus the kiva chiefs. The functions of these officers are similar to those of the Hopi hierarchy.

In his paper on the *Social Structure of the Western Pueblos*, Dr Eggan has summed up the characteristics of the Western type. Their social structure is based upon their kinship system, which universally conforms to the Crow-Omaha type, and upon their ceremonial organization. The household is composed of a matrilineal lineage or of a part of a lineage. This puts a group of women into the position of central core of the household. They must look to their husbands for economic support but to their brothers, who are members of their own clan, for ritual activities.

The functioning exogamous matrilineal clan is present in all the Western Pueblos and varies little in form, but there is variation in method of organization and in its relative importance.

Hopi, and Hano to a lesser extent, have definite exogamic phratry groupings. Zuñi formerly had a phratry organization primarily functioning for ceremonial purposes. The clan is weaker than at Hopi.

Laguna has a dual division of clans for certain dances. Clans were formerly strong.

Acoma lacks all phratry or moiety division. One clan, the Antelope, is dominant.

The Katsina tribal cult is strong in all the Western pueblos.

Curing is largely an individual matter among the Hopi. Rain making is the function of the fraternities. The Zuñi have both curing and rain priesthoods. Curing is the dominant function of the Acoma and Laguna societies, but they seem to have rain making power also.

The ceremonial organization is fundamentally related to the clan system. In Hopi the clan controls the clan ceremonies and keeps the requisite paraphernalia in a clan household. In the other Western pueblos the basic fetish is kept within the clan or within a certain lineage and household of that clan. The ceremonial calendar is based on the two halves of the year set by the solstices. Initiation into ceremonial groups is based on the selection of a ceremonial father.

In each pueblo the kinship system is tied up with the clan, fraternity, and kiva groups, although the range and nature of kinship extensions varies from pueblo to pueblo. Each form of organization overlaps the others in membership and functions, and hence, with the kinship system, forms a strongly integrated social structure.

SUMMARY AND CONCLUSIONS

For a long period many of those interested primarily in other fields of Southwestern anthropology have spoken of the pueblo social system and type of government as if there were but one, and that one founded on matrilineal exogamous clans, governed by a council of clan representatives, and living in houses owned by the women of a socialistic Eden where the aged as well as widows and orphans were never in want of provision.

This, with the addition of a few traits such as the phratry system, clan ownership of ceremonies, inherited clan leadership of fraternities, and emphasis upon the ceremonial father, chosen within or without the clan, as sponsor for initiation of a child into fraternity and kiva organizations, provides a fairly accurate picture of the Western Pueblo system as represented by Hopi. It was the Hopi system which was studied by the early Southwestern anthropologists. Since the material culture of Eastern and of Western Pueblos was similar enough to fall into one classification, it was generally supposed, by inference, that the social systems of all were likewise of one type and might be characterized by descriptions of Hopi. This popular mistake and its implications become apparent when one examines in detail the sections on social organization and government found in the more recent reports on the Pueblos, both of Arizona and of New Mexico. While many of these reports leave something to be desired, one can sketch the general divisions of Pueblo groups on the basis of distinctive social organization, one of the most intimate manifestations of the culture of a group.

These divisions are: an Eastern group taking in the Rio Grande Pueblos and Santa Ana and Zia, and a Western group including Hopi, Zuñi, Acoma, Laguna, and Jemez.

The Western group is characterized by the above description. The

Eastern group contrasts with the Western in emphasis on patrilineity, in the moiety rather than the phratry system, entrance into societies through patrilineal inheritance or through trapping or curing, and the non-importance of clans, whose functions are taken over by moieties, societies, or town groups.

Within these two large groups, the Pueblos appear to divide on social organization into the same divisions as those previously set off on the basis of linguistic affiliation. The Western group, containing Hopi and Zuñi, the two Western Keresan pueblos of Acoma and Laguna, and the Towa village of Jemez, divides into four sub-types of social structure. The Eastern group of the Tewa, Eastern Keresans, and Tiwa divides into three sub-types of social structure. The Western Keresan villages and the Western Tewa village of Hano show almost as many Eastern as Western traits. It is apparent that as one moves westward from the Rio Grande, center of the Eastern type, the Pueblos, while holding to many Rio Grande characteristics, have picked up an increasing number of Western characteristics from their neighbors.

It would appear that more than one group of people with different languages and different conceptions of social organization settled in the prehistoric Southwest. The exact correlation between linguistic stock and social structure in the Eastern Pueblos, and the remnants of Eastern Keresan and Eastern Tewa social structure in the Western Keresan and the Western Tewa villages suggest that the people who settled down as Pueblo agriculturists drifted into the region from various sources with various tongues. Those of each stock retained remnants of their old social organization, modifying it to suit new needs of communal life and, over a period of centuries, changing from the parent type in proportion to the nearness or the isolation of each pueblo from the parent group and to the strength of influence exerted upon them by other neighboring groups. The linguistic groups in the east modified their old institutions and adopted new ones, but through their close inter-influence they retained a general Eastern type within their collective systems of sub-types. In the west, Zuñi and Hopi developed their sub-types and so influenced the isolated Western Keresan villages, the Towa village of Jemez, and the Tewa village of Hano that these modified their original social systems and acquired a distinct western veneer.

Excellent evidences of these cultural processes are to be seen if one looks for acculturation in studies of the Tewa who settled at Hano on the first Hopi mesa during the Spanish period,¹⁸ of the Laguna migrants who

¹⁸ See references for the section on Hano.

moved to Isleta in 1879,¹⁹ and in similar studies of the Rio Grande villages on whom a Spanish decree forced a supposedly democratic secular government, the system resulting from that one decree varying in every group according to its previous type of organization.

When and from where did these groups enter the Southwest? These are problems on which archaeology and linguistics may indirectly throw some further light, although it is doubtful that we can ever answer the questions with certainty. Strong's theory of the linkage with southern California and of the development of both Eastern and Western Pueblo groups *in situ* appears too simple an explanation to fit the data known at present. It seems more reasonable to postulate at least two different places of origin for the cultures we know as "Pueblo."^{19a}

The recent work²⁰ by Haury, Sayles, and Gladwin on the Mogollon culture of southwestern New Mexico and on surveys from Texas to the Mississippi Valley, has convinced them of the possibility and perhaps of the probability that at least some of the migrants into the Southwest before the period of Pueblo I came from the east. Unfortunately most of the eastern periphery of the Pueblo region has escaped all but cursory examination or more might be said on this subject. Kidder's latest book on Pecos suggests eastern origin of some of the Rio Grande prehistoric groups.²¹

Looking for the linguistic affiliations of our two large groups of Pueblos, we find that the Tiwa, Tewa, and Towa are branches of the Tanoan stock which has been linked with the Kiowa stock of the Plains and listed as Kiowa-Tanoan.²² The Keresan has recently been tied to the Hokan-Siouan stock, many of whose members are likewise now found on the plains. The Western Tanoan and Keresan pueblos, as has been already described, show remnants of the Eastern system with superimposed Western traits, presumably borrowed from their Western neighbors.

The Shoshoneans, represented among the Pueblos by the Hopi, are of the large Uto-Aztecan linguistic stock, among the southern groups of which one finds the patrilineal system generally dominant. In contrast, the matrilineal family, clan, and exogamous phratry are the high points of the Hopi social system and appear to have spread from them to their neighbors.

¹⁹ See references for Laguna and for Isleta.

^{19a} Since the completion of this paper, Miss Nan Glenn has prepared a thesis for the University of New Mexico giving detailed comparisons of the social organization of the Pueblos and some of the Plains groups, with provocative results.

²⁰ Haury, *The Mogollon Culture of Southwestern New Mexico*.

²¹ Kidder and Shepherd, *The Pottery of Pecos*, Vol. 2, p. 396.

²² Sapir, *Central and North American Languages*.

The data at hand would appear to indicate that in prehistoric times, possibly even as early as Pueblo I or before, people drifted into New Mexico from the east and into Arizona from the north or south. They brought with them linguistic and cultural heritages, both of which were largely modified by their subsequent isolation from the parent groups and by the influence of their neighbors. The Basket Makers encountered by those who were to become the carriers of Pueblo culture may themselves have been of more than one stock and from different regions, and handing down different customs. At any rate, at least two large groups, one with a number of divisions, now stand as the remnants of the prehistoric people whose material culture elements can be traced step by step up to the modern period. It seems probable that, in general, the linguistic stocks as well as the main items of the present social systems were likewise characteristic of the ancestors of these people.

The ancestors of the Eastern Pueblos may have been Plains groups who came into the Rio Grande and adjoining districts in several migrations and settled to a sedentary agricultural mode of existence with the attendant development of arts, crafts, and ceremonialism. Into what is now Arizona, probably came immigrants of Uto-Aztecan stock, people who found satisfaction in the relative peace and plenty of farm life. People of other stocks or of other divisions of these stocks likewise may have comprised a part of the prehistoric Southwestern population but eventually died out altogether or were absorbed and lost their identity in the remaining stronger groups.

Although the present Pueblo culture pattern is predominantly Apollonian²³ in contrast to the Dionysian pattern of the Plains, such a change might be expected of a people who chose a communal agricultural life, where the welfare of the group rather than the prowess of the individual must be stressed for the internal social unity necessary for that type of existence.

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- MAAA Memoirs, American Anthropological Association
- RBAE Annual Report, Bureau of American Ethnology

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THE SCIENTIFIC INFLUENCE OF
SIR GRAFTON ELLIOT SMITH

By T. WINGATE TODD

THE contributions to science made by Grafton Elliot Smith were enhanced by four attributes: a vivid personality which infused enthusiasm and commanded respect, a clear, forceful and intriguing literary style, a gift of black and white draughtsmanship which was used to simplify his observations and interpretations, and a Bentham-like insistence on the practical issues in every study.

In the long series of publications standing to his name there is a very definite continuity linking in easy and obviously natural sequence successive articles on mammalian ecology, neurology, physical anthropology, and cultural anthropology. In this recital of research achievement reference is made merely to those writings which most clearly summarize Elliot Smith's enrichment of our scientific knowledge and mark important phases of his own progress. The essential feature in the long train of evidence secured and presented by this acute thinker is its pioneer character. Masses of conflicting misinformation clog the progress of any explorer in uncharted areas. Those who were nearest to Elliot Smith best realize his uncanny instinct for pursuing the course which leads to complete and satisfying solution of the problems he undertook. And as his experience grew he unhesitatingly plunged into ever more involved confusions, clarifying where clarification is possible and formulating the course which further studies must take where the limits of available knowledge still render clarification unattainable.

His first published essay dealt with the sympathetic nervous system, that adjuster of personality and social integration, a theme which, throughout his brilliant career, he undeviatingly pursued even to its ramifications in the development of human society and culture.

In 1894 Elliot Smith began to publish his observations on the brains of the very primitive pouched and egg-laying marsupials of Australia,¹ pilorying as he went the nonsense that had so far passed muster for science, and relating the structure of their brain to the conditions of their environment. He paid special attention therefore to the smell-brain by which these animals make their adjustment to the conditions of life. There could indeed be no field of study more favorable to the investigation of this problem, for the oral sense of the Platypus is greatly developed whereas

¹ *A Preliminary Communication Upon the Cerebral Commissures of the Mammalia, with Special Reference to the Monotremata and Marsupialia* (Proceedings, Linnean Society of New South Wales, Vol. 9, pp. 635-57, 1894).

that of the Spiny Anteater is markedly atrophied. The fortunate accident of leisure, arising from youthfulness in the general practice of medicine, which directed Elliot Smith's attention to these lowly mammals thus came to be of profound significance in determining the course of his further studies.

The opportunity of coming to London as a young man was followed by the opportunity to set in order the great collection of mammalian brains in the Museum of the Royal College of Surgeons. It is in the catalogue which he made for this collection² that one can find the germ of every interpretation which Elliot Smith developed in a host of special studies published in English and German journals.

Fresh from this work Elliot Smith went to Cairo as Professor of Anatomy in the Government Medical School newly established by Lord Cromer and in the autopsy room of the hospital there he found a wealth of opportunity to study the human brain. Whereas Brodmann and Campbell, from paucity of material, had to devote their attention to the slow procedures of histology, Elliot Smith, with hundreds of brains at his disposal every year, was able to cover a vastly greater material by simplified methods of gross dissection, and thus produced the convincing evidence till then lacking but absolutely essential to the elucidation of human brain structure as a basis for the study of function. It was his classic paper on the occipital region of the human brain³ which won him the Chair of Anatomy at Manchester University. Shortly after returning to England Elliot Smith summarized the results of his years of study in Cairo in a very significant series of Arris and Gale Lectures.⁴ His presidential address to Section H of the British Association in 1912 laid the foundation not only for a heightened activity into the story of the evolution of form and function but also for the endocranial cast as a reliable record of brain configuration.⁵

During his sojourn in Cairo the British Government resolved on the construction of the Assuan Dam. This necessitated the scientific examination of the hordes of ancient Egyptians buried in the area to be submerged.

² *Descriptive and Illustrated Catalogue of the Physiological Series of Comparative Anatomy Contained in the Museum of the Royal College of Surgeons of England* (London, Vol. 2, pp. 140-25, 138-481, 1902)

³ *Studies in the Morphology of the Human Brain, with Special Reference to that of the Egyptians. No. 1: The Occipital Region* (Records, Egyptian Government School of Medicine Vol. 2, pp. 125-73, 1904)

⁴ *The Arris and Gale Lectures on Some Problems Relating to the Evolution of the Brain* (Lancet, Vol. 1, pp. 146-53, 221-27, 1910).

⁵ *Address to the Anthropological Section* (Reports, British Association for the Advancement of Science, Sect. H, Dundee, pp. 575-98, 1912).

the main this task fell to Elliot Smith who therefore learned at high cost the physical characters of successive generations of these people dwelling over four millenia, and the results of this study were compiled in the Nubian Survey.⁶ At the same time Elliot Smith obtained the privilege of examining the Royal Mummies⁷ and to him is due the credit for first using X-rays in the elucidation of mummies of which sentiment forbade the wrapping. These experiences resulted in the Dublin address of 1908 at the British Association, an address which contains the germ of so many of the ideas which later played an important role in his interpretations on culture.⁸

Messrs Harper then asked Elliot Smith to formulate his findings on the relation of the physical to the cultural characteristics of the ancient Egyptians and this was done in an unpretentious little volume of stimulating thought and inquiry.⁹

It was at this time that Elliot Smith's imaginative faculty portrayed Egypt as a depository of the evidence of human culture. At that time the history of Sumer and more ancient cultures was not known. Elliot Smith recognized in the trade routes of Egypt and her military dominance at certain periods of history a potent influence in the development of human society and a stimulus for the diffusion of culture. He demanded that this stimulus be reviewed dispassionately and the whole course of cultural development be reinvestigated from the practical standpoint. That opinion is divided upon some of his claims is no disqualification of the central contention that this subject will bear much further examination in the light of our rapidly increasing understanding. The first essay in this direction was made at a meeting of the Manchester Literary and Philosophical Society in 1915 and published in its proceedings.¹⁰ The most mature and philosophic exposition is to be found in his volume on *Human History* published in 1929.¹¹

⁶ With F. Wood Jones, *Report on the Human Remains* (in *Archæological Survey of Nubia, Report for 1907-1908*, Cairo, Vol. 2, 1910).

⁷ *The Royal Mummies* (Catalogue général des antiquités Égyptiennes du Musée du Caire, Cairo, 1912).

⁸ *The History of Mummification in Egypt* (Reports, British Association for the Advancement of Science, Dublin, p. 847, 1908).

⁹ *The Ancient Egyptians and their Influence Upon the Civilization of Europe* (London, 1911).

¹⁰ *The Migrations of Early Culture, a Study of the Significance of the Geographical Distribution of the Practice of Mummification as Evidence of the Migrations of Peoples and the Spread of Certain Customs and Belief* (Memoirs and Proceedings, Manchester Literary and Philosophical Society, Vol. 59, Pt. 2, 1915).

¹¹ *Human History* (New York, 1929).

It was a matter of unconcern to Elliot Smith how far his contemporaries might agree or differ provided only that they kept undimmed a willingness to examine new data without prejudice and a flexibility of mind which makes for progress in human understanding, necessary qualifications for scientific endeavor with which, by his attitude to his own work and by his friendly discussion of their problems, he profoundly impressed all his pupils.

WESTERN RESERVE UNIVERSITY
CLEVELAND, OHIO

WITH the death of Thomas Talbot Waterman in Honolulu on January 6, 1936, there passed one of the vivid figures of American anthropology and one of its great teachers.

Waterman was born April 23, 1885, at Hamilton, Missouri, as the youngest of ten children of John Hayes Waterman, an Episcopalian clergyman of New England ancestry, and Catherine Shields Church of Mississippi. Most of his youth was spent in California, especially at Fresno. Destined by family tradition for a clergyman's career—his older brothers having successively passed the obligation on—he graduated from the University of California in 1907 with Hebrew as his major subject. His philological studies led him into a course in experimental phonetics offered by P. E. Goddard, whom he then accompanied on a field trip as assistant in recording California Athabascan dialects. This experience was decisive in diverting him from divinity to anthropology. He spent 1909–10 at Columbia University and received its Ph.D. under Boas in 1913.

At the University of California he was Museum Assistant 1907–09, Instructor and Assistant Curator 1910–14, Assistant Professor 1914–18, Associate Professor 1920–21. From 1918 to 1920 he served as Associate Professor at the University of Washington. A period of restlessness had by now set in. He left his professorship at Berkeley for a position at the Heye Museum, this for a connection with the Bureau of Ethnology, then for the technical directorship of the National Museum of Guatemala, coming to temporary rest again at Fresno State College, where he remained until 1927 teaching geology, geography, and anthropology. After a year at the University of Arizona, he went to Honolulu where he taught in the Territorial Normal College and University of Hawaii, engaged in newspaper and public relations work, and was appointed Territorial Archivist a few months before his death in 1936.

Waterman was twice married: in 1910 to Grace Godwin, to whom were born Helen Maria in 1913 and Thomas T., Jr., in 1916; and to Ruth Dulaney in 1927; all of whom survive him.

Waterman was first of all a brilliant, incisive, colorful teacher, rarely systematic and sometimes erratic, but extraordinarily stimulating. To literally thousands of students he remains an unforgettable memory and inspiration, who made anthropology mean something real to them to this day. The foundational course at California is his creation, he supplied its imagination, drive, and appeal, the present writer the framework and bal-
last. The *Source Book in Anthropology*, issued first by the University and

then in thoroughly revised form by a publisher, is the product of this cooperation, but it was Waterman's brain child.

In his professional work, he loved concrete facts and sharply defined findings, both presented with the same clean-cut picturesqueness which characterized him on the lecture platform and in intimate conversation. His range of interest was wide: folk-lore, Aztec antiquities, Diegueño and Duwamish ritual, Yana history, Yurok geography and institutions, Makah whaling, Paiute phonetics and Yurok affixes, native American house types and poetry, Shakerism, Pueblo pottery ornament, race classification and the negro. At times his passion for flaming clarity led him into over-simplification of situations; but with the compensation of lifting essentials into sharp relief.

Waterman was simple, sincere, direct, always vigorous, often drastic. Nuances were not for him, and he could travel only the straightest paths. Loyalty was spontaneous in his nature, as was courage. Some were offended by his brusqueness, administrators were often puzzled by his apparent reversals, but most men and women felt for him a powerful and unwavering affection.

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UNIVERSITY OF CALIFORNIA

BERKELEY, CALIFORNIA

REPORTS

ANTHROPOLOGICAL SOCIETY OF WASHINGTON

The Anthropological Society of Washington at its annual meeting held on January 19, 1936, elected the following officers for the ensuing year: *President*, Frank H. H. Roberts, Jr., *Vice-President*, Henry B. Collins, Jr., *Secretary*, Regina Flannery; *Treasurer*, T. Dale Stewart; *Vice-President of the Washington Academy of Sciences*, Frank H. H. Roberts, Jr., *Members of the Board of Managers*, George S. Duncan, Frank M. Setzler, Herbert W. Krieger, Julian H. Steward, and W. D. Strong.

A report of the membership and activities of the Society since the annual meeting held on January 21, 1936, follows:

Membership.	
Life members	3
Active members	39
Associate members	14
Honorary members	18
Corresponding members	18
Total	92
New Members	
Active members	2
Associate members	2
Total	4

So far as the Secretary is aware, our Society was fortunate in not losing a single member either through death, transfer, or resignation.

Members elected during the year were: Dr. Julian H. Steward, Mr. C. Martin Wilbur, Mr. H. Summerfield Day, and Mr. F. E. Newcomb.

The financial statement (Treasurer's report) is as follows.

Funds invested in Perpetual Building Association	\$1,289.17
21 shares Washington Sanitary Improvement Co., par value \$10 per share	210.00
2 shares Washington Sanitary Housing Co., par value \$100 per share	200.00
Cash in bank	305.16
Total	\$2,004.33
Bills outstanding	
To American Anthropological Association	\$65.00
To printer	4.00
To Secretary	.86
To Treasurer	1.02
Total	\$70.88
Net balance	\$1,933.45

Papers presented before the regular meetings of the Society were as follows:

January 21, 1936, 663rd regular meeting, *Archaeological Horizons in Florida*, by M. W. Surling, Chief, Bureau of American Ethnology.

February 18, 1936, 664th regular meeting, *Peabody Museum's Jeddito Valley Expedition 1935*, by J. O. Brew, Peabody Museum, Cambridge, Mass. During the same meeting T. Michelson, *Ethnologist, Bureau of American Ethnology*, gave a summary of the results of his 1935 studies among the tribes of the Hudson Bay region.

March 17, 1936, 665th regular meeting, Primitive Land Tenure, by John M. Cooper, Middle University, Washington, D. C.

April 21, 1936, 666th regular meeting, Recent Discoveries Under Ground and Under Water in Jamestown and Yorktown, Va., by B. Lloyd Flickinger, Superintendent, Colonial National Monument. Verne L. Chatelain, National Park Service spoke on the various parks being developed by the National Park Service.

October 20, 1936, 667th regular meeting, The 1936 Season's Field Work in Alaska, by Aleš Hrdlička, Curator of Physical Anthropology, U. S. National Museum. This was a joint Smithsonian-Anthropological Society of Washington lecture.

November 17, 1936, 668th regular meeting, Archeological Investigations in Northern Honduras, by W. D. Strong, Anthropologist, Bureau of American Ethnology.

December 15, 1936, 669th regular meeting, Medieval and Modern Witchcraft, by George S. Duncan, Professor of Egyptology and Assyriology, Graduate School of American University, Washington, D. C.

All regular meetings of the Society were held in Room 43 of the U. S. National Museum. The Smithsonian-Anthropological Society of Washington lecture by Dr. Hrdlička was delivered in the Auditorium.

F. M. SFTZLER, *Secretary*

FIFTEENTH ANNUAL MEETING OF THE CENTRAL STATES BRANCH, AMERICAN ANTHROPOLOGICAL ASSOCIATION

The 1936 annual meeting of the Central States Branch was held on Friday and Saturday, April 10-11th, at Northwestern University, Evanston, Illinois.

PROGRAM

FRIDAY, APRIL 10TH, 10:00 A.M.

President Robert Redfield in the chair. There were appointed Nominating Committee McKern (chairman), Herskovits, Guthe, Resolutions Committee Martin (chairman), Eggan, Gower, Auditing Committee Linton (chairman), Greenman, Webb. The following papers were read:

ANNE H. FULLER, Recent Progress in Near East Prehistory
ROBERT MCCORMICK ADAMS, Recent Contributions to Archaeology in Yugoslavia
MARGARIT KNEBBERG, Racial Hair Classification
AMOS BUTLER, Some Indian Trade Objects
FRED LOGAN, Historic Changes in the Choctaw Kinship System
A. R. RADCLIFFE-BROWN, The Present State of Anthropological Studies in China
HARRY HOJER, The Hokan Affiliations of Tonkawa

2.00 P.M.

DAVID B. SPOLY, Affiliations of Certain Types of Wisconsin Copper Implements
CARL LLOYD, Masonry Techniques at Lowry Ruin
HOWARD MINER, The Kincaid Site
J. BREWSTER BERRY, Excavation of a Burial Mound in Central Missouri
CHARLES R. KLAES, A Woodland Village Site
O. W. JUNK, Isolated Communities in Labrador
H. C. SHULTZ, The Folsom Phenomena as Seen from Ohio

6.30 P.M.

A banquet was tendered to out-of-town guests in the Georgian Hotel by Northwestern University. The address of the evening followed. RALPH LINTON, Culture and How to Study It.

SATURDAY, APRIL 11TH, 9:30 A.M.

DONALD COLLIER, The Ghost Dance Cults among the Kiowa

RAUF LINTON, Notes on the Family

M. J. HERSKOWITZ, A Note on 'Woman Marriage' in Dahomey

WILLARD PARK, Some Phases of Religion in North America and Historical Reconstruction

W. LEONARD WATNESS, The Social Configuration of Magic in North Australia

11:30 A.M.

At the annual Business Meeting, the reading of minutes was dispensed with. The Secretary's report showed 2 Honorary, 79 Active, and 22 Associate Members, a total of 103, a gain of 18 over 1935. The report was accepted.

Chairman Martin of the Resolutions Committee reported the following resolutions moved for adoption. Motion prevailed.

Resolved That the Central States Branch of the American Anthropological Association expresses its appreciation of the hospitality accorded it by the Department of Anthropology of Northwestern University and is particularly grateful to Dr Melville Herskovits and his colleagues for their efforts in arranging a very successful meeting.

Resolved That the Central States Branch of the American Anthropological Association regrets the loss it has sustained through the death of its esteemed member, Dr Otto L. Schreiner.

Resolved That the Central States Branch of the American Anthropological Association expresses its deep sense of bereavement at the death of one of its most distinguished members, Dr James Henry Breasted.

The report of the Treasurer showed:

Receipts

Cash in bank as of last report (May 1, 1935)	\$393.76	
Dues collected and deposited, to April 1, 1936	297.00	
Total,		\$690.76

Disbursements

Dowagiac Commercial Press, programs	\$ 20.20	
Geo. R. Fox, expenses, 1934 and 1935	45.25	
Dues, American Ethnological Society	2.00	
Dues, American Anthropological Association	144.00	
Total		211.45
Balance in Bank as of April 1, 1936		\$479.31

The Auditing Committee, by Chairman Linton, reported that it had examined the books of the Treasurer and found them correct. It moved that the report be accepted. Motion carried.

As the Central States Branch had received invitations from two cities, Milwaukee and Iowa City, it was moved and voted that the selection be left in the hands of the Executive Committee.

Chairman McKern of the Nominating Committee presented its report. *President*, Wm. S. Webb, *Vice-President*, W. M. Krogman, *Vice President*, Leslie A. White, *Secretary-Treasurer*, Geo. R. Fox, *Executive Committee*, Earl H. Bell, Emerson Greenman, Paul Martin, Charlotte Cowser, Horne Duval. The report was adopted and the nominees declared the officers elect for 1936-1937. There being no further business, the meeting adjourned. Total attendance, 384.

Geo. R. Fox, *Secretary*

BOOK REVIEWS

NORTH AND SOUTH AMERICA

- Lower Eskimo Settlements on Frederik VI's Coast.* THERKEL MATHIASSEN. (Meddelelser om Grønland, Vol. 109, No. 2. 58 pp., 28 figs. Kr. 2 50. Copenhagen. C. A. Reitzels Forlag, 1936.)
- Eskimo Archaeology of Julianhaab District, with a Brief Summary of the Prehistory of the Greenlanders.* THERKEL MATHIASSEN AND ERIK HOLTVED. (Same series, Vol. 118, No. 1, 1936. 141 pp., 4 pls., 60 figs., 4 maps. Kr. 7.)

These two articles, the first on the archaeology of the southern coast of Greenland just east of Cape Farewell, the second on that of the coast just west of the cape, conclude Mathiasen's archaeological surveys of Greenland, which were begun in 1929, when I enjoyed the privilege of serving as his assistant. They exhibit the painstaking scholarship and clear presentation characteristic of his previous works. The first article is short, only because the finds from the southern East Coast were scanty; sites on the southern West Coast were fortunately more prolific.

Neither report can be read intelligently without reference to the author's earlier publications on Eskimo archaeology, and Helge Larsen's excellent archaeological monograph on Northeast Greenland (Meddelelser om Grønland, 1934). This is because the bulk of the material from the South Coast is composed of types already illustrated and described, so that these types are only listed here. These two reports deal largely with settlements and house types; only distinctive artifacts are described in detail.

The general reader will welcome the summary of Greenland prehistory which concludes the second article, since this reviews all the previous work in that area. As Mathiasen sums up:

We see the Eskimos come from arctic Canada somewhere about a thousand years ago, they arrive with their whale-bone house and Thule Culture, and settle down in Cape York District. Soon they move southwards, over Melville Bay, to northern West Greenland, where they live for several hundred years, and where they fall in with Norsemen travelling northwards on their hunting expeditions; the Thule Culture by this time is changed to the Inugsuk Culture. In the 14th century the population has grown so big that it seeks new hunting fields. South Greenland is populated in the course of this century, the Norsemen are overrun, and a group of Eskimos wanders around Cape Farewell to the East Coast and far to the north along that coast. In the 17th century a group of Polar Eskimos migrates north about Greenland and mixed with Northeast Greenland's early population; and shortly afterwards more West-Greenlanders come round to Angmagssalik; at Kangedlugsuaq there is still a small remnant of the first immigration to the East Coast, and before long they die out. The same is the case with the inhabitants in Northeast Greenland. In West Greenland, however, the population grows rapidly, by and by helped by the Danish colonization, which also saves the population of Angmagssalik from the fate of the other people on the East Coast. At Cape York there is still a remainder of the old Thule Eskimos, added to by a new immigration from Baffin Land.

From a culture-historical point of view Greenland is a cul-de-sac, culture streams from the

outside deposit themselves but never go out again; culture developments in Greenland are never of any significance to the developments that are proceeding on the American continent . . . But to study the manner in which the culture develops within so limited a region within a limited period is surely also of importance to cultural science in general (Vol. 118, p. 125f.)

In conclusion Mathiasen states again his position on the origin of Eskimo culture, but there is little here that he has not already argued in the *AMERICAN ANTHROPOLOGIST* (1930). He still feels that the original home of the Eskimo is to be sought in Alaska or Siberia, and that the first Eskimo in Canada and Greenland were the Thule people. He admits, however, that Jenness' Cape Dorset culture of eastern Canada is something other than a local development from the Thule, and suggests that it may not be Eskimo at all, but Indian. The only problem of Greenland archaeology which he has not considered is whether the Cape Dorset types found in Northwest and West Greenland were brought there by the Dorset-ized Thule immigrants or whether they were introduced by a separate migration or diffusion. But he may, perhaps, be waiting for the results of Erik Holtved's recent excavations in Cape York District before considering this important question.

FREDERICA DE LAGUNA

UNIVERSITY OF PENNSYLVANIA MUSEUM

Myths of the Owens Valley Paiute. JULIAN H. STEWARD (University of California Publications in American Archaeology and Ethnology, Volume 34, No. 5, pp. 355-440, Berkeley, 1936.)

Nez Percé Texts. ARCHIE PHINNEY (Columbia University Contributions to Anthropology, Volume 25, xii, 497 pp. \$5.00. New York: Columbia University Press, 1934.)

These two volumes form a welcome addition to the published material from a region whose mythology is still scantily known. In Dr Steward's collection, thirty-eight Owens Valley myths are given, and a few myths from Mono Lake and from the Shoshone of Lone Pine. Coyote is one of the principal characters, and Wolf figures largely. Typical myths of the region are included, such as the Death Controversy with Coyote suffering the first bereavement, the Theft of Fire, the cooling of Sun by Cottontail. There are many and detailed similarities with Lowie's *Shoshonean Tales*. The strange tale of Coyote's fatal obsession with Trap would delight the student of psychology.

A study of the literary aspects of the myths opens the volume. It is apparent in this that Dr Steward has a rare appreciation of the myths which he has collected, few of us can speak of the "splendid" humor of a Paiute tale, or of its "intense" climax. Otherwise, I feel that the study does not contribute to an understanding of the myths. To dismiss the subtly complex characters of the tales as "animals" is to oversimplify. It is confusing to be told in one passage that the myths are "properly speaking, folktales," and farther on, that they are undoubtedly accepted as "Gospel truth." According to the collector, there is an "unusual freedom in story construction," since the narrators calmly accept the presence of varying

sions. But—and this is the test of freedom in construction—did the narrator feel free to alter his own version according to whim? Could he do so and still accept it as "Gospel truth?"

Other statements of Dr Steward's are directly contradicted by the myths. The myth of Coyote, Magpie, and Dove, dedicated as it is to a portrayal of conjugal tenderness and devotion, puts to shame the assertion that "themes of love are conspicuously absent." And, though we are led to expect in Coyote a simple trouble-maker, we find him responsible for such boons as fire and the spaciousness of the earth.

But we soon come to the myths and are grateful for the store of mythical material.

Mr Phinney prefaces his collection with a short introduction. Very illuminating is his discussion of the Indian's conception of the characters of myths. The Indian does not visualize these, he tells us, a clear picture would be ruinous to the overtones of fantasy and charm found in the myths: the character may not even be identified. This brings to mind how frequently, in the field, the informant avoids naming the myth or its characters, unless expressly requested to do so.

To my mind, the collection of these tales forms a landmark in American Indian mythology. The narrator is an artist. Unity is deliberately achieved through carrying unimportant details over from core to core. The myths excel in characterization, done in a few suggestive strokes, such as the picture of the two Coyotes who forget the ruined transformation and their original quarrel in the sheer excitement of chasing each other. In Skunk and White Eagle, the portrayal of Skunk, pitiful and ludicrous, in the grip of a compulsion to name the thing he fears, is masterly. But not alone the richness and variety of the material or the excellence of the narrator make this collection unique. Material is usually there for him who looks for it, and good informants are rarely lacking. Most unusual is the language of the translator. Our myths, so far, have appeared in the poor and limited English vocabulary of a native informant or interpreter, or in the rigidly dignified translation of the linguist, to whom the language of the myths is a thing at best mentally comprehended, and who renders it with a welcome but cold un-English flavor. Now we come upon a translator who feels the Indian language, for whom it is a sentient instrument, rich with emotional connotation. Gone are the careful non-committal renderings of the trained linguist. We read of Coyote's "delirious exultation," of Skunk who "reeks foully," and who "scampers away panic-stricken," passages such as the following owe their vividness chiefly to language. "At this moment Coyote shouted in glee, 'Be a feather!' Away he flew by himself. 'Be a sinker!' and downward he glided . . . when in his hilarity he got his tongue twisted . . . and plop! into the water he dropped." There are no cumbersome circumlocutions. If English refuses to express the Indian idiom, it is bent into new shape. So, emotive meaning is translated into emotive meaning, and the result is a brilliant narrative full of warmth and color.

For the first time in my experience, I have read American Indian myths for sheer pleasure, revelling in the delicious humor, in the perfectly attuned sympathy

between the narrator and the characters of the myths, in the delineation of character and situation. Indeed, I had to force myself to take note of the familiar Plateau incidents, which are there, however, to reward the seeker. To miss reading this volume would be a minor calamity.

D. DEMETRACOPOULOU LEE

CLAREMONT, CALIFORNIA

Civilization. THOMAS WILDCAT ALFORD. FLORENCE DRAKE (ed.). (xiii, 203 pp., 22 pls. \$2.50. Norman: University of Oklahoma Press, 1936.)

Although the volume under review contains a considerable amount of heretofore inaccessible ethnographic data relating to the Absentee Shawnee, this data should not be used, except with caution, as source material on the Shawnee. The author, Thomas Wildcat Alford, is an Absentee Shawnee Indian of mixed Shawnee and white ancestry whose knowledge of Shawnee ethnography is fragmentary. His background and proclivities to white culture have already been discussed in a previous review.¹

The book is in the form of an autobiography; the material was prepared for publication by Florence Drake, who has preserved in creditable manner Alford's spoken and written English style. Alford was born in 1860, so his biography opens in the period when the Absentee Shawnee were living in the Creek section of Indian Territory, goes on to their period of sojourn in Kansas during Civil War days, and closes in the allotment period. The bulk of the book (Chaps. 9-32) is concerned with case material bearing on Indian acculturation problems. A sketch of the history of the Absentee Shawnee, written by Alford (pp. 200-203) is mainly valuable for the concise statement it contains of the movements of the two major Shawnee groups during the 19th century; the material can be substantiated by treaty records and is in all essentials correct.

Some sixty-eight pages are devoted to ethnography (Chaps. 1-8). As his knowledge of Shawnee ethnography is limited, Alford rarely can give a well rounded description of any feature of Shawnee culture, there is, however, a good discussion of house building (pp. 15-17), the chapter on Indian foods (pp. 35-42) is also good, and the description of the spring Bread dance is fairly complete and reliable except for a few details. Occasionally Alford's material is self-contradictory, as when he says (p. 19),

Standards of conduct were just as rigid as the laws of any other people, but force seldom was used to enforce good conduct among the Shawnee. *Each person was his own judge,*

but later observes (p. 49),

misdeeds did not go unpunished. Punishment was of many kinds, and was determined by the gravity of the offence. Our chief's word was law, and any persistent refusal to obey the acceptable but unwritten code of honorable conduct was punishable by severe flogging or even death. *Anyone who refused to take his punishment . . . was ostracised from his tribe, his friends and his family.*

¹ *American Anthropologist*, Vol. 37, p. 675, 1935.

several questionable statements are made, among which are the following: "A child is named he automatically belongs to the same Um-so-ma [name group³] as the one who named him (p. 3, also p. 4). . . . Each clan [division³] had a certain duty to perform for the whole tribe (p. 44). . . . Family ties are binding . . . and succession is through the female line (p. 87).

From none of several Shawnee informants have I been able to obtain evidence which confirms these statements: in conversation Alford himself contradicted his remarks on maternal succession.

The title of the autobiography is, I believe, of Alford's choosing. The volume is attractively bound in cloth, and for lay readers especially, constitutes an interesting addition to the "Civilization of the American Indian" series which the University of Oklahoma Press is publishing.

ERMINIE W. VOEGELIN

KEENCASTLE, INDIANA

Ethnobiological Studies in the American Southwest. I. Uncultivated Native Plants Used as Sources of Food. EDWARD F. CASTETTER. *II. The Ethnobiology of the Papago Indians.* EDWARD F. CASTETTER AND RUTH M. UNDERHILL. *III. The Ethnobiology of the Chiricahua and Mescalero Apache.* EDWARD F. CASTETTER AND M. E. OPLER. (Bulletin, University of New Mexico, Biological Series, Vol. 4, Nos. 1, 3, 5, 1935-36.)

Symposium on Prehistoric Agriculture. (Bulletin, University of New Mexico, Anthropological Series, Vol. 1, No. 5, 1936)

This ethnobiological series, of which these are the first three bulletins, promises to be of considerable interest and utility to field workers in the Southwest and adjacent areas.

The first paper is general and opens with a statement concerning the scope and aims of ethnobiology, bringing out the fact that it is by no means confined to a listing of the natural resources which are utilized. The bulk of the paper contains under one heading the results of the ethnobiological researches which have been done to date in the American Southwest" and incorporates, in addition, field data of the author and his students. For comparative purposes the content is arranged in alphabetic order of the plants considered. The local bibliography appears to be well covered. Although comparative material from central California is cited, there is no mention of the excellent accounts from the intervening Yuman peoples (Spier: Havasupai, Maricopa; Gifford: Cocopa, Yavapai; Kroeber: Walapai.)

The second paper concerns the Papagueria, defined as the country "between the Gila River in southern Arizona and the Altar in northern Sonora." The content is said to be "a brief account of Papago customs and ethnobiology as reported by the

² C. F. and E. W. Voegelin, *Shawnee Name Groups* (American Anthropologist, Vol. 37, pp. 617-35, 1935)

³ *Ibid.*

first Spaniards and as seen in the remoter villages at the present day." Elsewhere (p. 30), however, one reads that the paper is confined to a consideration of Papago reservations within the United States. Despite this somewhat initial vagueness in locale, individual statements are well localized. A very considerable body of ethnographic material is included as well as the Papago utilization of plant and animal resources. The sections on cultivated plants are of particular interest, unfortunately, Papago maize is not identified as to variety, although this may be because specimens were not obtainable.

The introduction to the third paper contains a word on the natural landscape, helpful for orientation, as well as much ethnologic material. The latter includes a discussion of social and religious aspects, the psychological attitude toward natural phenomena, etc., as well as material culture. The second section, on plant resources, would perhaps have been somewhat more effective had the staple foods been segregated and explicitly specified as such and had the seasonal aspects of Apache diet been emphasized.

The *Symposium*, a collection of essays, opens with a stimulating and well written introduction by Brand, followed by papers by Weatherwax, Kempton, Bartlett, Franke and Watson, Halseth, Haury, Jones, and Hendry and Bellue. While none of the material appears to be particularly new or particularly startling nonetheless a symposium of this sort is of distinct value, familiarizing the archaeologist and the botanist with one another's problems.

For want of space it is not possible to comment on all the papers, the botanists' two on maize perhaps are of most interest to anthropologists at large. Of general theoretical interest is the botanists' use of distributional data for determining the center of origin. Concerning maize, the authors comment, among other things, on its high yield compared to other cereals; its inability to survive except as a domesticate, the significance of the cob, possible ancestral forms, the original center of domestication, the relation to teosinte, the interfertility of the varieties of maize and of maize with teosinte; and the time requirements for varietal diversification. Although mentioned briefly by Kempton, it is singular that, either deliberately or inadvertently, in neither of the papers on maize, nor in that on cotton, is serious consideration given the extensive research of Vavilov and his associates.

ISABEL T. KELLY

GHA PULIDO

Tuzigoot: the Excavation and Repair of a Ruin on the Verde River near Clarkdale, Arizona. LOUIS R. CAYWOOD AND EDWARD H. SPICER. (119 pp., 28 pls., 14 figs. Berkeley: Field Division of Education, National Park Service, 1935.)

Tuzigoot is a rambling masonry pueblo of 86 ground-floor rooms which was excavated during 1933-34 with Federal relief funds. The excavators present a summary of local archaeologic periods and proceed to describe the site and the excavation, the growth of the village, architectural features, the specimens and burial customs, closing with summary comments. Accompanying the report are two ap-

pendices: one on a spectrographic examination of potsherds by Morris G. Fowler and the second, by Helen Forsberg, on the skeletal remains.

Tuzigoot yielded a large number of specimens and all bodies of material are equally thoroughly described. Architectural features are treated in detail, considerable attention is given to pottery and, in description of the other artifacts, the method recently inaugurated by Kidder in *The Artifacts of Pecos* has been followed with admirable results. On the other hand, the presentation bears repeated evidence of having been carelessly assembled, at points the discussion is not conducive to a ready grasp of critical information, and the basis for conclusive statements is not always evident. The substitution of the authors' own term Prescott Black-on-gray for Verde Black-on-gray, a term which has been in literature since 1930, is also to be regretted. If there is good reason for the new term it is not given. The authors of the present paper are not lone offenders; there are other cases. The nomenclature of Southwestern pottery types is complicated enough without further confusing the situation with unnecessary synonyms.

Using pottery type-time correlations of the Museum of Northern Arizona, the writers place the occupancy of the site between A.D. 1350 and a point shortly prior to 1000. The Pecos classification is not used and, for benefit of those so-minded, we may remark that the structures and the bulk of the recovered material appear to be assignable to Pueblo III and early Pueblo IV. Indications of previous occupancy consist of a miscellaneous double handful of definite Pueblo II and possible Pueblo I sherds. The authors' conclusions that certain of the excavated rooms can be assigned to the same period as these sherds and that the site was continuously occupied from the earliest to the latest period are open to serious question.

The Verde valley is archaeologically important for cultural affiliations in three directions: with the Pueblo archaeological types to the north, the Hohokam to the south, and a little known complex to the west (the Gladwins' "Yuman root"). While the reader may question some features of interpretation, the paper presents a large amount of new data from this region and forms a welcome addition to the literature of Southwestern archaeology.

W. S. STALLINGS, JR.

LABORATORY OF ANTHROPOLOGY

Handbook of Latin American Studies: a Guide to the Material Published in 1935 on Anthropology, Archaeology, Economics, Geography, History, Law, and Literature. LEWIS HANKE (ed.). (xv, 250 pp. Cambridge: Harvard University Press, 1936.)

This Handbook is the first publication of a projected series to be issued annually under the auspices of the Committee on Latin American Studies formed in 1935. The aim of the series is to provide an annual record of important publications in the various disciplines concerned with Latin American culture. Special articles, chiefly bibliographical in scope, are appended to the bibliographies proper which make up the bulk of the Handbook. The bibliographies and articles are written by a group of scholars.

Anthropologists and archaeologists will be interested particularly in pp. 5-22 of the present volume giving two excellent annotated bibliographies for 1935. The first is for Middle America—one by Tozzer on physical anthropology, linguistics, archaeology, and hieroglyphic writing, the other by Redfield on ethnology. For most of the items listed in the bibliographies thumbnail comments, analytical and critical, are given. The two lists are preceded by statements reviewing current anthropological and archaeological trends and field work in the Middle American area.

JOHN M. COOPER

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AFRICA

Culture Areas of Nigeria WILFRID D. HAMBLY. (Anthropological Series, Field Museum of Natural History, Vol. 21, No. 3, pp. 365-502, 1935. 68 pls., map.)

In this brief survey of Nigerian ethnology, Hambly attempts the sort of analysis which is much needed for all parts of Africa. Aligning his own observations with those of other travellers and students, he correlates the distribution of races and languages, distinguishes "Negro" from "Mediterranean" cultural traits, and discusses the effects of the desert and forest borders and of the refuge areas of the plateau. South of 9° N. lat. he finds people of Negro race speaking Bantu and Semi-Bantu languages; north of this line, Hamitic and Semitic languages with diluted Negro types.

To illustrate the "predominating uniformity in the pattern of forest Negro culture from far West Africa through Liberia, the Ivory Coast, Ashanti, Dahomey, Nigeria, Cameroon, the Congo basin, and into the land occupied by the Ovimbundu of Angola," Hambly mentions: skillful iron-working with its attendant rituals, the carving of ivory, wood, and calabashes; bark cloth; raffia work; coiled basketry, pottery by coiling and punching, hoe cultivation by women; the lack of animal husbandry, fishing by means of nets, spears, poisons, conical basket traps, and weirs; huts with thatched roofs; craft guilds; similar musical instruments, songs, and dances, secret societies; a classificatory system of relationship with cross-cousin marriage; matrilineal descent, warfare and slavery; blood brotherhood rites, strong monarchies, the ancestral cult, etc., etc. For the comparative and historical purposes implicit in Hambly's paper, a list of this kind means very little. Some of the features are almost world-wide, some are strongly conditioned by environment, while for others the regional variations indicate quite independent histories.

The author designates most "non-Negro" elements as "Mediterranean"—a term which he uses with judicious ambiguity for Roman, Cretan, and Phoenician as well as Egyptian. He allows a possible Egyptian origin for the following traits present in Nigeria: kingship rites such as those of the Jukun, *cire perdue* casting, iron working (with a very big question mark), the harpoon of the Buduma, the papyrus reed canoe of Lake Chad, the vertical loom, coiled basketry, leather-working (again with a question mark), sandals, lamps, tattooing, kohl under the

henna, silver-working, a "systematic polytheism," the funeral boat, terra-cotta heads, the umbrella, and the shaduf. Meek in his *Northern Tribes of Nigeria* (vol. 2, pp. 161 ff.) gives another list with the same implications: "Mummification, certain burial practices," glass-making, the West African harp, long-horned cattle, rice, maize, king-killing, beliefs relating to the soul, etc., etc. Hambly offers no evidence, historical or otherwise, for such diffusion. Fortunately he does not accept Meek's suggestion that Egyptians came to West Africa for gold as early as the B.C., or that the Egyptian "King So of the Bible" may have given his name to the early Nigerians.

To be interesting these affinities should not be merely geographical, they should be placed in history. For example, when Hambly derives the umbrella from Egypt, does he mean Ancient Egypt? If so, he is centuries off. It came indeed from Egypt, but in the Middle Ages, when the sultans of Mali were emulating the pomp of the Mamelukes. We should make the same distinction for iron-working, lamps, rice, henna, kohl, and the shaduf, which were probably brought to Negro Africa across the desert after the fourth or fifth century, when camels came into general use in the Sahara. As for social and religious things, Ancient Egypt had nothing that the Negroes wanted. Students of Negro ethnology would do well to give far less prominence to the land of the Pharaohs.

I fear that in final effect this paper is muddled and disappointing. Hambly gives the same "sense of being confused with factual material" as do Meek and other writers on Nigeria, a confusion which he explicitly sets out to avoid. He provides, however, a harmless introduction to the area, a large bibliography, and a series of excellent photographs.

WALTER CLINE

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The Bantu Tribes of South Africa: Volume IV, Section II, Plates xli-lxxx: The Vachopi of Portuguese East Africa. A. M. DUGGAN-CRONIN. With an Introductory Article on the Vachopi, etc., by Henri-Philippe Junod. (20 pp., 40 pls 25 s. Cambridge and Kimberley: Deighton, Bell and Co., 1936.)

This volume forms part of an attractive series on the Bantu tribes of South Africa the avowed purpose of which is to present a photographic record of the rapidly vanishing native life of the general area. The published results undoubtedly have their greatest appeal for those who advocate the premise that a good illustration, a photograph or a drawing, is frequently more desirable than pages of explanatory text. But no series of photographs, however carefully taken and however detailed, can completely replace an adequate descriptive ethnology. The photographs therefore may be judged merely on their merits, as records of physical types and especially modes of life which will not be observable in Portuguese East Africa many years hence. The publishers have obviously been well aware of the shortcomings of a mere series of photographs, excellent as they are, and a brief summary of the culture of each group depicted introduces each series.

Henri-Philippe Junod, in this volume on the Vachopi, has supplied a succinct account of the main features of their culture. The fifty plates, with their explanatory captions, broaden somewhat the limits of the horizon laid down by the brief text. The Vachopi, Junod tells us consist of three groups, the VaLenge, the VaChopi, and the VaKhoka, numbering approximately 200,000. Their territory lies between the Limpopo River, Inhambane Bay, and the Indian Ocean: on its westward boundaries is the country of the VaThonga. Their distinctive Bantu dialect, according to Junod, stands linguistically between the Central and Southeastern Bantu groups. His chief interest is to record those elements of culture still extant which can be isolated as having been part of the Chopi pattern before their long period of intermarriage with invaders of their territory, especially the Venda and the Thonga. The material and social traits still uninfluenced are those known to be most characteristic of southern Africa and may be listed somewhat categorically. The Chopi are a bow and arrow people; they live in bee-hive shaped huts set up in rows enclosed in kraals; they practise filing of the teeth, tattooing, and scarification; babies are carried by straddling them on the hip or in a bark sling on the back; other carriage is typically on the head. The Chopi are fisherman, using hooks, nets, and spears; coconuts and manioc are important factors in their diet. Their principal manufactures are bark fiber and cloth, of which, by the way, there are excellent illustrations, and baskets; they are expert wood carvers and have achieved dexterity in the introduced art of iron working. Four different types of xylophones are made, so that grouping of instruments to form an orchestra is commonplace.

As among other Southeastern Bantu peoples, descent is reckoned patrilineally and residence is patrilocal. Circumcision appears to be the survival of an old Chopi custom, with probable Asiatic origins. For boys, a rather elaborate initiation ceremony is held, the main features of which are seclusion in the bush, circumcision, the observation of food taboos, all of which culminate in the teaching of the secrets of hunting and other tribal arts and crafts.

The Vachopi are now rapidly disintegrating, physically and culturally, under the onslaught of Portuguese mixed-bloods and Asiatics among the VaKhoka and of other Bantu tribes among the VaLenge. The authors are therefore to be commended for making available those few elements of their aboriginal culture still recordable.

BEILA WEITZNER

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ASIA AND OCEANIA

The Mysore Tribes and Castes, Vol. I. L. K. ANANTHAKRISHNA IYER. With introductions by R. R. Marrett and S. S. Levi (Mysore: Mysore University, 1935.)

This volume although listed as Volume I is the third of a four volume work on the tribes and castes of Mysore representing the results of a study commenced in 1903 and conducted originally by H. V. Nanjundayya and since 1924 by the author. Volumes II-IV contain the descriptive material. Volume I is intended not

only to serve as an introduction to the others but to acquaint the reading public of India with the "sciences of Anthropology, Ethnology, and Ethnography." The various chapters include such subjects as Caste, Marriage and the Family, Totemism, Magic, Animism, Religion, Funeral Customs, Occupations, Village Communities, Evolution of Taste in Dress and Ornaments, Foods, Games and Dancing. In each chapter the author introduces the subject with a general philosophical discussion of its "origin and evolution" as illustrated to his satisfaction by examples taken from various parts of the world, and by what he presumes to be or to have been the attitudes of primitive or primeval man concerning it. Hence it is no surprise to learn that the vast majority of non-Indian bibliographical sources are confined to Westermarck, Briffault, Frazer, and Jevons. Into such an evolutionary matrix the Indian appearances are cast and discussed as stages of development in human history. The present policy of the Indian government for aiding the tribes with less complex cultures is described in part and other data of sociological interest are presented in passing. One chapter devoted to racial studies is written by E. von Eickstedt who stresses "natural geographical environment" as a determinant in the development of races and of subsequent stocks resulting from intermarriage. The book is generously illustrated, principally by photographs of individuals and groups, forests, scenes, and views of government dams.

D. S. DAVIDSON

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The Hos of Seraikella, Part I. ANATHNATH CHATTERJEE AND TARAKCHANDRA DAS. (Anthropological Papers, University of Calcutta, n.s., No. 1. Calcutta: University of Calcutta Press, 1927.)

The Bhumijus of Seraikella. TARAKCHANDRA DAS. (Same series, No. 2, 1931.)

The Wild Kharias of Dhalbhum. TARAKCHANDRA DAS. (Same series, No. 3, 1931.)

All three monographs deal with Munda speaking tribes in the Chota Nagpur area. Each study is the result of an excursion made to the people by members of the Department of Anthropology of the University of Calcutta. Although annual departmental field trips are highly commendable, the stay among each tribe was brief and the results are correspondingly slender.

Each of the tribes has been influenced by neighboring Hindu peoples, largely in matters of ceremonialism and social organization. The Bhumijas have been most thoroughly Hinduized and are practically one of the low castes. Of the three tribes, the "Wild" Kharias have been least affected by Hindu culture. However, there are some Kharias who come within the caste system. This is true of many aboriginal tribes in India. The culture of one section of the tribe is often saturated with Hindu rites, deities, agricultural techniques. The members of this section are usually tribesmen who have migrated to the plains. A second section of the tribe keeps to the hills, has little contact with the plains; bears slight evidence of Hindu influence. Often there is a third section which stands midway between the hills and plains

both culturally and physically. Tribal appellations in India commonly represent linguistic and historical, rather than social entities.

Among the Ho, it is the Hinduized division of the tribe which is considered inferior and is treated as such by the Ho of the interior. The reverse is true among the Bhumija, where the more highly Hinduized section will have nothing to do with the others. The latter is generally the case, since the hill people represent absolutely zero in the Hindu social scale.

The Ho erect megalithic structures as ossuaries. Animal labor may not be used in their construction. The building of an ossuary is a costly affair but great prestige accrues to the gens which sets up large structures. Hence great megaliths are still being built.

Although the three tribes together number over half a million, they constitute but a small fraction of the total aboriginal population of India. The 1931 census classified some twenty-five million people as primitives. For this vast number there is only a handful of competent ethnological treatises. If there is anywhere a happy hunting ground for anthropologists, it is India.

DAVID G. MANDELBAUM

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A Study of Ancient Ships in Japan, Vol. 2. Part 1: Floats. SHINJI NISHIMURA. (ix, 162 pp., 14 pls., 33 figs., map. Tokyo: Society of Naval Architects, 1936.)

This volume discusses calabash, deerskin, and clay floats in early Japan. The description of calabash boats seems to have been published separately and was reviewed by Dr D. S. Davidson in the *AMERICAN ANTHROPOLOGIST* (Vol. 37, p. 352, 1935). In the two additional sections on inflated skins and pottery floats, Professor Nishimura pursues his double technique of study. On the one hand he employs minute textual criticism to establish the early existence of such craft in the Far East. On the other hand he jots down random occurrences of similar vessels through the whole gamut of time and space. There is a strangely irreconcilable mixture of the most rigorous historical research and of pan-diffusionistic conclusions which are reminiscent of Elliot Smith, to whom the author makes acknowledgement in the introduction. If there is any value in the publication, it lies in the establishment of early Japanese, Korean, and Chinese evidence for the three types of floats discussed. This value is, however, mitigated by the absence of any dating for the author's literary sources.

CORA DU BOIS

NEW YORK CITY

Die Verbreitung der Hausformen in Ozeanien. HERBERT TISCHNER. (Studien zur Völkerkunde, Vol. 7. 252 pp., illus., 9 maps. Leipzig: Verlag der Werkgemeinschaft, 1934.)

This careful, sound analysis of the distribution of house forms in Oceania may be welcomed as a timely and valuable addition to the few synthetic studies of the

With other recent distribution studies it shows that sufficient data on Oceania have accumulated so that such analyses may be undertaken with increasingly reliable results. In fact, as Dr Tischner points out (p. 232), the correlation of a large number of such detailed studies of Oceania, and also of Indonesia, in spite of their limitations, is a necessary step toward the understanding of Oceanian culture.

The author is aware of the difficulties involved in the study of the geographic distribution of culture elements. He believes the greatest of these is that they present a flat, spacial picture, lacking time depth (p. 232). The three *Baukreise*, defined by Frobenius,¹ he rejects chiefly because house types have been classified and their original type forms and development postulated mainly according to outer form without sufficient knowledge of their structure (pp. 1-2). Dr Tischner avoids this difficulty. He carefully weighs and compares the sources and sincerely attempts to understand and preserve their content. He classifies and describes house forms according to (1) *Standort* (i.e., tree houses, pile houses, houses on mounds, houses on the ground, caves, stone houses, earth holes) and (2) *Gliederung der Häuser* (i.e., *Grundriss und Aufriss* (i.e., wind shelters, round and oval houses, polyangular and rectangular houses, houses with additional extensions). Some information concerning the cultural and natural setting of house types is also recorded. Their functions and a list of native house names are treated in separate chapters. Hence, quite apart from the author's interpretations, the monograph is a reliable, descriptive source book. It is supplemented by detailed distribution maps and a useful bibliography. Unfortunately there is no index.

Dr Tischner is extremely cautious and avoids interpretations and conclusions based on insufficient evidence. He does, however, point out that the rectangular gable house with straight ridge pole may, from its wide use in Oceania, be considered the common "austronesian" building form. The meaning of "austronesian" is not explained. The *Pulldachhaus* of the Marquesas and the *Tonnendachhaus* appear from their distribution and structure to be closely related to the rectangular gable house (p. 229). Separate men's houses (as distinguished from women's or family dwellings), assembly and guest houses are universally distributed and may also be equated with the "austronesian" pattern (p. 230). He finds, furthermore, that the clearest distribution relations appear in the linguistic data, which fall into two groups designated by the ur-austronesian words *yuma* and *balaj*. The *yuma* house is distributed continuously in Melanesia and Micronesia and is absent in Polynesia, while the *balaj* house is found only on the western margin of Micronesia, the adjoining eastern margin of Melanesia, and in Polynesia (map 9).

Because of the high quality of Dr Tischner's work, one wishes that he had made a more systematic investigation of the function of each house type in its cultural and natural environment. Such an investigation is essential to the understanding of the problem and it would add a good deal to the value of the work.

Concerning house types in New Caledonia, it may be noted that Leenhardt's equation of house mounds with round houses does not, as Dr Tischner believes

¹ *Oceanische Bautypen* (Berlin, 1899).

(p. 228), exclude a sociological interpretation. In fact, according to Sarasin,² the larger and higher the house, the more powerful the inhabitants.

Regarding the problem of house types in the Marianas Islands, the author has not mentioned the regular extended burials, accompanied by artifacts, found between the rows of stone pillars. It is not impossible that the double rows of capped pillars were burial monuments rather than house foundations or both. There is some archaeological evidence pointing toward their having served as house pillars (as Dr Tischner concludes, page 46), especially the fact that they are set deep in the ground and reinforced with stone foundation props, that caps are fitted to the pillars, and large numbers of artifacts are found on the surface of the ground under and around them.³ Furthermore, it may be definitely concluded that Christy's statement (quoted on p. 45), concerning the use of mortar in the Marianas, is incorrect since no evidence of the use of mortar by the Chamorros has been found.

LAURA THOMPSON TUEBING

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GENERAL

Epitome de Culturologia. J. IMBELLONI. (Humanoir, Series A, Vol. 1. 320 pp., 61 figs. Buenos Aires: José Anesi, 1936.)

This is the first volume of an ambitious series, edited by Dr Imbelloni, which will eventually comprise thirty works encompassing all fields of anthropology with special reference to the American Indian and particularly to South America and Argentina. Since, as planned, the series will descend from the most general to the most particular, this *Epitome* (confined to social anthropology) is on the highest theoretical level, treating the philosophy of history, the nature of culture (and its relations to race, geography, and history) and the meaning of civilization.

The point of view of the author (and one is led to believe that he shares it with his colleagues) is made perfectly explicit. Imbelloni is an enthusiastic and unquestioning adherent of the German *Kulturhistorische* school. The neologism *culturologia* is defined as the exact Spanish equivalent of *kulturhistorische methode*, and the author pays his full debt to Graebner, Schmidt, Koppers, Foy, *et al.* In his discussion of anthropological method, the author recognizes but two general points of view: that of the Classical Evolutionists and that of the Graebnerites. When he shows why the evolutionary position is untenable, he is left with only *culturologia*, and of it he says:

We shall see that this method has been triumphant on all fronts, so that in all the world today there is hardly a student disposed to deny its fundamental criticism and its theoretical efficacy (p. 39).

Particular historical reconstructions may be open to criticism, Dr Imbelloni claims, but there are none to gainsay the validity of the theory, or the method, although

² *Ethnologie der Neu-Caledonier und Loyalty-Inselaner* (München, 1929), pp. 130-31.

³ L. Thompson, *Archaeology of the Marianas Islands* (Bulletin, Bishop Museum, No. 100, 1932).

some North American anthropologists, by confusing applications of the method with the method itself, are inclined to think that they disagree.

The volume is therefore devoted, in large part, to an exposition of the *Methode*, and it appears to be excellently and coherently done, with concrete and original examples, including proofs of diffusion from Polynesia to America. This reviewer, unfortunately impatient with the *Methode*, is neither disposed to nor capable of criticizing Imbelloni's exposition and application of it.

A thought comes to the reviewer that perhaps will come to many another North American. Why is it that, apparently, South American anthropology should follow a school whose method, in large part, we have proved to our satisfaction is unprofitable? Imbelloni has obviously read English, American, and French scholars as well as German, and yet apparently with little hesitation has discarded other points of view in favor of the *kulturhistorische methode*. Is it that Graebner's scientific theories are actually more satisfying than those of, say, Boas (justly or unjustly), or is it simply because personal and academic connections of the Argentine are closer with Germany than with England, France, or the United States? If it is only the latter, cannot the situation be to some extent remedied? For from our point of view anthropological resources on a continent almost virgin may otherwise be wasted for decades to come. "Intervention," academic no less than political, is not in fashion; yet a more direct exchange of ideas than that to which we have been accustomed might serve a most useful purpose.

SOL TAX

CARNEGIE INSTITUTION OF WASHINGTON

The Dream in Primitive Cultures. J. STEWARD LINCOLN. (xiii, 359 pp., plate, 18s. London. Cresset Press, 1935.)

The reviewer is in the position of knowing a good deal more about psychoanalysis and a good deal less about ethnology than is required to appreciate this book; with most readers, presumably, the case will be reversed. Mr Lincoln follows the path worn hard by traditional Freudian raiders in the ethnological field; he assumes the usefulness of the Freudian concept system and then finds confirmations in ethnological literature, especially that of continental United States. Since few ethnologists have the skill to affirm or deny the Freudian postulates, the work is bound to be more or less meaningless to them, beyond that it may be harmful by further increasing the distance between the hard-bitten objector and the fertile mind of Freud.

After a spotty canvas of isolated examples to demonstrate the universality of the Oedipus or nuclear family complex the author discusses dreams; sample dreams from various cultures are said to give evidence of the existence of this complex. Some proof is advanced for the hypothesis that dreams play a creative role in the formation of culture patterns and institutions. Mr Lincoln makes a contribution to the theory of distribution of culture patterns, he asserts that the assimilation of patterns from other folk is frequently due to the recurring need for new solutions

to the ambivalence dilemma arising out of the Oedipus situation. The author makes excellent use of the distinction between "culture pattern" or induced dreams and "individual dreams." Culture pattern dreams are indicated as an aspect of collective life since they tend to disappear when the unity of a society is broken down. That is, as it ought to be, but is not, a discussion of what are the psychic mechanisms by which a stereotyped dream may be induced in an individual. Here is a place for the creative use of Freudian perception in ethnological field work. Mr Lincoln is aware of the caution which must be used in interpreting dreams without abundant associations from the dreamer and knowledge of his actual life situation; in spite of stating the caution he does not follow it and often permits himself to make interpretations on the basis of a scanty knowledge of the bare contours of the society. The lack of the dream context cannot be compensated for by any amount of personal conviction or theoretical ingenuity.

It is a general defect of the book that the Freudian formulations, such as the Oedipus complex, are not reduced to their impulse components on the one hand, and cultural prohibitions and restrictions on the other. When this is done the whole issue can be made more clear and intelligible from the standpoint of the student of other societies.

If this review seems somewhat severe, let the reader reflect on how refreshing it would be to have an analyst take the trouble to do a tight little piece of field work which was really unexceptionable from the technical standpoint and which actually used the analytical technique—however modified—instead of drubbing the existing materials with the shillalah of analytic concepts. If analytic perception were used in a modest and realistic field research, its results would undoubtedly be much better received by social scientists. An analytically sophisticated life history of a primitive, for example, would be a real contribution and would make all cross-cultural comparisons more vital. This book is frustrating mainly because it so lamentably fails to provide a growing point for the actual use of analytical skills within the field of ethnology; it is, of course, much less objectionable than other works of its kind, since the author has some sophistication as an ethnologist.

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DISCUSSION AND CORRESPONDENCE

PUBLICATIONS NEEDS AND OBJECTIVES

Incidental to a recent review in this journal of Dr Lowie's book on the Crow Indians, Dr Wissler makes several statements which are likely to be construed as conviction that most ethnographic material might as well remain unissued. "All the important results of field work can be published privately" [viz., commercially]. "Perhaps the ideal of the future will be to publish brief summaries followed eventually by a [commercial] book. If, then, all field notes were typed and a few sets made available for circulation when needed, the costly memoir might be omitted."

This is startling doctrine. The implication seems to be that what is "important" will sell; and therefore that what will not sell a thousand copies is unimportant. Is scientific significance really to be measured by quantitative appeal? If so, are we ready to proceed to advertising, sales organization, and propaganda? I doubt it. Dr Wissler intends this. He has not defined importance. He can hardly mean that an ethnographic book which will sell to laymen is more important scientifically than one which will not. In that case a book that sold 20,000 copies would be more important than one that sold 1000, and it is plain where the race would end—especially with the lure of money at the goal. Perhaps he believes that an ethnographic work which makes an appeal on sight to psychologists is more important than one that appeals to anthropologists only. This proposition might be arguable, but it is a defeatist attitude for anthropology. Has Dr Wissler really wearied of his life work—has budgetary pressure possibly discouraged him—or, sensing certain very real deficiencies and wastes in our present habits, has he said in a thoughtless moment more than he intended?

The "deposit in two or three central libraries of typed copies" of notes may carry appeal to the Finance Committee of a harassed Board of Trustees. But every scientist and scholar knows that it will reduce the effective operation of science and the progress of scientific knowledge to a fraction. This need not be argued. One has only to think of ten concrete cases, to realize that in six, scientific work would be impeded, and in three, blocked. Because a typewriter can run three carbon copies, we are not really going to go back to pre-Gutenberg days in science, and leave the printing press to serve propagandists, politicians, writers of best sellers, and others with an interest.

There is a clause, now three hundred years old, in the unwritten constitution of science, that the results of scientific work do not exist in the body of science until they are published, and publication has meant adequate, freely accessible distribution in print. Is the suggestion that this clause be amended for the whole of science, or that our own branch is so feeble that it should be exempted from the clause?

The justifiable basis for Dr Wissler's over-pessimistic renunciation is that the vast majority of our ethnographic and archaeological descriptions are over-lengthy.

written and perhaps over-expensively printed; and descriptive material—new facts—constitute and probably will long continue to constitute the bulk of what anthropologists have to offer. Such facts are the basis on which better understanding rests. Where we possess defined technical terms, or well understood semi-technical ones, like multiple-rod coiling, paddle-and-anvil smoothing, figure-4 trigger, atlatl, tangential feathering, cross-cousin, soul-loss, there is nothing at all gained for science, and time and clarity as well as costs are lost, by using a layman's description. "Tertiary arrow-release" is perfectly definite, and allows superstructural variants of the technique, when they occur, to be set into specific relief. An attempted description every so often remains incomplete or ambiguous enough to mystify or irritate the user of the account. A listing, with or without summarizing tabulation, of the contents of a series of graves is far more readily analyzable by the next archaeologist, than a separate description of each grave for content, orientation, depth, soil, etc. There was a time when any excavation of prehistoric implements was an event, and the recognition of cross-cousin marriage or disease-object sucking constituted an exciting discovery. Such matters have long since become expectable routine of field investigation, and to treat them otherwise than to indicate their presence or absence, special variants of detail, and their functional relations, is worse than useless. In an account of kinship, "m br d d," as compared with "mother's brother's daughter's daughter" or "first cross cousin once removed on the mother's side," is not only eighty or ninety per cent cheaper to print, but far more readily and precisely grasped by the anthropologist concerned with kinship—and all others will avoid the passage anyway as if it were poison.

As we gradually learned that over-generalization of new primary data often led to error and always prevented re-analysis and check by later workers, we began to make a fetish of giving all data *in extenso*, irrespective. We no longer allow informants to generalize for us, but control their theory that they do not marry relatives, or that they do marry grand-daughters, by collecting case-histories or genealogies. But the fact that an ethnographer has collected twenty elaborate genealogies is no valid reason for spending several hundred dollars on engraving or setting by hand large folding sheets that have to be tipped in—dollars that would finance more investigation or print other data which remain unpublished. If the reporter will tell us that his genealogies contain 217 marriages of men of which 76 are with cross-cousins, 40 with f s's d and 36 with m br d, we have the essential facts on that point, and no intelligent fellow-ethnologist would waste his time making the recount in the family trees. Why then print the genealogies, or more than perhaps one as a sample? Many of us, including myself, have in the past been guilty of just such waste—and there is corresponding waste in archaeological reports. But essentially it is pedantry: either we love our data too much to let go of any of them even when they no longer contribute to the interpretation of a problem, or we are unsure of ourselves and wish to prove that we have at all costs used the most modern and approved techniques.

Similarly with conflicting testimony of informants. Sometimes this needs to be given verbatim; but hardly ever with the habitual bald repetitions and irrelevant

details of the native. Mainly what we want to know is how much testimony the ethnographer had, the degree of its variation, and his best judgment as to the probable truth underlying the conflicting testimony. If he consistently takes refuge behind the sanctity of what Lena Antonio and Happycamp Jack said and refuses to draw the judgment which he should be able to render better than anyone else, he brands himself as being a phonograph rather than a thinking machine, and his claims for publication funds deserve only secondary consideration. Ditto for the archaeologist who sees virtue chiefly in imitating a camera.

As a result of both writing and editing extensively for many years, I have become convinced that the gross of our descriptive publication in ethnography, and probably archaeology also, could on the average be reduced twenty to thirty per cent in bulk and cost by rearrangement and rewriting, without loss and probably with gain in scientific effectiveness. With use of the "telegraphic" or "telescoped style," it can be cut down another ten to forty per cent, according to how compact the author's presentation is before such condensation. This style offends some, but we ought hardly balk at what systematic and descriptive biologists have done for a century. Only, to make the telegraphic style effective instead of a mere minor nuisance, one must do more than write customary prolix English and then strike out the articles and copulas. One must think in the condensed style; and this means that authors cooperate. Finally, another economy, as high as perhaps forty per cent in favorable cases, can be effected by the use of offset printing or other manufacturing devices. All in all, it seems reasonable to say that half of the cost of what now goes into printing the descriptive portions of our ethnographic and archaeologic product could be saved, or, that our present institutional budgets for printing suffice to print twice as much material as they issue.

Why then do we go on with masses of good material that never see the light, or of which only selections are published, and with delays and vexations in what is issued? There are three chief reasons. Two are due to authors and editors, one to institutions.

First of all, the ethnographic field investigation or archaeologic exploration is still, to many of us, too much of an adventure. We identify ourselves with a culture because we have dealt with it; we fall in love with our material, we try to preserve for perpetual record every scrap of it, irrespective of whether the material bears on a problem or fills out a picture or not.

Next, we forget that though our data may lead to insights of general importance, the data themselves are highly technical, that they mostly can be transmuted into interpretation only by technicians, that they are record material and not reading matter. The average descriptive ethnological monograph is perhaps actually read, on its appearance, by a dozen persons. Several dozen probably glance it through, enough to appraise its content and quality, then restore it to the shelf with a mental note that when they want such and such information they can find it there. In the end, the monograph is consulted and used probably by hundreds of workers dealing with hundreds of different problems, but—and this is crucial so far as publication costs are concerned—it is reference material which is not read as a book is read.

As authors we persist in writing our descriptive material very much as if we were writing books for the public. We are no more doing so than is the biologist describing new species; and it is time to recognize the fact.

The third factor is the attitude of many institutions, who still prefer publications which look like books and have a visible prestige value irrespective of intrinsic scientific value. This attitude will presumably not be overcome as long as it is fostered by the anthropologists who advise the budget-making bodies of institutions.

Our task as anthropologists has two extreme aspects. At the foundation is the gathering of new knowledge, critically and soundly obtained, which means in general that it must be gathered by trained men. At the top is the valid interpretation of the totality of this knowledge in terms significant to other scientific specialists and to intelligent non-scientists. There are no publication difficulties about these interpretative fruits, at least not in countries which still preserve essential freedom. But without the humble roots of data, there will be no fruits.

Dr Wissler says of a chapter: "It reaches for the emotions, is highly selective, speaks as an informing document, but achieves its objective; it grips the reader." There is no quarrel with this objective. But one must protest absolutely the seeming implication that this is the end of science, and that the rest of it can be summarily dispensed with. That an anthropologist can write a salable book is nothing against his anthropological competence or integrity. But we are hardly yet ready to judge his anthropological ability and value solely by the public salability of his product. That would make *Growing Up in New Guinea* a more significant contribution to science than the aggregate output of Franz Boas. It would mean that nearly the whole California series might better have remained unpublished, as long as a carbon copy were sent to the Smithsonian and the British Museum, because Stephen Powers, an able journalist, in 1877 had published his *Tribes of California*, which is highly selective and unequal in its interpretations, which reaches for the reader's emotions and grips them, and is weak as an informing document. Perhaps my *Handbook* on California would be permitted to survive as a rump stripped of its small-type matter and factual content.

The elemental virtue in science and scholarship is respect for fact. This means that fact is not only gathered and organized but presented accessibly to other scientists and scholars. Without this soil and root-stock, the growth that Dr Wissler hopes for would soon turn into rank weeds. I believe that he accepts these propositions—because he is an eminent scientist himself. Recognizing real abuses, and seeing more clearly than the mass of his colleagues the need of broad perspectives, he has however overshot the mark in seeming to suggest a reform that would be fatal. There is nothing illegitimate in mutually profitable alliances between authors and publishers. It is to be sincerely hoped we shall see more of them—but to the advantage of science, not at the cost of pulling out its foundations. On the other side it is the business of anthropologists to realize that because it happens that they can present most of their new basic facts in simple English, and deserve to have them published, they are not therefore *ipso facto* writing books or literature in presenting these facts, and consequently have no valid claims on the privileges of

dilution, graces of style, format of a particular type—least of all as long as these privileges are paid for out of other people's money. The sane and enlightened course lies between the bad habits of presentation which persist into modern anthropology from the days of its immaturity as a science, and the repudiation of sound anthropology which Dr Wissler's suggestions imply. Perhaps he was only sending up a trial balloon.

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A REPLY TO A REVIEW OF "SEX AND TEMPERAMENT IN THREE PRIMITIVE SOCIETIES"

To the Editor:

Dr Thurnwald's meticulous review of *Sex and Temperament in Three Primitive Societies*¹ raises several problems which seem sufficiently important to deserve a careful reply. He first summarizes my aim as being "to demonstrate whether and to what degree temperamental differences between the sexes are innate, biologically determined, or to what extent culturally conditioned." In this statement Dr Thurnwald has used the word *temperament* in a different way from that which I intended. My stated purpose (p. xvi) was to make "a study of how three primitive societies have grouped their social attitudes towards temperament about the very obvious facts of sex difference." I use the word temperament in the accepted technical sense for those aspects of the personality which are physiologically "given," as opposed to *character*, the latter being that part of individual personality which is the result of the interaction between native equipment—or temperament—and cultural conditioning; by *personality*, I mean the sum total of temperament, character, and idiosyncratic traits which are characteristic of the whole individual. By *social personality* I mean the approved type of personality for individuals of a given community, whether or not further specified in respect to age, sex, status, etc. So we can speak of the social personality of a Samoan, or in greater detail, of the specific social personality of a Samoan woman of rank. So used the term refers, not to the personality of any given Samoan woman, nor to the average personality of all Samoan women, but to the approved personality for Samoan women, to which each Samoan girl child is expected to approximate as closely as possible. The individuals who, in spite of educational pressure, fail signally in exhibiting the approved personality decreed for them by their community, are deviants. Under conditions of cultural break-down practically every individual of a given age and sex may become, in his own mind, and in the opinion of the community, a deviant.²

I further stated my original problem as "a study of the conditioning of the social personalities of the two sexes, in the hope that such an investigation would throw

¹ American Anthropologist, Vol. 38, pp. 663-67, 1936.

² Cf. Robert S. Woodworth, *Psychology* (3rd ed., 1934), pp. 108-109; William McDougall, *Outline of Psychology* (1926), pp. 354-59.

³ Cf. Margaret Mead, *The Changing Culture of an Indian Tribe* (1932), p. 193.

not upon sex differences" (p. xxii). But nowhere do I suggest that I have found any material which disproves the existence of sex differences. I stated on p. xvi, "This study is not concerned with whether there are or are not actual and universal differences between the sexes, either quantitative or qualitative." The conclusions which I drew from the material which I gathered were:

1. Those temperamental attitudes which we have traditionally regarded as feminine—such as passivity, responsiveness and a willingness to cherish children—can so easily be set up as the masculine pattern in one tribe, and in another be outlawed for the majority of women as well as for the majority of men, we no longer have any basis for regarding such aspects of behavior as sex-linked.

I go on to suggest that certain types of behavior which have been traditionally regarded as sex linked are not sex linked, but are temperamental—i.e., physiologically given—potentialities of some of the members of each sex. Whether or not these temperamental traits are equally distributed among both sexes remains for further investigation, as does the whole problem of sex differences, whether those differences be regarded as innate or as functions of the character formation of individuals with different biological equipment.

In Dr Thurnwald's examination of my description of the approved personality for Arapesh men and women, a great deal of his argument is based upon the misunderstanding referred to above, i.e., the use of the word temperament, and the differences between the actual personality of identified individuals and the ideal of social personality held up to each succeeding generation. The "day dream of innocence" is an Arapesh day dream, which, I point out a great many times, and as part of my argument, they fail to attain, and fail to attain for *both* men and women. The fact that so many of the Arapesh fail to develop the type of mild maternal *personality* which the Arapesh culture approves, may be evidence for the assumption that the *temperament* which they postulate is of proportionately infrequent occurrence. Alternatively we may find that differences in the proportion of deviants are a function of the congruity between the cultural ideal, the social structure, the system of education, etc. It is quite probable that discrepancies within the culture may be even more conducive to the formation of deviant characters than the discrepancy between the social ideal and the proportion of individuals whose temperamental potentialities deviate most radically from the social ideal.⁴

Dr Thurnwald mentions that I do not state the percentage of aberrant persons among the Arapesh, but I stated specifically that they were the unusual cases and on p. 122 dealt specifically with the problem of recording the even and uneventful tenor of the average Arapesh life so as to indicate how unusual these more dramatic cases of maladjustment were. Statistics in a population of about 400 persons of the proportions in which a group of individuals of different ages and both sexes occur are obviously quite meaningless. Researches which depend upon statistical validation cannot be carried out among primitive peoples under the existing conditions of

⁴ Cf. Margaret Mead (ed.), *Competition and Cooperation in Primitive Societies* (1937), "Interpretative Statement."

field work. Such field work does give qualitative results and suggest hypotheses which can be tested quantitatively in civilized communities.⁵

Dr Thurnwald also mentions that I record a great number of differences between the institutionalized socio-economic roles of Arapesh men and women. The reviewer quotes for instance as a "temperamental difference" the fact that boys are allowed to have temper tantrums later in their lives than are girls, that girls are absorbed earlier into the domestic economy, and that the two sexes are trained to have different attitudes towards the supernatural. Here I need only refer again to the sense in which I use the word temperament, and to acknowledge that it is quite evident from the confusion shown that I should have defined my terms more rigorously. The reviewer also raises the question: "Should polygyny not imply polyandry with the other sex, if cultural and social distinctions between the sexes be denied?" In the first place, I nowhere say that cultural and social distinctions between the sexes are denied, but rather discuss these differences explicitly and in detail, and in the second place, Dr Thurnwald has neglected to consider one point which I extensively documented, that the authority of the husband and his ability to control even one wife depends upon a discrepancy in age between husband and wife (p. 81). The failure of marriages which lack this discrepancy in age and lack the long period of fostering care which emphasizes it, is more vivid evidence for the lack of differentiation between the approved social personalities of the two spouses, than is the absence of a highly complicated form of social organization, such as polyandry, evidence against it.

Dr Thurnwald has attempted to characterize the Arapesh as "introvert." It is hard to identify as introvert a people who are "more enthusiastic about the achievements of others and less inclined to initiate artistic and skilled occupations" (p. 145). As throughout his discussion, Dr Thurnwald interprets as the rule the cases which I cite as exceptions. So "the man interested in knowledge" whom Dr Thurnwald quotes to prove that the Arapesh are introvert, is described in the following sentence (p. 145).

There are certain types of individuals—the violent, the jealous, the ambitious, the possessive, the man who is interested in experience or knowledge or art for its own sake—for whom they have definitely no place.

The reviewer does equal violence to my meaning in other partial quotations. He quotes a series of recorded acts of unpatterned violence and temper as evidence against my opinion, which he quotes as being that "the violent person among the Arapesh cannot find . . . any expression of the internal drives." Dr Thurnwald has omitted under cover of dots these phrases, "either in the literature, or in the art, or in the ceremonial or in the history of his people" (p. 314).

In conclusion Dr Thurnwald discusses a number of "slips" in such a way as to bring my ethnological and linguistic accuracy rather seriously in question. It is surprising to find in a scientific review the sentences: "A number of slips occur in

⁵ Cf. Mead, *The Changing Culture of an Indian Tribe*, "Introduction," *Growing Up in New Guinea* (1930), Appendix, I: "The Ethnological Approach to Social Psychology."

book. The Mundugumor are described as 'cannibals and headhunters' (pp. 167, 211). *Ordinarily* the one excludes the other" (italics mine). Even the reviewer seems to have been uneasy about the old ethnological assumption which he uses as a major premise to prove a supposed inaccuracy. The Mundugumor were cannibals, i.e., they ate the entire body of their war captives with enjoyment and gusto and not for ritualistic reasons, and headhunters, i.e., they took the heads of their captives, ornamented them with clay and shells and preserved them as valuable trophies which were bequeathed from mother's brother to sister's son, as part of the elaborate reciprocities between the descendants of brother and sister. Dr Thurnwald then goes on to tax me with having ignorantly used the Pidgin English word *tamberan* under the mistaken assumption that it was an Arapesh word. On the page to which he refers (p. 63), I give not only the singular, *wareh*, of the Arapesh word for *tamberan*, but also the plural, *warehas*, and give considerable detail about the position of the word in the complicated Arapesh noun class system. I have used *tamberan* as a technical term, as shaman, *berdache*, etc., are used as technical terms. It does not seem unreasonable that a reviewer who criticizes a technical usage for a local area which is also his own, might have been expected to be familiar with my two previous discussions of (1) the fact that *tamberan* was a Pidgin English word with widespread use, (2) that I proposed to use it in carefully defined ways, and (3) that I was excluding it as applied to a ghost.⁶ Under these circumstances, the sentence: "It is, however, not Arapesh but common Pidgin English, and was probably used by Dr Mead's boy," is as unwarranted as is Dr Thurnwald's assumption that when (p. 63) I was talking about the *tamberan*, which was defined on the same page as "the supernatural patron of the grown men of the tribe," I was referring to a "ghost." It happens that in the Sepik-Aitape District from which my "boy" came, the ordinary Pidgin English for a ghost is not *tamberan* at all but *devil* belong man he die."

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NAMING PRACTISES IN ARIZONA

Certain parallels in naming practises between the Yuman tribes of southern Arizona and the Hopi of northern Arizona should be kept in mind in current reconstructions of Arizona history.

Matrilineally descended Hopi infants are named by their father's sister, the name being associated in some way with the women's clan (Each clan has a stock of personal names relating to the associations, sometimes misleadingly called totems, of the clan.) There is a trace of this clan practise at Acoma (White), but in no other pueblo. (Tanoan and Tewan like Hopi personal names are translatable but are without group associations, Zuñi and Keresan names are without group.

⁶ Cf. Margaret Mead, *The Marsalai Cult among the Arapesh* (Oceania, Vol. 4, 1933), p. 49; *Tamberans and Tumbuanas in New Guinea* (Natural History, Vol. 34, 1934), p. 245

i.e., clan associations and as a rule are untranslatable.) Compare Maricopa practise. Maricopa women (and some men) have names which refer to the characteristics of their clan totems; Maricopa clans are patrilineal (Spier). Actually, therefore, although not theoretically, Maricopa and Hopi get their personal names in identical ways; totemic or totemlike names from the father's clan.

After marriage Maricopa were called not by their personal name but by their clan name. Hopi married women may not be called by their personal name, and a woman leaving the maternal house may take for herself and her descendants another of the lineage names of the clan (rare practise). In general Hopi are frequently referred to by clan or lineage name with a term (*wungwa*) appended which indicates that an individual, not a group, is meant, and in English a person will be referred to as Bear man, Badger man, etc. There is a trace of this practise at Iaguna, but in no other pueblo as far as recorded.

For Hopi as for Yumans "the number of totems assignable to any clan is indefinitely large" (Spier). This prepossession appears to account for the plural naming of several Hopi clans quite apart from the above mentioned practise of renaming a split-off lineage.

The singular Hopi naming practises I have mentioned have puzzled Pueblo students for a long time, so parallels from a region not beyond the possibility of cultural influences may be welcome, if only to be held in suspense until larger complexes are determined. Giving "totemic" names from the father's clan and calling women (or men) by the clan name are widespread practises, which in one form and another are found from the Miwok to the Pima (Spier).

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AN OBSERVATION ON HOPI CHILD BURIAL

In *Hopi and Navajo Child Burials*¹ there is a statement, as follows: "If the burial was in a crevice the stick projected from the earthfilled crack." This statement is made in reference to child burial among the Hopi, and specifically at Mishongnovi. This would lead to the assumption that it was the practise when burying a child in a crevice to fill in such a crevice with earth, just as would be done in filling in a grave.

In speaking of "Hopi Death Customs" the Beagleholes² mention this custom of burial in a rock fissure only in connection with "a premature, still-born, or unnamed infant." This statement would lead to the assumption that a child of any age at all was not buried in a rock fissure but in the regular children's cemetery. Further there is here no reference to filling in the fissure.

The information upon which both these papers was based was presumably obtained very recently, and may be entirely correct for the present.

¹ D. Senter and F. Hawley, *American Anthropologist*, Vol. 39, pp. 131-34, 1937.

² E. and P. Beaglehole, *Hopi of the Second Mesa* (Memoirs, American Anthropological Association, No. 44, 1935), pp. 11-14.

The following incident which occurred in 1911 may, however, be of some interest.

In that year I spent over six months in the Hopi country, residing at Mishongnovi, and visiting all the other Hopi villages at intervals. I was collecting material and information for the Milwaukee Public Museum.

Like the above-mentioned authors, I also found information concerning death and mortuary customs the hardest to obtain. I had to depend almost solely upon observation for data.

However, when the school vacation arrived several children who had been away to school, returned and with them came a combination of measles and dysentery. The results were disastrous, especially among the younger children. A considerable number of adults also succumbed.

The reticence of the Hopi to discuss any phase of death, combined with their custom of so speedily disposing of the body, made observation very difficult indeed, but I did gather that it was the usual custom at that time to drop the bodies of younger children into certain special crevices along the edge of the mesa. Adults were buried in cemeteries, but so far as I could gather the infants were bundled and simply dropped into these special crevices, and this applied to children up to the age of several years. Try as I would, however, I was never able to be present at such an interment.

One day, some time later—it was after the epidemic had passed and toward the latter part of the summer—my interpreter, Luke Kwanieusiwa, and I were riding our horses over to Oraibi to attend a Tasaf Katsina and we made a detour to look over some reservoirs hewn into the top of the mesa, perhaps three-quarters of a mile west of Mishongnovi. At the time I had no opportunity to measure or closely observe these reservoirs or their drainage areas, so, a few days later I set out, alone and on foot, to return to them for a little closer observation.

I followed along the edge of the mesa, and at a point perhaps a quarter of a mile from the village my nostrils were assailed by that peculiar effluvium which is unmistakable and I soon came upon a crevice at the very edge of the mesa in which were a number, as I recall it now, not less than a couple of dozen, of these small burial bundles. The crevice was perhaps two feet wide and the topmost of the bundles was about fifteen feet below the surface of the mesa. Each was carefully wrapped, usually in cloth of native weave, and carefully bound. In size these bundles varied considerably, which indicated that there was a variation in age from earliest infancy up to at least several years. They lay in the crevice without any apparent orientation, just as if dropped from above without any attempt at definite placement.

There were not at that time visible any prayer sticks, food, or other offerings. That several of these bundles were the result of the recent epidemic was evident from the freshness of the wrappings on the uppermost, but that this crevice had long been used as a depository was also evident from the weathered appearance of some of those farther down. It would seem that, had offerings been placed here with the more recent bundles, some vestiges of these would have remained.

Obviously there had been no attempt to fill in this crevice with earth or to erect any sort of a marker or ladder for the spirits of the departed.

The crevice ran out to the precipitous edge of the mesa which dropped vertically for a great distance. It would have been physically impossible to have gained access to the level where these bundles were located without a ladder. Furthermore, the success of my work among the Hopi depended upon my keeping friendly relations, and it would have been the height of folly for me to have attempted to descend into this crevice, even if access had been easy.

Any activity on my part in the vicinity of this crevice would have been quickly noted by some sharp-eyed villager. I therefore tarried above the crevice only long enough to make the above observations, and I prudently made no inquiries concerning the place upon my return to the village. Had I done so the news would have quickly spread throughout the villages of all three mesas that I was aware of this burial spot. Besides that, I could not have found a soul who would have given me any information concerning it.

This observation, which was of necessity more or less fleeting, would seem to indicate that at that particular time at least, younger children as well as the infants were accorded this crevice burial, that no attempt was made to fill the crevice with earth, and that the same crevice was used over a considerable period, that apparently no spirit ladder was used, and that apparently offerings were not placed with these child burials.

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THE DUAL ORGANIZATIONS OF THE RAMKO'KAMEKRA (CANELLA) OF NORTHERN BRAZIL By CURT NIMUENDAJÚ AND ROBERT H. LOWIE¹ HABITAT AND AFFILIATIONS

THE Ramko'kamekra (Canella) occupy² the village of Ponto, situated in the steppes of Maranhão, Brazil, about 78 km. south of the town of Barra do Corda. Linguistically, they represent the southern dialect of the Eastern Timbira division of the Gê family. A simplified scheme of this stock is appended herewith, with the proviso that Snr Nimuendajú is not yet clear as to the affinities of the Eastern branch (E). In this stock linguistic, geographical, and ethnographic classification happen to coincide to a striking degree.

Gê Stock

A Northern and Western Gê

I Timbira branch

(a) Eastern Timbira

(1) Northern Dialect

Timbira of Rio Gurupy, *et al.*

(2) Southern Dialect

Ramko'kamekra, *et al.*

(b) Western Timbira

Apinaye'

¹ Under the auspices of the Institute of Social Sciences of the University of California, Snr Curt Nimuendajú of Belém do Para has during the last two years synthesized some of his previous observations (in German) and undertaken two supplementary expeditions to Gê tribes in the interior of Brazil. The present paper presents primarily data bearing on dual organizations; his extremely ample material on ceremonialism has been rigidly excluded except when too intimately interwoven with social structure to permit segregation. Even so it is not easy to summarize clearly the characteristics of a society simultaneously recognizing four distinct principles of dichotomy, two being of tribal scope, the other two limited to males. The data here presented supersede Nimuendajú's brief notes in *Anthropos*, Vol. 24, pp. 670-72, 1929 and E. H. Snethlage's relevant statements in his article *Unter nordostbrasilianischen Indiern* (*Zeitschrift für Ethnologie*, Vol. 62, pp. 111-205, 1931) —R. H. L.

² The epidemic of 1935 and the death of the oldest chief led to disorganization and a schism into two separate hamlets, but in 1936 Snr Nimuendajú effected a reconciliation and a joint celebration of the Tep-Yarkwa' festival in a new village —R. H. L.

- II Northern Kayapo'
- III Southern Kayapo'
- IV Suyá'
- B Central Gê
 - I Akwe
 - Šere'nte
 - Šavante
 - Šakriaba'
 - II Akroa'
- C Southern Gê (Kaingang-Ingain)
 - I Kaingang
 - (a) Coroado-Kaingang
 - (b) Guayanã of Faxina
 - (c) Botocudo of Santa Catharina
 - II Ingain
- D Jaico'
- E Kamakan, *et al.*

THE VILLAGE

Several years ago Ponto had a population of 298, housed in thirty huts ranged in the circumference of a circle. Directly within the circumference there runs a broad concentric street, while the middle of the settlement is marked off as a circular plaza with which each hut is connected by a radial path. Thus, Ponto as a whole represents the appearance of a spoked wheel. Away from the village there are roads about 15 m. wide, which lead into the steppe toward the cardinal directions, the northern road being 17 km. in length. These roads figure prominently in the relay races, which play a conspicuous part among native pastimes.

The present huts are rectangular gabled structures, which the Indians assert represent their ancient type. This is questionable, for in 1814 Major Francisco de Paula Ribeiro noted only small round huts. Even today the women put up hemispherical, semi-ovoid, occasionally conical structures of three meters diameter—approximately the maximum size consonant with the material and technique. On the other hand, some ampler type must have antedated these small huts since the several social and ceremonial groups, one of which still numbered 57 in 1933, require larger domiciles for their meetings. Possibly Ribeiro did not visit the Ramko kamekra during the rainy season.

The women own the houses, and residence is definitely matrilineal. Adult men, however, continue to frequent their maternal homes, where they enjoy greater prestige than the husbands of their kinswomen; and a widower or divorced man naturally returns to his old home. A couple may, indeed,

occupy a separate hut, but in that case it is immediately adjacent to the wife's mother's.

The resulting extended family protects its members, but plays a minor part since it lacks either ceremonial or economic functions. Not the matrilineal household, but each married woman figures as the owner of a plot for

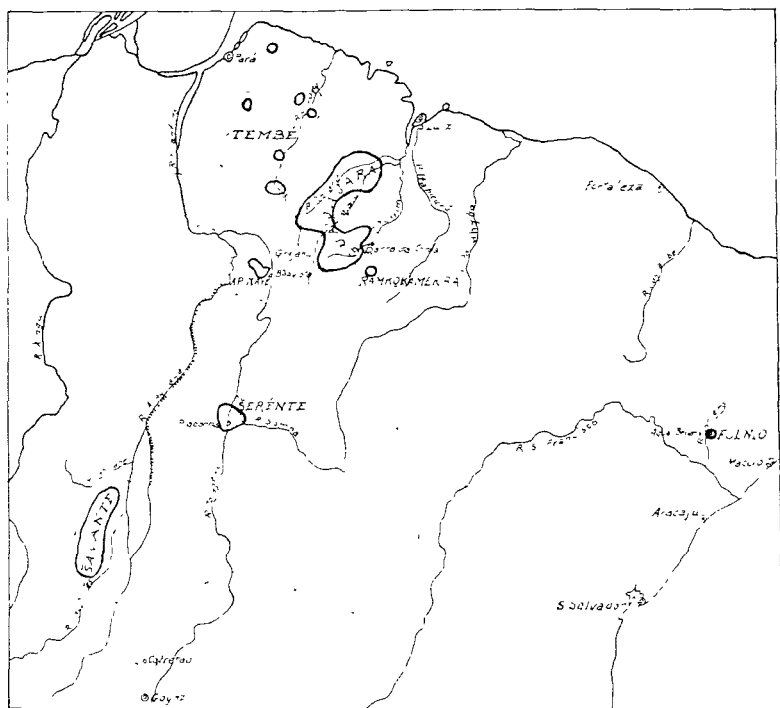


FIG. 1. Map of northeastern Brazil, showing the location of the Ramko'kamekra and other tribes. The Fulnio speak an isolated language; the Tembe' and Guajaja'ra are of the Tupi stock.

cultivation. In this context it is well to correct the picture of all Gê as pure nomads. The Ramko'kamekra, while mainly dependent on hunting and gathering, also did some farming. Indeed, they grew a plant of the genus *Cissus*, unknown to the neighboring Tupi' and the neo-Brazilians.

EXOGAMOUS MOIETIES

There are two distinct principles of dichotomy that affect the entire tribe, yielding the exogamous and the Rainy Season moieties, respectively.

The tribe is divided into two matrilineal, non-totemic, theoretically exogamous moieties of equal rank, Ko'i-kateye (ko'i, east) and Hara'-kateye (hara', west), the latter occupying a somewhat larger part of the village circle. Moiety emblems are lacking. Apart from the regulation of

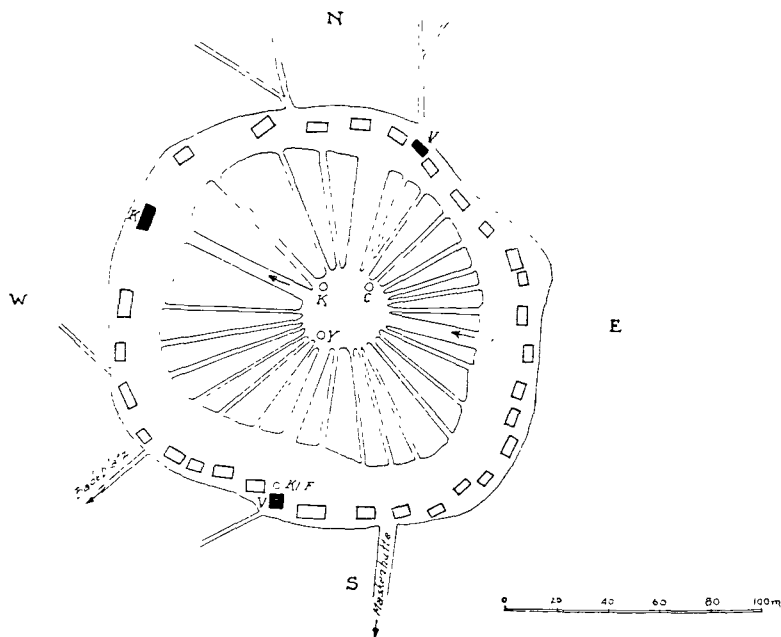


FIG. 2 Ramko'kamekra village at time of Kokrit (Mummers') ceremony. Note the circular plan of the village, the ring-shaped thoroughfare left in front of the houses, the radial lanes connecting this boulevard with the central plaza. For the convenience of the Kokrit the lanes leading from their assembly hut (K) in the west of the plaza and thence to a spot for assemblage diametrically opposite their hut have been specially widened. Note the diametrically opposite location of the two Vu'te' houses (V), that of the northeastern sector housing the Agouti society, that of the southwestern sector the Jaguar society, two organizations always collaborating in the performance with the Mummers. The south pointing arrow indicates the direction of the shed two km away in which the masks are made. In the center the circles marked K, C, Y indicate the plaza sites proper to the Mummers, Agouti, and Jaguar societies, respectively. KLF represents the hut of the actor called the Little Falcon.

marriage, the moieties are of little significance. The exogamous rule is rapidly breaking down, notwithstanding the disapproval of the old guard.

Contrary to Snethlage's findings, the moieties have no separate chiefs, nor are they subdivided into age-classes, which embrace members of both moieties.

RAINY SEASON MOIETIES

The second tribal dichotomy holds seasonally. The Ramko'kamekra divide the year into two parts, Vu'te' and Meipimra'k, which roughly coincide with the dry and the rainy season, respectively. During the latter the village is bisected into the Ka' and the Atu'k, designations used with distinct masculine and feminine suffixes. These units may be called "Rainy Season moieties." They are not coterminous with the exogamous moieties; affiliation hinges on one's set of personal names, which automatically determines membership.

A boy acquires his set of names, one after another, from matrilineal kinsmen; a girl gets hers from patrilineal kinswomen. As a rule the transfer is from one generation to the immediately adjacent lower generation, but it is not unusual to skip a generation or two. On the other hand, there are occasional transfers within a generation if the individuals concerned are separated by a considerable difference of age. The Ramko'kamekra themselves regard as ideal the conveyance of names from a maternal uncle to a sister's son, and from a paternal aunt to a brother's daughter. Actually, of twenty-eight cases genealogically examined only five conformed to this norm. This must be correlated with a principle of reciprocity that asserts itself in any transfer: if a man lacks a daughter to whom his sister could transmit her name series, she does not permit her brother to pass his set on to her son, but will cast about for a more remote kinsman who has a daughter. Thus, Waka'i had a sister, Pieka'ra, but no daughter, hence did not transfer his names to his nephew.

In this conveyance of name sets, siblings by adoption are reckoned equivalent to blood-siblings. Further, the notion of co-paternity extends the range of donors and recipients. If a pregnant woman has extra-marital relations with a man, the adulterer and her lawful husband are both subject to the couvade and both figure as fathers of the child. Thus, on Patkwei's birth her mother, Kopkre, confessed having had intercourse with one Hukraino. Consequently Patkwei became the sister of Hukraino's legitimate son Kroyamri, to whose daughter she gave her own name, while Kroyamri conferred his name on Patkwei's son.

Normally, a person transfers his or her names to a single recipient. But if the donor should die before completing the conveyance of the entire set, he is succeeded by a second transferrer. This involves the possibility of changing one's membership in the Rainy Season units. One girl acquired her paternal aunt's name Kentapi, thereby becoming a member of the Ka half. When the aunt died, another patrilineal kinswoman gave the niece *her*

names, which happened to be *Atu'k* and thus automatically transferred the girl to the *Atu'k* half. The potential impermanence of individual affiliation with these groups thus contrasts with the hereditary membership in the exogamous moieties. An individual who owns names pertaining to distinct sets is careful not to mix series in conveyance.

Not only the *Ramko'kamekra* and their name series are apportioned to the Rainy Season moieties, but all of nature is antithetically divided between them, as indicated by the following partial scheme:

<i>Ka</i>	<i>Atu'k</i>
East	West
Sun	Moon
Day	Night
Dry Season	Rainy Season
Fire	Firewood
Earth	Water
Red	Black
Red plants and animals	Black plants and animals
Maize	Sweet Potato
Manioc	Cucurbit ³

Each Rainy Season moiety has a headman appointed by the chiefs and elders; and it is this headman's maternal home that serves as a place of assembly for his unit. In consonance with the above scheme the *Ka* use red paint for body decoration, the *Atu'k* black paint. Each unit, moreover, has its own slogan: on the birth of a boy, e.g., his maternal uncle or mother's mother's brother—the prospective name-transferrer—steps in front of the door and by uttering the war-cry "wa-wa-wa!" or "ke-ke-ke!" announces to the village that a new *Ka* or *Atu'k*, respectively, has seen the light of day.

These two complementary halves figure predominantly during the rainy season, and principally in two ways—in racing and hunting. The teams pitted against each other in relay races during the season are recruited each from the *Ka* or *Atu'k* moiety, respectively. In these competitions each side carries a log, which is appropriately marked with red or black pigment. The two groups also go on joint hunting trips, accompanied by their female members. Continence is observed during these expeditions, but on their return there is an exchange of women.

After the beginning of the maize harvest, i.e., at the very close of the

³ Snr Nímuendajú's manuscript has "Kurbis "

Meipimra'k period, the men of each moiety assemble in the plaza, each accompanied by a sister's son. The Atu'k lay down samples of the crop before the Ka, the last donor contemptuously throwing down an armful of cobs. Each moiety boasts of the number and appearance of their sisters' sons, simultaneously casting ridicule on their opponents' nephews.

During the dry season—that of ceremonialism par excellence—the Rainy Season moieties play a very subordinate role. In the Pepye, one of the two initiation rituals, the Atu'k novices go to their individual seclusion huts at night, the Ka to theirs in the daytime, in accordance with the dichotomous scheme. Also on certain occasions the distinctive colors serve as badges of membership.

Nimuendajú suggests that the Rainy Season groups originally coincided with the exogamous moieties since other tribes, who also dichotomize nature, assign each half of the universe to one of their *exogamous* moieties, as Nimuendajú himself discovered in 1912-1913 among the Kaingang of the Ivahy region, State of Paraná.

PLAZA MOIETIES

Independently of both the exogamous and the Rainy Season bisection of the entire tribe, there is a dichotomy restricted to males,⁴ who are ranged into the Koi'-rumenkăca and the Hara'-rumenkăca. These appellations again, as in the case of the exogamous groups, refer to East and West. Each of these dual groups embraces three subdivisions, as shown below:

<i>East</i>	<i>West</i>
1. Haka (Giant Snake)	4. Auçet (Armadillo)
2. Čepre (Bat)	5. Ke'dre (Dwarf Parrot)
3. Čodn (Carrion Vulture)	6. Kupe' (Alien Tribe)

Membership in these groups, too, hinges on the acquisition of certain personal names. Just as every name, irrespective of sex, belongs to either the Ka or the Atu'k Rainy Season moiety, so every masculine name belongs to one of the six Plaza subdivisions. Further, each of these Plaza names determines membership in two clubs, either in the Falcon and Jaguar (or Kokrit = Mummers') societies or in the Duck and Agouti societies. On the other hand, affiliation with two other organizations, the Clowns (Me'ken) and the King Vultures (Tamhak) is quite independent of the personal names.

⁴ The female auxiliaries at the Pepye initiation are *ex officio* brought into the Plaza organization, being always assigned to the Bat and Dwarf Parrot subdivisions.

While there is no evidence of transference of Ka and Atu'k names by the same donor, in three instances the same person conveyed the names of distinct Plaza groups—in one case even of distinct Plaza moieties—to the same recipient.

Especially significant is the experience of Hiino and his uncle Yua-Hiino, both members of (a) the Bat division of the Eastern Plaza moiety; (b) the Atu'k Rainy Season moiety; and (c) the Falcon and Mummers' club. When Hiino was to be secluded for his Pepye initiation, it was his Plaza group's duty to prepare his seclusion hut, seclusion yard, and a recess, but they shirked all but the first of these obligations. Yua was offended and left the Bats, joining the Armadillo subdivision. Henceforth the names Yua and Hiino were no longer the property of the Bats but were added to the Armadillo series. However, the two men remained Atu'k, Falcons, and Mummers.

Each Plaza group takes up a definite position in the village plaza and has an assembly house, whose location on the periphery of the village circle roughly corresponds to the position of the subdivision in the plaza.

The functions of the Plaza groups are ceremonial and largely relate to the two initiation phases, Ketuaye and Pepye. Thus, in the Ketuaye initiation all offices are dual, one officer of each pair being recruited from the Eastern, the other from the Western Plaza group. Only in the case of the girl auxiliaries (*mekuičwe'i*) this exclusively masculine dualism is inapplicable. The Plaza grouping is very conspicuous in the Pepye initiation, where each of the six subdivisions uses a distinctive pattern in body painting. Apart from these celebrations the Plaza groups figure in the Tep-Yarkwa', one of the major festivals intercalated between the two phases of initiation. In the log-races characteristic of this ceremonial period the Plaza halves are regularly pitted against each other.

The present Plaza divisions may be survivals of earlier clan divisions of the exogamous moieties, once localized within the village circle. These clans possibly lost their genealogical character, retaining ceremonial functions. As analogous may be cited the ceremonially functioning localized clans of the Akwe-Šerente and the Bororo.

AGE-CLASS MOIETIES

Finally, there is another dual division affecting only males, viz. the two pairs of age-classes that engage in competitive sport during the dry (*Vu'te'*) season. Once more there is a correlation with East and West—in fact, the pairs of active classes bear exactly the same designations as the two exog-

amous moieties, Ko'i-kateye and Hara'-kateye. Notwithstanding this nomenclature, the two forms of dual grouping do not coincide at all, since each of the age-classes comprises members of both moieties.

The total number of age-classes, inactive and active in sport, is indefinite. The system is tied up with the initiation ceremonies, which thus require brief mention. All males undergo two phases of initiation, each being repeated at intervals of two or three years, so that the complete cycle occupies about ten years. All those jointly initiated form a fixed class for the rest of their lives, so that apart from the youngest boys the whole male population is grouped into age strata. Even the as yet uninitiated boys who look forward to initiation at the next celebration form an unofficial age-class and mimic the activities of the young men.

The active classes have each a definite position in the plaza. This, however, shifts whenever the unofficial boy's group receives formal admission to the plaza, which automatically promotes the oldest active class to the athletically inactive status of councilors. Its members move to the very centre of the plaza, continuing, however, to preserve their identity with reference to any surviving predecessors. Thus, recently there were three councilor classes of eight, four, and two men, respectively, each occupying a distinctive position in the plaza centre and representing the "graduates" of 1903, 1893, and 1883.

To illustrate the scheme by one example, in 1913 the class D in the northwest corner of the plaza completed its initiation cycle. This signaled the admission to the plaza of the hitherto unofficial youngsters, E, who assumed a place opposite D, i.e., in the northeast, thereby crowding out class C, which moved south, ousting A, the oldest of the active classes, which passed into the council, leaving the normal set of four competitive groups in possession. In 1923 the novices entered on the northwest, displacing the class located there, which moved south, thus initiating corresponding transpositions throughout the plaza. Successive entering classes thus regularly alternate from northeast to northwest, and vice versa, when joining the plaza.

KINSHIP TERMS

The following list is rearranged from Nimuendajú's original data, as subsequently revised by himself (letter of Dec. 20, 1935). The prefixes *id-* and *i-* are possessives of the first person singular, the suffix *-re* is diminutive, *-ti* augmentative, *-i* feminine, *-tum* denotes old age, *-ndu'wu* youth; *-ye* remains problematical but occurs with nouns denoting social groups. Vocative forms are lacking.

*Blood Relatives⁵**Ascending Generations*

i-nču	my father, father's brother, father's sister's son, [mother's sister's husband]
i-nče'	my mother, mother's sister
ke'de-ti	mother's brother, mother's father, mother's sister's son older than ego ⁶
ke'de-re	father's father
tu'i-re	father's sister, father's sister's daughter, father's mother, mother's mother [father's brother's wife], mother's sister's daughter

Ego's Generation

i-ha	older sibling, older child of father's brother
i-to ⁷	older sibling
nyo'he'u-re	younger sibling, younger child of father's brother (For father's sister's and mother's sister's children, see under Ascending Generations; for mother's brother's children, see under Descending Generations.)

Descending Generations

i-kra	my son, my daughter, my sister's child (w. sp.) [my wife's sister's child], my mother's brother's child, my brother's child (m. sp.)
i-tam-čwe'	my sister's son (m. sp.), my daughter's son (m. sp., w. sp.), my mother's sister's son younger than myself, ⁸ my brother's son (w. sp.), my son's son (m. sp., w. sp.), my mother's brother's son (w. sp.)
i-tam-čwe'-i	my sister's daughter (m. sp.), my daughter's daughter (m. sp., w. sp.), my mother's sister's daughter younger than myself, ⁹ my brother's daughter (w. sp.), my son's daughter (m. sp., w. sp.)
i-yapa'la ⁹	synonym of i-tam-čwe'
i-yapal-čwe'-i	my brother's daughter (w. sp.)

Affinal Relatives

i-piye'	my husband
i-pro	my wife
pai-ke't	wife's father
i-mpaye'	my wife's brother, my wife's sister, my wife's brother's child

⁵ Extensions to affinal relatives are bracketed.⁶ This last meaning probable, not certain.⁷ It remains uncertain how far this term is synonymous with i-ha.⁸ See footnote 6⁹ Invariably so given for all meanings; the feminine form below, on the other hand, occurs only once, as indicated; perhaps through an oversight.

kra'-tumye	husband's father
propeke'i	husband's mother
i-wawe'	my son-in-law (m. sp., w. sp.), my sister's husband (m. sp., w. sp.)
i-piyoye'	my father's sister's husband, my son-in-law (m. sp., w. sp.), my sister's husband (m. sp., w. sp.) ¹⁰
i-čwe'i-ye	my son's wife (w. sp.), my brother's wife (w. sp.)
ha-čwei-ye	my son's wife (m. sp.), my wife's mother
tokti'i-ye	husband's sister.



FIG. 3. Temporary round hut of the Ramko'kamekra.

Although the terms for husband's brother and for brother's wife (m. sp.) are not recorded, the vocabulary for affinities is evidently ample. The discrimination between wife and wife's sister is consistent with the absence of the sororate.

On the other hand, the father's brother is designated by the same term as the father, and correlatively he addresses his brother's children as his own. Also the mother's sister's husband figures as a "father" and groups his wife's sister's children with his own. In short, normal "bifurcate merging" features are not lacking.

Two characteristics of the system are noteworthy—the inclusion of the

¹⁰ Evidently largely synonymous with i-wawe'.

father's sister's son under the "father" term, and the classification of the father's sister's daughter with the father's sister. The additional meanings of these terms fail to obscure these identifications, which perhaps for the first time establish a South American occurrence of these Crow-Choctaw-Hopi features. Interestingly enough, the patrilineal Šerente were found to have the Omaha feature of merging the maternal uncle's son with the maternal uncle.

However, the list in its present form implies certain logical discrepancies, such as the identification of the daughters of a maternal with those of a paternal aunt—notwithstanding the equation of the maternal aunt with the mother. It is further clear that classification largely ignores moiety lines; e.g., the mother's brother and the mother's father are grouped under one head, as are the mother's mother and the father's mother.

COMPARATIVE NOTES

Twenty years ago Spinden wrote that "no clear case of kinship clans has been reported south of the area of the United States."¹¹ While this proposition may still hold for the area he primarily discussed, certainly it does not do justice to South American data, to which perhaps it was not meant to apply. Admittedly much remains obscure as to the social organizations of the continent. That the Peruvian *ayllu* was an exogamous patrilineal clan seems highly probable; Nordenskiöld found "Sippen" among both the modern Quechua and the Aymará.¹² That these units were once matrilineal has indeed been alleged by some authors, but apparently without definite proof, a statement that seems to hold for the Araucanian equivalents. Olson's citations for Colombia and Ecuador indicate some measure of nepotic succession and inheritance, but no definite proof of exogamous clans.

When, however, we turn to the trans-Andean region, a whole series of unequivocal cases stand forth. The Ona still lack clans of the accepted type, but their emphatically exogamous, patrilocal, and patrilineal bands emphasize the unilateral principle as strongly as the Australian hordes, like these latter units, they turn out to be wholly consistent with the

¹¹ H. J. Spinden, *Ancient Civilizations of Mexico and Central America* (New York, 1917), p. 186.

¹² Erland Nordenskiöld *Forschungen und Abenteuer in Sudamerika* (Stuttgart, 1924), p. 76; Ronald L. Olson, *Clan and Moiety in Native America* (University of California Publications in American Archaeology and Ethnology, Vol. 33, 1933), p. 375 s₁; W. Schmidt and W. Koppers, *Völker und Kulturen* (Regensburg, 1924), p. 121.

economic and social importance of the bilateral family.¹³ Further north, the Witoto-Bora of the upper Amazon and the Arawakan Palikur have patrilineal clans, those of the latter tribe being ranged in an "upper" and a "lower" moiety. The Mundurucu have patrilineal exogamous moieties subdivided into clans; while Nimuendajú's earlier researches establish such groups among the Parintintin, Tukuna, Kaingang, and Šerente, those of the Šerente and Tukuna embracing several clans each. Less decisively C. Estevao reports indications of patrilineal moieties from the Tembe' of the Rio Acará.¹⁴

Matrilineal exogamous moieties, subdivided into totemic clans, have been established beyond cavil for the Eastern Bororo of the Rio das Garças, a tributary of the Araguaya; and a more recent investigation of the Bororo of the Rio Vermelho, an affluent of the upper Sao Lourenço, bears out the earlier account. The Canella facts described in the present paper demonstrate a second unexceptionable Brazilian instance of matrilineal exogamous moieties. Farther north, Kirchhoff's critical sifting of the evidence leaves the Goajiro and the Coastal Arawak (Lokono) with an indefinite number of matrilineal clans.¹⁵

Whatever, then, may have been the facts among the more civilized pre-Columbian peoples, in trans-Andean South America a considerable number of tribes, representing such distinct linguistic stocks as the Bororo, the Gê, the Arawak, and the Tupi-Guarani, definitely recognize unilateral exogamous groups, some with matrilineal, others with patrilineal descent.

Of the exogamous moieties listed above, some are subdivided into clans, others are undivided, though the Plaza groups of the Canella conceivably evolved out of earlier clans. At the present stage of knowledge it is more profitable to insist on the frequency of nonexogamous moieties. It may well be that some of them have merely lost the exogamous feature, but that can never be assumed without some definite evidence. In the southeastern United States, e.g., the Choctaw were the only tribe in which these units

¹³ Martin Gusinde, *Die Selk'nam: vom Leben und Denken eines Jagervolkes auf der Grossen Feuerinsel* (1931), pp. 302, 319, 419, 425.

¹⁴ Curt Nimuendajú, *Die Palikur-Indianer und ihre Nachbarn* (Goteborg, 1926), pp. 22, 86, 132; *id.*, personal communication; Paul Kirchhoff, *Die Verwandtschaftsorganisation der Urwaldstämme Südamerikas* (Zeitschrift für Ethnologie, Vol. 63, 1931), p. 175; Albert Kruse, *Mundurucu Moieties* (Primitive Man, Vol. 7, pp. 51-57, 1934).

¹⁵ D. Antonio Colbacchini, *I Bororos Orientali "Orarimugudoge" del Matto Grosso (Brasile)* (Turin, no date [1925?]), pp. 1-30; C. Lévi-Strauss, *Contribution à l'étude de l'organisation sociale des Indiens Bororo* (Journal, Société des Américanistes de Paris, Vol. 28, pp. 269-304, 1936), Kirchhoff, *op cit.*, pp. 147, 151.

regulated marriage; and the moieties of the Eastern Pueblos are emphatically nonexogamous.¹⁶ Turning to South America, we have among the Canella three distinct forms of nonexogamous bisection over and above the exogamous moieties; while Colbacchini and Lévi-Strauss have revealed a secondary dual division, resulting in an Upper and Lower (*superiori* and *inferiori*) or an Up-stream and a Down-stream half of the village (*de l'amont* and *de l'aval*). The Uro of Bolivia retain a similar division into a "section d'en haut" and a "section d'en bas;" the Čipaya have nonexogamous moieties, which Métraux equates with the Aymará units identified by Bandelier as Upper and Lower and assigned to definite geographical halves (North and South) of a settlement. Taking these data with those cited by Olson from the Cañaris of Ecuador and Nimuendajú's Palikur observations, we may infer that over a wide area, including much of Brazil, there were moieties, often not exogamous or not demonstrably so, that were associated either in terminology or actually with a definite geographical location. The arrangement of the Bororo village with one exogamous moiety on the north and the other on the south of a central square recalls Bandelier's description of Tiahuanaco.¹⁷

As regards the concepts associated with the moieties, some discrimination is obviously indicated in drawing historical inferences, but the recurrence of the Upper-Lower antithesis can hardly be set down to sheer coincidence. On the other hand, the Weak and the Strong moiety of the Bororo of the Rio das Garças suggest that the contrast felt between two complementary social groups can be adequately expressed by any number of naturally contrasted ideas. That such labels need not refer to moieties is instructively brought out by the way the Bororo distinguish between related clans within the same moiety: their Strong moiety includes a Red and a Black Caterpillar clan, a Red as well as a Black Burity Palm clan.

On the axiom that "unilateral descent" is an ethnological concept, not an ethnographic reality, we must—unless there is strong evidence to the contrary—consider matrilineal systems apart from patrilineal ones. Owing

¹⁶ J. R. Swanton, *Aboriginal Culture of the Southeast* (Forty-second Annual Report, Bureau of American Ethnology, 1928), p. 695, *id.*, *Source Material for the Social and Ceremonial Life of the Choctaw Indians* (Bulletin, Bureau of American Ethnology, No. 103, 1931), p. 76; E. C. Parsons, *Tewa Kin, Clan, and Moiety* (American Anthropologist, Vol. 26, p. 333 sq., 1924).

¹⁷ Colbacchini, *op. cit.*, p. 8, Lévi-Strauss, *op. cit.*, p. 271; A. Métraux, *Contribution à l'ethnographie et la linguistique des Indiens Uro d'Ancoquí, Bolívie* (Journal, Société des Américanistes, Vol. 27, 1935), p. 87; *id.*, *L'organisation sociale et les survivances religieuses des Indiens Uru-Čipaya de Carangas, Bolívie* (Twenty-fifth International Congress of Americanists, pp. 191-96, 1932), Olson, *op. cit.*, pp. 373, 377, 402.

to the sparseness of Goajiro and Lokono material, we must therefore restrict detailed comparison to the Canella and the Bororo. Certain striking similarities are undeniable. Both share (a) matrilineal exogamous moieties; (b) linkage of the moieties with contrasted cardinal directions; (c) circular arrangement of a settlement; (d) radial streets from the peripheral huts to a ceremonial center; (e) matrilocal residence. The Bororo men's club-house and bachelors' dormitory, so graphically described by von den Steinen¹⁸ and similarly pictured by his successors, has no formal parallel among the Canella, but functionally their plaza is a rough counterpart. On the other hand, the Canella age-classes, while distinctive and (so far as I know) unique in South America, bear names suggestive of the totemic clans of the Bororo. The kinship nomenclatures present hardly any noteworthy similarities, though it must be remembered that the information on the Bororo system remains scanty. The Bororo stress seniority within a generation to a greater extent; e.g., only the father's *elder* sister is equated with the grandmother. Also the Bororo dependence on descriptive technique in designating, say, a paternal uncle as "my father his elder brother" has no Canella parallel.

Among the Canella repeated inquiry has failed to establish forms of preferential marriage. A member of the related Apinayé (Western Timbira) tribe declared he would be ashamed to marry a deceased wife's sister. On the other hand, both our recent authorities on the Bororo note simultaneous marriage with a woman and her daughter by a previous marriage; and Colbacchini adds instances of sororal bigamy. From the nomenclatorial identification of the father's mother with the father's elder sister and a man's mother-in-law Lévi-Strauss infers marriage with a father's younger sister; and the identification of father-in-law and father's father leads him to infer marriage with the daughter of a father's elder sister. He admits lack of evidence for the actuality of such unions.¹⁹

Contrastive perspective throws the Bororo and Canella systems into relief as against other South American social structures. They reveal a basic unity, but at the same time so many differences that we must allow a considerable period for their respective individualization.

From a broader point of view the South American data suggest a revision of traditional Americanist views. It has been customary to correlate matrilineal systems with a higher plane, specifically with intensive farming;

¹⁸ Karl von den Steinen, *Unter den Naturvölkern Zentral-Brasiliens* (2nd ed., Berlin 1897), p. 367 f.

¹⁹ Colbacchini, *op. cit.*, pp. 18, 27; Lévi-Strauss, *op. cit.*, p. 282 f.

the aberrant Northwestern instances could be explained away as at least coupled with a sedentary mode of life, the Crow may have become hunters through loss of the maize complex once shared with their Hidatsa congeners. But the Bororo-Canella organization cannot be thus disposed of. As recently as von den Steinen's visit, the Bororo of the lower Sao Lourenço did not farm at all. The women would dig up governmentally planted manioc tubers precisely as they would wild roots; and even on the headwaters of the stream they had not learned to grow any plant species for food, contenting themselves with tobacco, cotton, and a variety of gourd.²⁰ The Canella have evidently been for some time further advanced economically, but even with them hunting and gathering clearly predominated over farming. Nothing in the scanty material on the multiple clans of the Goajiro and Lokono even faintly suggests the moiety structure of the two tribes under discussion; while their fellow-Brazilian aborigines with dual organizations follow patrilineal descent. As for the majority of the Tropical Forest tribes sometimes cited to illustrate matrilineal institutions, many of them actually practise matrilocal residence, but Kirchhoff's scrutiny of early sources proves the general insufficiency of evidence for a definite rule of descent. As for matrilineal reckoning, it is barred by the explicit orthodoxy of a man's marriage with his sister's daughter among the Tamanak and Macusi, both of Carib stock; the same rule applies to the coastal Tupi.²¹

In short, on the axiom stated, the Bororo-Canella social system may be conceived as an independent local growth until specific evidence proves otherwise. If so, full-fledged matrilineal institutions are consistent with a hunting or at most an incipiently horticultural condition—with obvious chronological implications.

Granted that matrilineal organizations may arise independently, the scientific ethnologist must determine which phenomena are significantly correlated in the known matrilineal complexes. We must reject as too vague the idea that maternal clans will arise now and then on the doctrine of chances; and Dr Eggan does not mend matters when in an otherwise highly meritorious essay he prescribes the rule that descent and marriage arrangements, instead of being merely correlated, must be considered "functions of some factor or principle which they have in common."²² The idea would be excellent if that common principle were stated; without such formulation

²⁰ Von den Steinen, *op. cit.*, pp. 342, 369.

²¹ Kirchhoff *op. cit.*, pp. 98 f., 102, 117, 185.

²² Fred Eggan, *Historical Changes in the Choctaw Kinship System* (American Anthropologist, Vol. 39, 1937), p. 51.

the proposition is pretentious and meaningless. Is it necessary to point out that a functional relationship between two phenomena neither implies a one hundred percent correlation nor excludes other functional relations?

The following remarks are merely meant to foreshadow promising inquiries. Canella society in some respects recalls the familiar Hopi situation: matrilineal descent, matrilocal residence, feminine house ownership, and an obtrusive avuncular relationship occur in conjunction among both tribes. Further, both have two distinctive traits of kinship terminology which are *not* common to all matrilineal peoples, yet are probably definitely more frequent with matrilineal than with patrilineal reckoning and logically more congruous with it. These two features are the identification of father's sister's son with father, and of father's sister's daughter with father's sister. I consider it very interesting that on these points the Canella are nearer to the Hopi and Crow than to the geographically closer Bororo. On the other hand, the Bororo share with the Crow the not altogether frequent phenomenon of a single term for the elder brother and the maternal uncle, though apparently only for a woman speaking.

Turning now to so remote a people as the Trobrianders, the Canella-Bororo complex reappears only in part: men are the house-owners, residence is patrilocal; but the avunculate is tremendously strong and the clans are matrilineal. As for kinship terms, these Melanesians resemble the Canella in both of the significant Crow-Hopi features. But in another direction Trobriand usage approaches that inferred for the Bororo by Lévi-Strauss and actually observed on the coast of British Columbia: a man quite properly mates with his paternal aunt and either mates with or marries this aunt's daughter. The Tlingit favor marriage with a father's sister, for whom her daughter may be substituted. The Trobrianders reveal other similarities with the Northwest Coast of America. Precisely as among the Melanesians, a Haida boy leaves his parents to live with a maternal uncle, who assumes charge of his education and acts as his protector; and succession is nepotic (or fraternal). As for widow-inheritance, the Haida—like the matrilineal Banks Islanders—pass on a maternal uncle's wife to his nephew. And, most remarkable of all, there is in northwestern America the same typical rivalry of avuncular and paternal attitudes that Malinowski describes for his people. The Haida father tries to advance his son's social status, and this is "the dominant incentive to industry and thrift." Tsimshian tales reveal antagonism between uncle and nephew, legal relations devoid of sentiment—in striking contrast to the bond between father and child.²³

²³ B. Malinowski, *The Sexual Life of Savages in North-western Melanesia* (New York, 1929) pp. 515 f., 534 f.; T. M. Durlach, *The Relationship Systems of the Tlingit, Haida and Tsimshian*

No adequate account of Australian matrilineal institutions is available, but we may use African data as a check. Among the Vachokue the maternal uncle assumes control of his nephews when they are about six or seven years of age, they live with him and inherit his possessions and office; the oldest son in a household bears his maternal uncle's name. Residence, however, is patrilocal, and specifically Crow features of terminology are lacking.²⁴

Obviously, there is no one hundred percent correlation between any two of the features entering the Canella-Bororo complex. Matrilineal residence flourishes in many South American communities without any definite rule of descent; in the Trobriands men own the houses; the Iroquois lack the terminological equation of the paternal aunt's son with the father; and so forth. Nevertheless, a survey of matrilineal peoples strongly suggests an organic nexus of certain elements of the matrilineal complex. Why does Canella society suggest that of the Hopi rather than that of the near-by Mundurucu? Whence the haunting resemblances between Trobriand and Tsimshian family attitudes? Whence the Vachokue, British Columbian, Trobriand parallel as to residence in an uncle's home? Why, of all South American tribes, should the matrilineal Canella turn up with the Crow kinship equations? Why do inheritance rules at once change when we turn from the matrilineal Vachokue to their patrilineal neighbors?

It is our task to analyze these phenomena and establish which of them really belong together in the only scientific, i.e. the mathematical sense of the term "functional relationship." Long ago Bachofen alleged a mystic bond between the rule of descent and a matriarchate, a preference for the left hand, and what not. He erred, not in assuming that a rule of descent has correlates, but in substituting for an empirical investigation a fantastic a priori scheme. Our present duty is to prune the extravagances and retain the sound core. A series of detailed comparisons of social variants within major but practically controllable areas is indicated.

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(Publications, American Ethnological Society, Vol 11, 1928), p 65, G. P. Murdock, *Kinship and Social Behavior among the Haida* (American Anthropologist, Vol 36, 1934), pp 358 f, 361; W. H. R. Rivers, *The History of Melanesian Society* (Cambridge, 1914), Vol 1, p 48; F. Boas, *Tsimshian Mythology* (Bulletin, Bureau of American Ethnology, No 31, 1916), p 425 f.

²⁴ Hermann Baumann, *Lunda: Bei Bauern und Jägern in Inner-Angola* (Berlin, 1935), pp. 16, 89, 124 sq., 130.

BLOOD GROUP DETERMINATIONS OF PRE-HISTORIC AMERICAN INDIANS

By LELAND C. WYMAN AND WILLIAM C. BOYD

INTRODUCTION

THE development of a method¹ for determining the presence of the agglutinogens A and B (Landsteiner blood groups) in desiccated tissues from human remains, centuries old, has led to its application to certain problems in American anthropology. Three general problems have presented themselves: the antiquity of the blood groups in the New World, whether prehistoric or resulting only from historic European migration; the relations between the physically different Basket Maker people² and the later Pueblo people of the Southwest, together with the relation between these peoples and certain other peripheral groups of as yet uncertain position; and the relation of ancient Peruvian peoples to the present inhabitants of South America and to ancient North American groups.

METHODS

The methods of blood group determination in mummies were based on the methods successfully used abroad and in this country³ for the determination of the group of blood stains. A detailed description of the technic can be found in the references given.¹ Briefly, it depends upon the fact that the chemical substances called agglutinogens A and B occur not only in the blood, but in the tissues as well, and are quite stable chemically. They can not be demonstrated in the tissues by direct agglutination; but can be shown to be present, since they combine specifically and firmly with the agglutinins anti-A and anti-B. A mixture of these agglutinins is placed on a small sample of the finely ground tissue, and later the mixture is tested with known A and B red cells to see if both agglutinins are still present. If the A cells, say, are not now agglutinated by the mixture, it follows that the anti-A agglutinin has been taken out by the mummified tissue, and in order to do this the tissue must have contained the substance A. In practice

¹ W. C. Boyd and L. G. Boyd, *Blood Grouping by Means of Preserved Muscle* (Science, Vol. 78, p. 578, New York, 1933), *Blood Grouping Tests on 300 Mummies* (Journal of Immunology, Baltimore, Vol. 32, p. 307, 1937)

² For convenience we use the so-called "Pecos classification" of the culture horizons of the Southwest, realizing that sites of the same horizon are not necessarily contemporaneous and that analogy with peripheral cultures is only comparative (see F. H. H. Roberts, Jr., *A Survey of Southwestern Archaeology*, American Anthropologist, Vol. 35, p. 1, 1935)

³ L. Lattes, *The Individuality of the Blood* (London, 1932), W. C. Boyd and L. G. Boyd, *Blood Grouping in Forensic Medicine* (Journal of Immunology, Baltimore, in press).

it is advisable to do this in a quantitative manner, with careful controls and checks.

MATERIAL

The material⁴ consisted of dried tissue from 226 human burials. Of these, 59 were from the southwestern United States, 34 from Alaska or the Aleutian Islands, and 133 from Peru. Most of the specimens included desiccated muscle but a few were of skin or other tissues only. Table 1 presents a description of this material.

TABLE 1. SOURCES OF MATERIAL

<i>Culture Horizon</i>	<i>Locality</i>	<i>Males</i>	<i>Females</i>	<i>Children</i>	<i>Unknown</i>	<i>Total</i>
<i>North American (Southwest)</i>						
Hypothetical	Northern Periphery	1		2	1	4
Basket Maker I	Lovelock Cave, Nev.					
Basket Maker II	San Juan drainage	2	5	3	6	16
Basket Maker III	San Juan drainage and Eastern Periphery	1	1		5	7
Pueblo I	Northern Periphery				2	2
Pueblo III	San Juan drainage	5	3	3	4	15
Pueblo IV	Rio Grande drainage	1	1	1	1	4
Big Bend cave culture	Eastern Periphery Val Verde Co., Texas		2	3	1	6
Unknown	Mojave Desert		1	1		2
Unknown	Kern Co., and Monterey Co., Calif.		1	1		2
Unknown	Cocomorachi, Chihuahua, Mexico		1			1
<i>North American (Alaska)</i>						
Prehistoric Eskimo	Alaska	1	1		1	3
Aleutian	Aleutian Islands				31	31
Pre-Russian or Early Russian	Four-Mountain Island (30 specimens)					
<i>South American (Peru)</i>						
Various Peruvian	See note under "Material"	60	42	11	20	133

⁴ We wish to express our gratitude for information and the generous gift of material to the following individuals and institutions: Donald Scott, E. A. Hooton, Clyde Kluckhohn, Peabody Museum, Harvard University; E. W. Gifford, Museum of Anthropology, University

The Basket Maker II material was from southeastern Utah, the Tsegi drainage in northern Arizona, and the Chin Lee drainage, northeastern Arizona. The culture horizon of the specimens called Basket Maker III was somewhat problematical. Two were of the circular pithouse period from the Lukachukai Mountains and the Tsegi drainage, Arizona, three from the Guadalupe Mountains, New Mexico, and two from near Shiprock, New Mexico. The Pueblo I specimens again were problematical, being of the "Fremont River culture" from near Vernal, Utah. The Pueblo III material was from sites well scattered throughout the San Juan area in Arizona, Utah, Colorado, and New Mexico. Two of the Pueblo IV specimens were of doubtful provenance, one being possibly Pueblo III.

Of the Alaskan material, one was of the Old Bering Sea culture, one was late prehistoric and the other of doubtful provenance.

The Peruvian material was from the following localities: Ica Valley (24 specimens), Chincha Valley (15), Pachacamac (18), near Cuzco (7), Ancón (17), Mochica (5), Yauca (6), Supe (2), Pisco (1), Marca Huamachuco (5), Moche, near Trujillo (5), Paracas (6), Nasca (1), Valle Chillón (2), Valle Wayurí (1), Dept. de Huancavelica (1), near Lupo (4), Titicaca (1), unknown (12). The majority of the sites were prehistoric, but a few sites are known to have burials made after the Spanish conquest.

RESULTS

Table 2 presents a summary of the positive results. We wish to emphasize that the hypotheses concerning the relations and migrations of the ancient Americans offered in the following discussion are highly speculative, and should be considered as such. They are presented solely for the purpose of stimulating discussion. Any worker today who would draw definite conclusions based on the results of blood group determinations in as small numbers of living people as the number of specimens we were able to obtain,

of California; H. L. Shapiro, American Museum of Natural History; A. Hrdlička, F. M. Setzler, H. B. Collins, United States National Museum; E. B. Howard, F. de Laguna, University Museum, University of Pennsylvania; Byron Cummings, University of Arizona; H. S. Colton, Museum of Northern Arizona, Flagstaff; D. D. Brand, University of New Mexico; Paul Reiter, School of American Research, Santa Fé; J. L. Nusbaum, H. P. Mera, Laboratory of Anthropology, Santa Fé; E. P. Leavitt, Mesa Verde National Park, Colorado; F. E. Watkins, Southwest Museum, Los Angeles; A. Woodward, Mildred V. Brown, Los Angeles Museum of History, Science and Art; M. J. Rogers, San Diego Museum; J. Bronfenbrenner, Washington University School of Medicine; G. C. Martin, Terrell Wells, Texas; G. A. Matson, University of Montana; E. H. Morris, Carnegie Institution of Washington; National Park Service, U. S. Department of the Interior, Witte Memorial Museum, San Antonio, Texas; Museo Nacional, Lima, Peru; Royal College of Surgeons, London.

TABLE 2. POSITIVE RESULTS

<i>Culture horizon or period</i>	<i>Locality</i>	<i>Material</i>	<i>Blood group</i>
<i>North America (Southwest)</i>			
Hypothetical	Northern Periphery	Male	A
Basket Maker I (possibly later)	Lovelock Cave, Humboldt Valley, Nevada	Museum of Anthropol., Univ. of Calif., No. 12- 2652	
Basket Maker II	Chin Lee drainage Cañon del Muerto, Ariz.	Amer. Mus. of Natural History, No. 99-8372	A
Probable Basket Maker III; circular pithouse period	Tsegi drainage Duggagei Canyon, Ariz.	Female Univ. of Arizona, Tucson	A
Big Bend cave culture	Eastern periphery Goat Cave, Burial No. 4, Val Verde Co., Texas	Senile female U. S. National Mu- seum	B
<i>South America (Peru)</i>			
Prehistoric or Early Spanish	Near Lupo	Child San Diego Museum, No. 1149	A
End of 1st century A. D.	Paracas, Necropolis	Museo Nacional, Lima, No. 12-8951	AB
14th-15th century A. D.	Valle Chillón	Same, Lima No. 33-86	AB
End of 1st century A. D.	Paracas, Necropolis	Same, Lima No. 12-8951	B
Aboriginal, probably pre- historic	Ocucaje, Ica Valley, Site A	Female, 22 Museum of Anthropol., Univ. of Calif., No. 12-2880	B
Aboriginal, probably pre- historic	Tate-Chulpaca, Ica Valley, Site T, gr. "k"	Adult female Univ. of Calif., No. 12-2961	B
Aboriginal, probably pre- historic	Santiago, Ica Valley Site S	Male, (?), 25 Univ. of Calif., No. 12-2990	B
Aboriginal, probably pre- historic	Supe	Male, 40 Univ. of Calif., No. 12-3143	B
Aboriginal, probably pre- historic	Ancón, Site M, gr. 12	Male, 30 Univ. of Calif., No. 12-3044	B

would be held up to ridicule. If we had been able to test thousands of specimens instead of 226 the results might have been quite different. All we claim to have shown definitely is the presence of both the antigens A and B in prehistoric Americans. This in itself is significant, proving a greater antiquity of these groups in the New World than had previously been admitted. Theories regarding the occurrence of these groups as recent muta-

tions in the Old World since the last migration of the original settlers of America, based on the often repeated assertion that full-blooded American Indians are all of group O, have been weakened by recent findings of more A in putative full-bloods than can be explained on the basis of white admixture.⁵ We believe that our findings definitely disprove these theories.

Basket Makers. Although the Lovelock Cave culture cannot be placed with assurance in the chronology which has been developed for the Southwest, the occurrence of the atlatl and the absence of the bow in the earlier deposits indicates that the earlier period may be of Basket Maker horizon. In their report on the cave Loud and Harrington⁶ say, "... it may well be that this culture exemplifies the hitherto hypothetical 'basic culture' of the Southwest from which the typical Basket Maker is thought to have developed after the acquisition of agriculture," and they suggest that the term "Sub-Basket Maker" or Basket Maker I might be appropriate. It should be noted that the cranial indices of the adult bodies found in the cave ranged from 71.6 to 78.4⁷ (mesocephalic), which might be against a close relationship to the long-headed Basket Makers of the San Juan area. The body in which we found the antigen A came from a medium depth in the cave, so it may not correspond with the hypothetical Basket Maker I horizon of the lower levels. Since our other positive tests for A in North American material were in pre-Pueblo remains, none being found in material from Pueblo or later horizons, a relationship between the Lovelock Cave people (at least those of medium antiquity in relation to the upper and lower levels) and the pre-Pueblo group of the Southwest might be inferred.

The other two positive tests for antigen A were in material of typical Basket Maker II and probable Basket Maker III horizons, respectively. Negative tests in all the other material does not necessarily mean that the people from which it came were all of group O. They may have been O, or the antigens may have deteriorated. There is yet no sure method for determining which is the case. The facts, however, that we did obtain our three positive results for A among the 27 specimens of pre-Pueblo horizons and obtained no positive results among the 21 specimens of Pueblo horizons may be presumptive evidence that the Pueblo remains were predominantly

⁵ G. A. Matson and H. F. Schrader, *Blood Grouping among the "Blackfeet" and "Blood" Tribes of American Indians* (Journal of Immunology, Vol. 25, p. 155, Baltimore, 1933).

⁶ L. L. Loud and M. R. Harrington, *Lovelock Cave* (University of California Publications in American Archaeology and Ethnology, Vol. 25, p. 1, Berkeley, 1929).

⁷ E. W. Gifford, *Californian Anthropometry* (University of California Publications in American Archaeology and Ethnology, Vol. 22, p. 217, Berkeley, 1926).

group O. It is certain that there had been no greater opportunity for the antigens to disappear in the Pueblo material, since the conditions of burial and preservation by desiccation were the same in the two groups. The contrary is more likely, since the Basket Maker material is much older.

It is known that a change in physical type occurred between Basket Maker and Pueblo times, the Basket Maker people being dolichocephalic, the Pueblo people predominantly brachycephalic, with occasional round-heads among the late Basket Maker III people, and a decreasing number of long-heads through the Pueblo I, II, and III periods. There are, of course, other differences which we shall not discuss. This situation is consistent with the absorption of the original Basket Maker stock by numerous new round-headed immigrants. We interpret our findings as confirmatory evidence of the physical distinctness of the two groups, and suggest that the Basket Makers had a fair percentage of A, whereas the newcomers were predominantly O. The living Pueblo Indians are still predominantly O, the amount of A and B being explicable on the basis of recent mixture (83.6% O, 14.3 A, 1.7 B, 0.25 AB; 1175 cases), although there may have been some retention of the original A. Large numbers of Pueblo type immigrants, and a certain amount of extermination of the original inhabitants by them would account for the change in blood group frequencies. The new genes for O, though recessive, would of course nevertheless "dilute" the A.⁹

Allen and Korber¹⁰ found that the living Navaho have a fairly high percentage of A (69.13% O, 30.6 A, 0.16 B; 622 cases), somewhat higher, especially in the presence of so little B, than can be accounted for on the basis of white admixture. The Navaho, however, are supposed to be fairly recent arrivals in the Southwest (possibly around 600 years ago). We do not regard their high percentage of A, therefore, as being necessarily derived from the original A of the Southwest, but more likely from farther north. Matson and Schrader's finding of 76.5 percent A in putative full-bloods among the Blood and the Blackfoot⁵ may be related to this problem. Hooton¹¹ has postulated four major groups of migrations to the New World by way of the northwestern route from Asia: first, the long-headed Basket Maker type; second, the round-headed Mongoloids in large numbers; a later

⁵ F. W. Allen and W. Schaeffer, *The Distribution of the Human Blood Groups among the Navajo and Pueblo Indians of the Southwest* (Bulletin, University of New Mexico, Biological Series, Vol. 4, No. 2, Albuquerque, 1935)

⁹ Some apparently not versed in genetic principles have carelessly assumed that the relative frequency of a dominant gene necessarily increases in a population containing both dominant and recessive. Needless to say, this is false.

¹⁰ F. W. Allen and J. Korber (see F. W. Allen and W. Schaeffer, *loc. cit.*, footnote 8).

¹¹ E. A. Hooton, *Up from the Ape* (New York, 1931), page 568.

invasion of tall, long-headed types; and lastly the Eskimo. It is possible that the first invasion brought much A, the second was predominantly or all O, and the third again brought A which was distributed to the northern peoples, among them the ancestors of the Navaho, the Blood, and the Blackfoot.

The tests on the 34 specimens from Alaska and the Aleutian Islands were all negative (suggestions of reactivity in some of the Aleutian material could not be confirmed). Unless other evidence is found this would indicate that the last migration was predominantly O.

Big Bend Cave Culture. The position of the Big Bend cave culture¹² of southwestern Texas is still problematical. Although there are certain similarities between it and the classical Basket Maker II culture and it has been called the Big Bend Basket Maker culture, Setzler has advanced the opinion that it represents a more or less independent group or possibly a cultural lag. He suggests that it may be part of a larger basic culture centering in northern Mexico, and not an example or development from the classical Basket Maker. He points out certain affinities with the late Basket Maker III of the Chin Lee drainage in Arizona, but admits that its relative chronology is very indefinite. The dolichocephalic indices, 66.2 to 72.3 in the Shumla material and 65.3 and 67.2 in the Goat Cave burials, would relate these people to the Basket Makers rather than to the Pueblo people, unless they are a distinct group. Our finding of a positive test for antigen B in one of the Goat Cave specimens (C.I. 67.2) might be interpreted in several ways. It must be remembered, however, that any interpretation is highly speculative, since the material was so limited. If we had tested a hundred specimens instead of six we might have found plenty of A. Since our few positive results in Basket Maker material showed only the presence of antigen A it might be assumed that this is evidence that the Big Bend people were not related to the Basket Makers. Another attractive hypothesis would be that our finding of considerable antigen B in prehistoric Peruvian material, and this single demonstration of B in North American material, is evidence that the Big Bend culture represents a station of people bearing B who migrated along the eastern periphery of the Southwestern area to populate South America. This is not unlike Setzler's suggestion that the culture may be part of a basic culture centering in Mexico, independent of the Basket Makers.

¹² G. C. Martin, *Archaeological Exploration of the Shumla Caves* (Big Bend Basket Maker Papers No. 3, Southwest Texas Archaeological Society, Witte Memorial Museum, San Antonio, 1933); F. M. Setzler, *Cave Burials in Southwestern Texas* (Explorations and Field-Work of the Smithsonian Institution, Washington, 1933); F. M. Setzler, *A Prehistoric Cave Culture in Southwestern Texas* (American Anthropologist, Vol. 37, p. 104, 1935).

Peruvian Material. The finding of eight positive tests for antigen B and only three for A in Peruvian material is especially significant (AB in two individuals). Although two of the burials may have been made after the Spanish conquest the other seven are almost certainly prehistoric. Rahm's remarkable finding of 91 percent B in the Yahgans of Tierra del Fuego and Golden's report of 51 percent B in the Caraja Indians of Brazil,¹³ although based on relatively few determinations (33 and 61) and needing confirmation, were difficult to explain unless the prehistoric population of South America possessed B. Our findings show that B was present in South America in prehistoric times and make these findings in living Indians understandable. The low percentage of B in living North American Indians, plausibly attributed to historic admixture, and our failure to find B in the Basket Makers, needs explanation. We did find one B, however, in the Big Bend material. Hooton¹⁴ gives as modern representatives in mixed form of the "Palae-American" or "Basket Maker" type the Yahgans and many Amazonian tribes, the very people in which the authors cited found much B. Is it not possible that this B and that found by us in Peruvian mummies was brought by very early migrants who had wandered to South America, possibly by the Big Bend route, becoming extinct in North America before the arrival of the O bearing Mongoloids? The lack of B and the presence of A in the Basket Makers may be because they were a distinct stock, although physically similar in other respects; because they came later bearing A, or because we did not have enough Basket Maker material to test. It must be remembered that we did find some A in the Peruvian material, which would relate the Basket Makers to South America, either originally or later. We know very little about what are now supposed to be the earliest migrants to America, the so-called "Folsom Man." They are said to have preceded and to have been distinct from the Basket Makers. Perhaps they were the ones who brought the B, which was carried to South America and disappeared except in the Big Bend region in North America, before the arrival of the A bearing Basket Maker stock.

ANTIQUITY OF THE BLOOD GROUPS

It would seem that the finding of A and B in prehistoric American material strengthens the arguments we have elsewhere advanced¹⁵ in favor

¹³ G. Rahm, *Die Blutgruppen der Araukaner (Mapuches) und der Feuerlander* (Forschungen und Fortschritte, Vol. 7, p. 310, 1931 cited by Lattes), *Los grupos sanguíneos de los Araucanos (Mapuches) y de los Fueguinos* (Investigación y Progreso, Vol. 5, p. 160, 1931 cited by Gates and Darby); G. Golden, *Distribution of Blood Groups in South American Indians* (Lancet, Vol. 219 II, p. 278, London, 1930).

¹⁴ E. A. Hooton, *op. cit.*, page 522.

¹⁵ L. C. Wyman and W. C. Boyd, *Human Blood Groups and Anthropology* (American Anthropologist, Vol. 37, p. 181, 1935).

of the antiquity of these factors in human evolution, and against the idea that they can be relatively recent mutations. The only alternative to supposing that the B gene was brought by early immigrants to two regions in South America¹³ is the hypothesis of two independent centers of mutation.

We have previously pointed out the mathematical considerations which show that it would require a fairly high rate of mutation for a gene to increase so much without selection in the time which is generally considered to have elapsed since the first migrations to America. For example, for blood group B to have increased from 0 to 91 percent in 20,000 years would require a mutation rate of about 1.3×10^{-8} . The most frequent mutation observed in *Drosophila* has a frequency, according to Hanson,¹⁶ of about 10^{-4} . In man little data on mutation rates are available, but the highest estimate¹⁷ so far is 2×10^{-5} .

It is possible, however, that some genes mutate more rapidly than this in man, as maintained by Gates,¹⁸ in his stimulating paper in "Genetica." Even if we grant that the rate can be as high as 10^{-3} and admit that the B in America could be of relatively recent origin independent of that in the Old World, there is still a certain inherent improbability in trying to account for the whole of the existing data on such a basis. Thus Gates finds it necessary to assume in the New World two independent centers of B mutation, and one center of A mutation. If the A in the Basket Maker mummies and the B in the Big Bend and Peruvian material are proofs of the original existence of these factors in America, this requires three more independent centers of mutation, unless relationships between the people in these areas are assumed. This implies that the blood grouping genes, in the course of man's history, have in at least eight independent places, at eight independent times, begun to arise by mutation and *continued to do so* at a rate more rapid than that found for the great majority of genes thus far studied in *Drosophila* or in man. This may not be impossible, and we certainly have no means of disproving it, but it is surely not more credible than our suggestion¹⁵ that stocks lacking one or more of the blood groups have occasionally originated by accidental isolation from an original human stock possessing all three factors O, A, and B, like the anthropoids.

Even Gates, to account for the lack of B among the other American Indians, since he admits that the mutations A and B might already have

¹⁶ F. B. Hanson, *Radiation-genetics* (Physiological Reviews, Vol. 13, p 466, Baltimore, 1933)

¹⁷ L. S. Penrose and J. B. S. Haldane, *Mutation Rates in Man* (Nature, Vol. 135, p 907, London, 1935).

¹⁸ R. R. Gates, *Recent Progress in Blood Group Investigations* (Genetica, Vol. 18, p 47, The Hague, 1936).

been present before the first migrants crossed the Bering Strait, has recourse to the concept of isolation, which, as he says, ". . . must have been a factor of extraordinary importance in the early history of man." Blood grouping tests on Egyptian mummies¹⁹ indicate that A and B were probably present and fairly well distributed (if B comes from India) in the population of ancient Egypt at least 5,000 years ago. If we must admit that the mutation to B, which according to those who maintain the separate human origin of the blood groups is much later than A, was already fairly common in the Old World 5,000 years ago, and consequently had originated even earlier, we are not in much better position to explain the peculiarities of the blood grouping distributions of the American Indians, the Australians, the Lapps, the Bushmen, and the Polynesians, than if we admit the human race has always possessed some A and B. And if we are forced to assume isolation as well as mutation, it would seem that we might as well do without the concept of mutation in this connection, for the sake of economy of hypotheses. It can, of course, never be shown that mutation rates high enough to produce the type of change under consideration do not occur in man.²⁰ It is strange that the possibility does not seem to have been considered also in the case of other physical anthropological characteristics.

SUMMARY

The presence of agglutinogens A and B has been demonstrated in pre-historic American Indian tissue, thus favoring the antiquity of these factors in human evolution and weakening the theory that they arose as recent mutations in the Old World since the last migration of the ancestors of the Indians to America. Tentative hypotheses are offered relating the Lovelock Cave people to the pre-Pueblo group of the Southwest, confirming the physical distinctness of the Basket Maker and Pueblo peoples, suggesting a later origin for the high percentage of A among the Navaho, relating the Big Bend Cave people to South American groups, confirming the antiquity of B in South America, and suggesting relations between ancient South Americans and the usual postulated migrations.

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¹⁹ L. G. Boyd and W. C. Boyd, *Les Groupes Sanguins chez les Anciens Égyptiens* (Chronique d'Égypte, No. 23, p. 41, Bruxelles, 1937)

²⁰ Inbreeding, often invoked to explain high percentages of a blood group in isolated populations, is of course powerless, without selection, to increase the original frequency (in the detached population) of the gene.

THE BLUEJAY CHARACTER IN THE PLATEAU SPIRIT DANCE

By VERNE F. RAY

PERHAPS nowhere in America did the guardian spirit concept play so great a role in the lives of a people as among the Salishan groups of the Plateau. Long ago James Teit made this discovery in his work with the Salish of interior British Columbia, and his descriptions of their beliefs and practices¹ furnished both the type picture² and a considerable bulk of documentary material for Dr Benedict's subsequent study of the concept for North America generally. More recent studies have demonstrated a similar intensity of the concept among the Salish of interior Washington.³

Not only was the guardian spirit idea highly developed here as a formal religious complex in which very nearly every man participated, but that participation was no passive acquiescence to cultural tradition. It was rather an intense identification highly charged with emotional content. This emotional intensity reached its peak during the winter period of spirit dances in which all those possessing guardians participated. But this winter period was not a time when otherwise absentee spirits returned or were recalled for sake of the ceremony, as, for example, in nearby Puget Sound. For here spirits were ever present. A man and his tutelary led parallel existences; the one had only to reach out, so to speak, and draw the other to him when the occasion made unitary action or cooperation desirable. The power inherent in the possession of a guardian spirit was not, in other

¹ James Teit, *The Lillooet Indians* (Publications, Jesup North Pacific Expedition, Vol. 2, pp. 193-300, 1906); *The Shuswap* (same series, Vol. 2, pp. 443-789, 1909); *The Thompson Indians of British Columbia* (same series, Vol. 1, pp. 163-392, 1900); *The Salishan Tribes of the Western Plateaus*, Franz Boas, ed. (Report, Bureau of American Ethnology, Vol. 45, pp. 23-396, 1930).

² "The type picture of the North American guardian-spirit practices corresponds most nearly to the customs of the Plateau area, let us say the Thompson River Indians (Teit-Thompson Indians). There was here the isolation in the mountains at puberty, the long ceremonial purification, the intentness upon supernatural communication, and the acquisition of the name and power and song of the guardian spirit in a vision. For months or even years the youths carried out strict dietary regulations with frequent rigid fasts; purged themselves with medicine and induced vomiting by pliant sticks; purified themselves by sweat bathing, followed by a plunge into the cold stream. There were no limitations of rank or ownership upon the experience or the tutelary spirits; the quest was open to and incumbent upon all the young men of the tribe" (Ruth Benedict, *The Concept of the Guardian Spirit in North America*, Memoir, American Anthropological Association, No. 29, 1923, p. 10).

³ Verne F. Ray, *The Sunpoil and Nespelem Salishan Peoples of Northeastern Washington* (University of Washington Publications in Anthropology, Vol. 5, 1932), pp. 169-211; Walter B. Cline, et al, *The Sinkaietk or Southern Okanogan of Washington* (General Series in Anthropology, No. 6, in press).

words, a talent once conferred and thereafter effective, or a vague store of energy to be drawn upon when needed; but rather a highly specific relationship with a powerful ally available for personal action in any exigency. Over-formalization in ethnographic accounts has obscured this fact.

In everyday life this intimacy of relationship was reflected in countless references to one's own or another's tutelary, and in the invariable explanation of all unusual, impressive, or significant occurrences in terms of spirit power. Reference to one's own spirit was never specific, however, but always couched in vague or generic terms: "my power," "my helper," or, in colloquial English, "my partner." This last expression, heard constantly among these peoples even today, reflects most accurately the native attitude toward spirit power.⁴

Another key to the depth of the personal relationship is the spirit-ghost concept. When a man died his tutelary did not return to its genus, disappear, or merely become non-existent. It was inconceivable that an entity so intimately associated with the deceased should not undergo a major transformation likewise. Consequently, the spirit "died" also, becoming thereby transformed to a spirit-ghost, one of a class of supernatural beings closely resembling in form the ghost of the soul. The identifying characteristics of the spirit were no longer retained, the new form being vaguely anthropomorphic.⁵ The full distribution of the spirit-ghost concept is not yet determined, but it is known for the Southern Okanogan,⁶ Sanpoil, Nespelem, Kalispel, Wenatchi, and Kittitas. It apparently is present in modified form among the Lakes (Senijextee), but is definitely absent for the Klikitat.

The winter dance was mentioned above as the period when spirit consciousness reached the point of saturation. Impersonation of guardian spirits was a well recognized feature of the ceremony. Another aspect, far more instructive for a fundamental understanding of the complex, was identification—as opposed to mere impersonation—with the spirit. Identification involved radical and thorough transformation from the normal human state to a condition in which all social relationships were abjured, and activities duplicated, as closely as physical limitations permitted, those of the spirit.

Such identification may seem to do violence to the strongly developed duality principle. To resolve this apparent difficulty it is necessary to recall the native theory of genesis for the guardian spirit. During mythological

⁴ Cf Ray, *loc. cit.* The description holds for the Salish of interior Washington generally.

⁵ Cf Ray, *op. cit.*, pp 173-76.

⁶ Cline, *op. cit.*

times there were no human beings; neither were there guardian spirits. But the beings of that time possessed attributes of both. Their physical and psychological differentia were those of animals, but for the most part they appeared in the superficial guise of men. The animal form as such was taken only when an emergency demanded. They sought no spirit power since they possessed it inherently. "When people came on earth," it is explained, "these beings *became* the spirits." But in the new era the dominant form became that of the animal; the man-like form was taken only when in communion with men. Human beings appeared at the same time, inheriting as their guise the prevalent form of the previous beings. But men did not receive the power to reciprocate with their spirits and appear in animal form. This was possible, in modification, only in ceremonial identification. This identification was not an amalgamation or a substitution, but a re-identification to the conceptualized form of the mythological being.

Such transformation was peculiarly the prerogative of persons with Bluejay⁷ power; thus the term "Bluejay character" may conveniently be used for purposes of designation. Despite the dramatic nature and theoretical implications of this aspect of the dance it has been largely overlooked.⁸

A short summary of the Sanpoil form will serve as a basis for discussion. Subject to the transformation were those with either Bluejay or Owl as tutelary. With the approach of the winter period given over to guardian spirit dances (December, January) these individuals removed all clothing except the breechcloth, and blackened the face, hands, and feet. For the two months of the dances they donned no clothing despite rigors of the weather. They shunned "human beings" and avoided conversation, even among themselves. Further, they ate apart, becoming scavengers of refuse from meals and stale food (in keeping with the habits of bluejay and owl). During the dances they acted as sentries, patrolling the grounds outside the dance house or perching on the rafter supports inside the building. From the latter vantage point they observed intently the actions of those below to detect any breach of the formal rules of conduct. If one were seen eating during the dance, for example, the sentry "flew down and recovered the food from the person's throat." In like manner other rules were enforced. The sentries themselves never danced or mingled with the dancers.

⁷ The terms bluejay and owl are here capitalized when the guardian spirit or the dance character is intended; uncapsalized, the reference is to the bird as such.

⁸ In 1932 I described the Sanpoil form (*op. cit.*, pp. 191 f.); in 1933 Dr Harry Turney-High provided a description for the Flathead (*The Bluejay Dance*, *American Anthropologist*, Vol. 35, 1933, pp. 103-107); an account for the Colville is contained in Cline (*op. cit.*). I am aware of no others.

Sometimes they disappeared from the dance for a time to perform similar duties in other parts of the village; restrictions of conduct during a dance applied to the village as a whole. The sentries were capable of detecting violations, no matter where they occurred. Similarly, they perceived the approach of visitors long before the dance house was reached; this information was conveyed to the dancers by symbolic actions. When the dance season ended it became necessary to capture the sentries in order to return them to their former state. Though the original metamorphoses had been achieved by the characters themselves, they were powerless to effect the retransformation. In fact they violently opposed it. They were caught by being waylaid by friends, whereupon they "died." Through being held over a smudge they were revived as normal human beings. Had they been allowed to go their way, "they would have gone wild and run themselves to death."⁹

The Colville are adjacent to the Sanpoil on the north. From Walter Cline we have the following description which is quoted in full:

Among the Colville, power derived from the bluejay differed from any recorded for the [Southern] Okanagon, for it especially enabled its possessor to find lost articles and people, and sometimes impelled him to flee human society and lead an insane life in the woods. Bluejay shamans¹⁰ used black face-paint. A man with this power once disappeared from a settlement near Marcus just after the winter dance and was not found till the next autumn. His brother dreamed that he might be discovered with a group of wild horses at noon on the day following the dream. Enlisting the aid of a few good ropers, the brother went out to capture him. They found him as foretold in the dream, leaping from one horse's back to another. The horses escaped, but the maniac was bound, fumigated with xacxac¹¹ root, and restored to normal life. David, the narrator, knew of no other instance of this kind, but Johnnie's account of the behavior of Colville bluejay shamans when they went out to find lost things, and the repeated statement that they "turned into bluejays," indicate a much more violent form of spirit possession than occurred among the Okanagon. Johnnie recounted that those with this power turned into bluejays at a dance, which no one was allowed to leave. They would fly through a crack in the door. They would stay naked in the mountains for months, living only on pitch. One such named inyās (Aeneas, Ignace ?) could jump up a tree and dance with one foot on its tip. People "spoke backward" to him. If a man was lost in the mountains

⁹ Ray, *loc. cit.*

¹⁰ Apparently the word shaman here is intended to convey a meaning analogous to my term character. Cline in general uses the word shaman in a very broad sense. It is certain from my own acquaintance with the Colville that there could be no confusion between the Bluejay character and the true shaman.

¹¹ This is a small plant, found in the mountains, which was valued for the fragrant odor characteristic of the root. The Sanpoil used it both as a perfume and a medicine.

or drowned, or if horses were lost, he was asked to find them. He hopped out on one foot; reporting on his return, "I did not find him," meaning the reverse. Another named q'ai'iyä would hop out and find a coin secreted in the snow. In all the cases given, the shamans had possessed their bluejay power for some time; we obtained no account of their conduct soon after getting the power.¹²

These data place the Southern Okanogan definitely outside the area of the Bluejay character. For the Colville, it introduces two new or modified features: the ability to find lost objects or persons; and the "talking back-

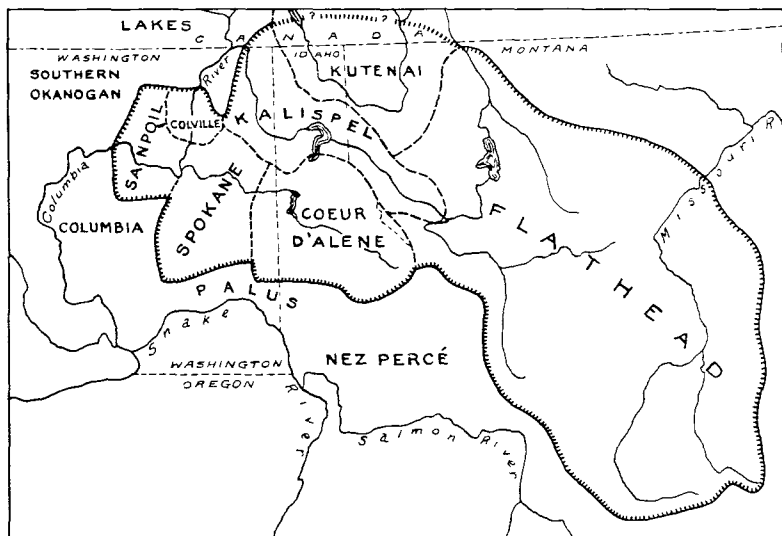


FIG. 1 Probable distribution of the Bluejay character

ward" as contrasted to the careful avoidance of conversation among the Sanpoil. Also, Owl has disappeared as a participant, and, to anticipate, will not reappear.

Among the Spokane, adjoining the Sanpoil on the east, a new element appears in the participation of the Bluejay character in the dance as a pseudo-shaman. Treatment was limited, however, to cases where the peculiar attributes of the bluejay indicated aptitude. Thus, just as among the Sanpoil the sentry retrieved food from a person's throat, among the Spokane he withdrew from a patient's stomach foreign matter which inadvertently had been swallowed.

¹² Cline, *op. cit.*

The Kalispel, immediately north of the Spokane and east of the Colville, extended this curing power to include one of the typical shamanistic procedures, that of sucking. Bluejay characters were paid by ailing dancers to treat those illnesses for which sucking was deemed proper. They were not privileged to use the other shamanistic modes of treatment, namely blowing and drawing out with the hands.¹³ The Bluejays were definitely set apart from true shamans and contrasted to them. Informants emphasized this distinction by pointing out that true shamans never were paid.¹⁴ The talent for ferreting out the missing is encountered here also. The phrasing is closely parallel to that of the Colville, especially in the interest expressed over objects recovered from under the snow. Thus one informant told how his father had regained a knife by enlisting the services of a Bluejay during a winter dance. The knife had been lost the previous summer; it was found at some distance under three feet of snow. It was explained that this power of the Bluejay was limited to such objects as contained material derived from animal life. If this provision were met, success was certain regardless of distance or elapsed time. The knife in question had had a handle of buckhorn; had it been of wood the Bluejay would have been powerless.

The Kalispel characters observed a "complete" fast of water as well as food for as long as eight days, the duration of the dance. Clothing was not shunned as uniformly as among the Sanpoil, this being largely an individual matter. Likewise, some blackened their faces and hands, some did not. The transformations occurred during the first hours of the dance, not prior to it. Bluejays often left the vicinity of the dance house, remaining away for several hours or a whole night. But contrary to Sanpoil custom, the nature of these escapades was kept strictly secret, even from one another.¹⁵ The revivification and return to normal was sometimes accomplished by smudging but more often by shamanistic treatment. The shaman in charge of the dance, or another practitioner, allayed the condition by the common drawing out and blowing procedure.

South of the Kalispel and east of the Spokane lay the Coeur d'Alene. Here familiar elements are encountered together with new ones and a strong emphasis upon Bluejay as a seer is discerned. Like the Sanpoil they blackened the face and hands, wore nothing but the breechcloth, sat among the roof poles, and strictly avoided the dancers. Sentry duties were likewise similar but somewhat more formalized in that a badge of authority in the

¹³ Cf. Ray, *op cit.*, pp. 205 f.

¹⁴ But they might receive presents. This distinction may seem artificial, but to the Kalispel it is very real and strongly defended.

¹⁵ This explains the "eight day complete fast."

form of a diminutive bow and arrows were carried. The Coeur d'Alene sentry was not exempt from the food tabus but was restricted to the same degree as the dancers. As a seer the Bluejay was not only able to find lost or hidden objects or persons but made a point of using this knowledge to the embarrassment of the dancers, by calling down to the audience from his perch in the rafters and disclosing secret activities, tabu violations, and other irregular actions on the part of members. The foretelling of events with great specificity was likewise characteristic. Speech with the dancers was not restricted. In keeping with the character of the bird, the Bluejay was said to travel with great speed, ignoring impediments. The characters travelled from one dance to another throughout the dance period. No pseudo-shamanistic attributes were possessed. Here even more than elsewhere the Bluejay resisted being returned to the human state. Informants tell of pursuits over many miles of snow covered ground before the capture of one or another Bluejay could be effected. Once caught, the retransformation was accomplished by holding live coals covered with aromatic roots under the subject's nose. Smudging the whole body was never practiced.

Among the Flathead, east of the Coeur d'Alene and Kalispel, the complex appears, at first glance, to have been considerably altered in character. Turney-High writes:

The Dance of the Bluejay is probably the principal expression of the hopes and woes of the Montana Salish. . . .¹⁶

Three days before the ceremony is to begin, the shamans, who are called quasquays in Salishan, assemble. . . . Their personal "medicine," called in Salishan *sumesh* . . . is usually in the form of an animal who became the guardian of the man at the time he developed into a quasquay.

When . . . the dance of the second night begins, the sick lie down before the quasquays. . . . Should, for example, the *sumesh* of the quasquay be the Bluejay, he will throw a feather into the vital organs of an enemy [to cause illness or to demonstrate power in the dance contests].

By this time in the dance [second night], the Bluejay *sumesh* has begun to possess the quasquays. . . . The old men of the assembly decide that it is time to allow the quasquays to "go wild." . . . At this the quasquays are entirely possessed by the Bluejay, in fact become bluejays, and begin to "speak in tongues." . . . Chirping and cawing they ascend the lodgepoles and run about the rafters with remarkable agility. . . .

The "Bluejays" are permitted to "go wild" until the beginning of the third night. Then the old men . . . smoke the spell out of them.¹⁷

¹⁶ Turney-High uses the term Salish for the group more commonly called Flathead. The name Salish is ambiguous since it is more widely used to designate the extensive linguistic stock to which Flathead speech belongs.

¹⁷ Turney-High, *op. cit.*, pp. 103-105.

Translations of the native terms used above are required before this data can be interpreted.¹⁵ Turney-High's word "sumesh" is simply the familiar Salishan term for guardian spirit (Sanpoil: *sumi'x*^u; Southern Okanogan: *sumi'x*; Lakes: *su'mi'x*^u), while "quasquay" is the common term for bluejay (Sanpoil: *qwa'skei*, Coeur d'Alene: *qwa'sqwel*; Klallam: *kwa'ckwac*¹⁹). But if the native terms in the quotations above are translated confusion still prevails.²⁰ It is obvious that Turney-High, after incorrectly equating the terms shaman and "quasquay," proceeds to use them interchangeably. This at once accounts for the apparent transformation of all shamans into Bluejay characters, regardless of their tutelaries; resolves the contradiction in the statement that the guardian spirit is the animal that becomes the patron of the Bluejay; and renders meaningful the tautological statements.

The identification of the shaman with the Bluejay might be accepted at face value were it asserted for Flathead ("Salish"¹⁶) culture. A step in this direction was found among the neighboring Kalispel. But Turney-High presents the identity as a feature of the Salishan language ("called in Salishan") which has been shown to be false.

The "Bluejay Dance" of the Flathead was merely the familiar winter guardian spirit dance of the Plateau, telescoped to comprise but four days of activity. Bluejay characters played a somewhat greater role than among groups to the west, but on the first and fourth days, half the duration of the ceremony, there were no distinctive features of the Bluejay complex. In the quotations above appear direct parallels with the spirit dance elsewhere (power contests, shamanistic curing). Other analogues include purification rites, ritual preparation of the dancing ground, guardian spirit dancing, similar formal rules of conduct, ritual quenching of the fires, a ritual expedi-

¹⁵ Without such translation it would appear from the first remarks that the actors in the dance were true shamans and that all such participated. This would imply that all shamans possessed Bluejay as guardian, or that participation in the "Bluejay Dance" was not limited to those with this power. But this will not hold in view of the statements, "Should, for example, the sumesh of the quasquay be the Bluejay," and, "the Bluejay sumesh has begun to possess the quasquays." This implies that shamans possessing guardians other than Bluejay nevertheless "become bluejays."

¹⁶ Herman Hachlerlin, *Types of Reduplication in the Salishan Dialects* (International Journal of American Linguistics, Vol. 1, pp. 154-74, 1918) p. 167.

²⁰ We then read "the shamans, who are called bluejays in Salishan;" "their personal 'medicine,' called . . . guardian spirit . . . is usually in the form of an animal who became the guardian of the man at the time he developed into a bluejay;" "the sick lie down before the bluejays;" "Should, for example, the guardian spirit of the bluejay be the Bluejay;" "the Bluejay guardian spirit has begun to possess the bluejays;" and, "At this the bluejays are entirely possessed by the Bluejay, in fact become bluejays."

tion to bring in the center pole, branches allowed to remain on the center pole, giving of gifts, and dancing for weather control.

The Bluejay feature itself exhibits numerous parallels with groups discussed above. Transformation occurred after the opening of the dance, as with the Kalispel. The old men (presumably the shamans) decided to let those with Bluejay power "go wild," that is, become transformed. Metamorphosis was complete; the characters perched among the rafters; retransformation was accomplished through smudging. Further perusal of Turney-High's text reveals many other parallels, including: lack of clothing, blackening of the face, duty as sentries, fasting, curing, specialization in curing, no conversation with human beings, and inability to retransform themselves.²¹

The expanded curing role of the Flathead Bluejay character deserves emphasis. But the questions, "Were the Bluejays true shamans; did they practice curing apart from the dance?" can be answered in the negative with reasonable safety. On the whole, the Bluejay complex of the Flathead fits well into the general pattern.

The distribution of the Bluejay character now apparently is exhausted, with the possible exception of the Kutenai. I was unable to visit this group, but the southern Kutenai, at least, may very well have shared the trait. Lakes (Senijextee) and Columbia informants declared the complex was unknown to their groups. It was absent, as we have seen, for the Southern Okanogan. Spinden does not mention it for the Nez Percé.²² For peoples to the east of the Flathead the complex is not mentioned, to my knowledge, in the existing literature. The accompanying map shows the probable boundaries of the Bluejay complex.²³

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²¹ Turney-High, *op. cit.*, *passim*.

²² Herbert J. Spinden, *The Nez Percé Indians* (Memoirs, American Anthropological Association, Vol. 2, Pt. 3, 1908).

²³ The name Sanpoil as used in this paper and on the map should be understood to include the Nespelem who were culturally identical. Likewise, the term Spokane is used broadly to include the three divisions, the Lower, Middle, and Upper Spokane. (See Verne F. Ray, *Native Villages and Groupings of the Columbia Basin*, Pacific Northwest Quarterly, Vol. 27, pp. 99-152, 1936.)

IN the AMERICAN ANTHROPOLOGIST¹ for last year, M. E. Opler analyzes the kinship systems of seven South Athabaskan groups. The data are presented compactly and conveniently for comparison; and a classification is made into two types, called Chiricahua and Jicarilla, of which the first is construed as developmentally earlier. Dr Opler's paper is executed with genuine workmanship, and his data are a boon: there has not been even one Apache kinship system previously on record, so far as I know. He has however taken no cognizance of Northern or Pacific Athabaskan kinship systems, long ago recorded by Morgan and Gifford; and it seems worth while examining these to see whether, at least on certain points, they do not suffice for a tentative reconstruction of primitive Athabaskan kinship which in turn will illumine the South Athabaskan situation.

Southern Athabaskan includes Chiricahua, Mescalero, Western Apache, Navaho, Jicarilla, Lipan, Kiowa Apache. They are all Apaches, historically and in Spanish usage. The first three are southwesterly, the last four northeasterly within the Southwest.

California Athabaskan includes Kato, Wailaki, Lassik, Sinkyone, Hupa, Tolowa. Linguistically there appear to be three groups: Kato-Wailaki-Lassik-Sinkyone; Hupa; and Tolowa-Oregon Athabaskan.

Northern Athabaskan (superordinate to the preceding) includes Slave Lake, Hare, Yellow Knife ("Red Knife"), Kutchin, Tukuthe, Carrier (this last from Morice, *Carrier Language*).

Kinship abbreviations are as introduced by Gifford in *Californian Kinship Terminologies*.

All original Athabaskan forms are rough generalizations, not proven or arguable reconstructions such as a philologist would designate by a *. The purpose is recognition of former kinship plans, not of precise linguistic forms.

Grandparents—Chiricahua and Mescalero have 4 terms: FF nale, FM tc'ine, MF tsoye, MM tco. Western Apache merges the two last, Navaho the two first, Jicarilla and Lipan use MF for both GF, and MM for both GM; Kiowa Apache has again merged and uses MF for all 4 GP. That this is the historical sequence is shown by the California Athabascans having the 4-GP scheme, with close correspondence of forms: FF al (Lassik, Wailaki; others aberrant: Tolowa ame', Hupa maatcwuñ, Sinkyone abak, Kato tcau); FM trene, tcin, tcuñ, tcañ; MF tcugi, teigi, tchuwe, sagi; MM tco, tcwo, su. Northern Athabaskan has only two terms, of type tsian and tsu, or tsun and tsea, for GF and GM; these forms probably correspond to FM and MF. The whole of America east of the Rockies is a region of

¹ M. E. Opler, *The Kinship Systems of the Southern Athabaskan-Speaking Tribes* (American Anthropologist, Vol. 38, pp. 620-33, 1936).

only two (or one) GP terms, so that the loss of half an original stock of 4 terms in the Northern as also among the Southern Athabascan of the Plains (Jicarilla, Lipan, Kiowa Apache) is infinitely more likely than that the Chiricahua-Mescalero and the California tribes should have independently devised not only 4 concept words but the same stems for them. In short, original Athabascan possessed separate terms for FF, FM, MF, MM.

Parents and Children—For F, the general Athabascan stem—Northern, Southern, California—is ta: Lipan and Kiowa Apache aci is therefore a secondary specialization. On the contrary, for M, Chiricahua-Mescalero-Western Apache-Navaho ma seems specialized, and Jicarilla-Lipan-Kiowa Apache ni, nandi, nade probably agrees with Northern Athabascan nde, ana, na-aing, etc., and California Athabascan nan, nang, ne, etc.

For children the situation is complicated. The clearest is a stem yacetc, yactce, yatce in California Athabascan and yatse, yadze in Northern Athabascan, which uniformly means wn D. This is obviously Southern Athabascan yatc'e', jatc'e', etc., which however appears more generalized as D; and which is replaced in Western Apache and Navaho by tsi'.

A second term, perhaps the base of the preceding one, is yaz, yaza in Northern Athabascan and yac in California Athabascan, and means wn Sn. In Hare and Yellow Knife this is widened to mean Sn; in Hupa and Tolowa it is lost, and its place taken by a term of different meaning, tse or sie, mn D, which is probably cognate with the just mentioned Western Apache-Navaho tsi', D.

The situation becomes too intricate to make further dissection profitable without more accurate phonetic and semantic record than is available for most tribes; but it does look as if original Athabascan might have had 4 terms (mn Sn, mn D, wn Sn, wn D); or at any rate 3, which is the number in all six California systems (though on two logical plans) and in Western Apache (adds wn Ch) and Navaho (adds wn Sn). In any event, the simple Sn-D terminology of Chiricahua-Mescalero does not seem original.

Siblings—Original Athabascan had 4 terms, each specifying sex and seniority.

	<i>Northern</i>	<i>California</i>	<i>Southern</i>
OB	unaga, unda	onung, onaga, on, ungutc	Navaho, Jicarilla, Lipan na'i oB
OSS	ada, ache, yat	at, ati, ade	Western Apache de oSb; Navaho adi oSs; Kiowa Apache dada oSs
YB	acha, chilea, che, chel	teil, teel, teal, kil, teelc	Navaho tsili yB; Kiowa Apache tcitl'a yB
YSs	adaze, tis, chith	t'eci, te, de, eci, detc	Western Apache dije ySb; Navaho deji ySs; Kiowa Apache detc'a ySs

Chiricahua-Mescalero are aberrant with a totally different plan: k'is, parallel Sb, la' cross Sb, without reference to seniority. Both these terms recur among the

five other Southwest Athabascan groups, but apparently either with narrower sense or as alternative words. (Opler's tabular data are difficult to interpret on this point because they refer to male egos.) In California, Tolowa has *disle mn Ss*, and *la'e* (*mn* and *wn*) *B*, as evident cognates; in the North, Carrier *lthes* "Ss" and *tetsin* "B" may or may not be cognate. There may thus be a second system of original Athabascan *Sb* terms, based on the parallel-cross principle instead of seniority or absolute sex; as there also is in Algonkin. The Tolowa forms of this type I have previously characterized as "evidently an idea-loan from the Yurok."² If so, Tolowa may have had the supplementary terms ready from its original Athabascan heritage.

In any event, parallel-cross *Sb* terminology recurs in Kiowa, Tanoan, and especially Keresan. Whether these influenced Southern Athabascan or were influenced by it remains to be seen. The closest linkage in this point appears to be between Chiricahua-Mescalero and Keresan.

Cousins—While Chiricahua-Mescalero distinguish only parallel *Sb* and cross *Sb*, they do not distinguish parallel cousin and cross cousin, in fact call them all *Sb*. The equation all cousins = *Sb* recurs among the Northern Athabascan groups reported on by Morgan. On the other hand, Carrier has *zit*, "♀ cousin on M side," and *unte*, "♂ cousin on M side." Of these the first corresponds to Western Apache-Navaho-Jicarilla *zede*, ♀ cross cousin, and Tolowa, Lassik, Wailaki, Kato *seti*, *tce*, *tcet*, ♀ cross cousin. Carrier *unte* also corresponds to *ontde-si*, *untu*, *un'd*, *unt* in the same four California languages, where it always denotes ♂ cross cousin and sometimes ♀ also. Western Apache-Navaho-Jicarilla *lna'ac* does not seem cognate, but has just the range of meaning of California *ontdesi-untu*.

It must be concluded that original Athabascan had a pair of terms of a type represented by *zede* and *untu*, meaning ♀ and ♂ cross cousin respectively, which have been preserved in Navaho, Jicarilla, Western Apache, but were lost in Chiricahua, Mescalero, Lipan, Kiowa Apache.

Uncles-aunts—Athabascan designations for *P Sb* are varied, and the original pattern promises to be difficult to reconstruct. Both in Northern and Southern Athabascan, *FB* = *F* and *MSs* = *M* sometimes but not always. The most consistent Southern form is *da'i* or *da'a* for *MB*, but this has no recognizable Northern or California cognates. In California the most consistent feature is the use of *oSs* for *FSs*. California throughout equates parallel *U*—*A* not with *P* but with *StP*. There is also a strong tendency toward this in Southern Athabascan. The universal stem for *StF* there is *bedje*. In Western Apache and Navaho this also denotes *FB*. In Western Apache, Navaho, Jicarilla, Lipan, Kiowa Apache, also *FSs*; *FSs* and *StF* can obviously be named alike only through the *FB*; the logical chain must run *StF* = *FB* = *FSb* = *FSs*. Yet in Southern Athabascan, the ends of this chain mostly remain equated, the links have mostly got specialized away. California appears to

² A. L. Kroeber, *Yurok and Neighboring Kin Term Systems* (University of California Publications in American Archaeology and Ethnology, Vol. 35, No. 1, 1934), p. 17.

have no cognates to *bedje*, but in Northern Athabascan Slave Lake shows *embadza* and Carrier *pizyan* for FSs. *Bedje* is therefore old Athabascan, but whether its original meaning was StF or FSs or something between, is not clear.

Chiricahua-Mescalero use of *dede* for both FB and FSs, as distinct from *bedje* StF, is therefore, within Southern Athabascan, a specialization. The stem may be the same as Slave Lake *eta* FB, Tukuthe "tye" (=tai ?) FB, Carrier *thai* FB, Kato and Hupa *tai* FB = StF. If these are true cognates, the indicated original meaning would be FB. In that event we should have this curious set of Athabascan developments for the concept of FSs:

Chiricahua-Mescalero, FSs ex FB.

Western Apache-Navaho, FSs ex FB-StF.

Jicarilla-Lipan-Kiowa Apache, FSs ex StF, FB different.

California, FSs ex oSs.

Slave Lake, Carrier, FSs ex FB-StF, as in Western Apache-Navaho.

Hare, Yellow Knife, FSs ex GM (but Northern Athabascan also tends to associate StP and GP).

StM in Southern Athabascan is throughout *ka* or *ka'a*. This is also used for MSs, regularly in Western Apache, alternatively in Mescalero and Navaho. (Jicarilla, Lipan, Kiowa Apache use *M* for MSs; but their term for *M* is *ni*, *nandi*, *nade* as against the *ma* of the other Southern tribes.) California throughout uses *unkai*, MSs and StM. Can this *unkai* be equated with Southern Athabascan *ka'a*? And in Northern Athabascan with Slave Lake *anga*, Yellow Knife *krea*, Tukuthe *kui*, Carrier *akei*, all = MSs?

The original Athabascan kinship system cannot be reconstructed, so far as it may prove definitively reconstructible, until we shall have records more accurate both phonetically and as to inclusion of meaning, from more languages, and until sound shifts have been worked out to allow the determination of true cognates. Even in the present state of knowledge, however, thanks to Opler's most welcome new Southern Athabascan data, certain salient features of original Athabascan kinship nomenclature emerge as probable. These are:

(1) Four grandparent terms. Where fewer occur, there has been reduction of terms, extension of meaning.

(2) More than two children terms, through recognition of parents' sex. On the other hand, son and daughter are sometimes merged for the same parent.

(3) Four sibling terms on the widespread American plan of older brother, older sister, younger brother, younger sister.

(4) Possibly a second set of two sibling terms, expressing parallel *vs.* cross relationship. (Cross sibling is sibling of opposite sex.)

(5) Two cross cousin terms, probably for male cross cousin and female cross cousin, though these meanings have at times been narrowed or altered.

(6) An unstable pattern of uncle-aunt designations, with however a strong tendency for the equations step-mother equals mother's sister, and step-father equals father's brother equals father's sibling equals father's sister.

The various local developments were no doubt both internal and due to contacts. America east of the Rockies favors limitation to two grandparents and two uncles-aunts (through merging of parallel uncles-aunts with parents). Some of the Northern Athabascan tribes and the easterly ones of the Southern Athabascan division show these features. California is on the whole an area of four grandparents, four uncles-aunts, four siblings, and the California Athabascans conform, even if they have to use older sister for father's sister and step-mother for mother's sister. Western Apache, Chiricahua, Mescalero, heavily self-reciprocal, are the most western and southern groups of Southern Athabascan, adjacent to Sonoran-Piman-Shoshonean and Yuman tribes, among whom reciprocal expression has its strongest development in America. Contact influences are therefore almost indubitable. Whether it was the logic of nomenclature that was diffused as such, or sets of kinship usages and institutions which were then also reflected in nomenclature, cannot be decided without much fuller knowledge and analysis. Probably both processes were operative; the problem is, at what points and to what degree. What is clear empirically or behavioristically is that *nomenclature logic has diffused*; how far this happened directly, as such, or on the contrary through the medium of social usage or institution diffusion, is something to be ascertained, not assumed. For reciprocal terminology, a corresponding institution is hard to imagine. Reciprocity seems essentially a thought-pattern.

As regards Opler's "guess" that the Chiricahua-Mescalero-Western Apache type of kinship is the older in Southern Athabascan, this is confirmed as probable at some points, rendered highly improbable at others. These three groups, which front Sonora, have certainly had their systems warped away from primitive Southern Athabascan by Uto-Aztecan and Yuman contacts; the Jicarilla, Lipan, Kiowa Apache by Plains (and Eastern) influences; the Navaho, who live essentially between the various Pueblos, by Pueblo influences, I suspect, and perhaps also by Plains in-

fluences through the Jicarilla. While guesses are in order, mine is that Navaho, though altered at a great many separate points, has been altered least consistently according to any one systematic pattern or logical plan, and may prove to preserve a greater number of features of original Southern Athabascan than any one Apache group.

Methodologically, it is clear that, because kinship systems are sets of words, we are neglecting extremely pertinent evidence when we do not use comparative philological findings. Technically rude as Morgan's and Gifford's data from the North and California are philologically, they throw genuine beams of illumination into the South Athabascan situation, and show that Opler's excellent typological classification cannot in the main be read historically. Wherever we are dealing with members of a larger indubitable or close-knit speech family—Uto-Aztecan, Siouan, Algonkin, Muskogean, Salish, Eskimo—the same must apply. It seems sterile to grope for understanding of why a particular system is what it is, while philological evidence that contains at least a partial answer is not even examined. Nor is high technical competence of lifelong absorption in the study of a family of languages requisite for preliminary and orienting results. I am certainly not an Athabaskanist, and am quite unable to "prove" the cognates which I indicate; I may well have guessed a few false ones. Nevertheless, so much is patent, that while philologists will correct, they will also no doubt accept the majority of the present findings as self-evident; and I do not see how ethnographers can feel differently.

There has been, for one reason or another, enough discredit cast on historical reconstruction as such, among American and English anthropologists of recent decades, that it seems well to reemphasize that comparative philology, whether Indo-European, Sinitic, Bantu, Athabascan, or Algonkin, is in its very nature and essence a reconstructive discipline. To be sure, philologists mainly reconstruct the forms or sounds of words, and only secondarily their meanings; and we have in culture relatively little material so sharply formalized as to lend itself to comparison as exact as that of language forms. There is consequently some reason for the difference that in ethnography there is still argument whether one may legitimately reconstruct at all and that in philology the main argument is which reconstructions are the sounder. Nevertheless it is well to remember that philology in reconstructing follows techniques definitely more rigorous than most of those used in ethnography even when this is not reconstructing. The implications of this fact are too often forgotten.

At any rate, since kinship systems are first of all systems of classifica-

tory logic expressed in words which are parts of languages, the analysis and comparison of such systems without reference to their linguistic history, so far as this may be available, is an arbitrary limitation on understanding.³

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³ Since the above was written, Osgood has published Tatlit Kutchin and Kutcha Kutchin systems from the far North (Yale University Publications in Anthropology, No. 14, 1936) p. 116, Peel River; p. 133, Fort Yukon; p. 136, same inf. by Sapir, 1923. In general, these confirm Morgan. There are 4 terms for Ch, 4 for Sb, 4 for U-A StF, StM, C are not given. There is no indication of parallel-cross Sb terms. GF is tsi or chi', GM tsio or chio. The latter is evidently extended in meaning from FM, because it still has the reciprocal meaning of wn SnD also, in Tatlit GF tsi is obviously not cognate to Southern and California Athabascan FF, it may or may not be cognate to MF. Both dialects have 3 GCh terms: Tatlit, mn GCh, wn DCh and SnSn, wn SnD=GM; Kutcha, wn GCh, mn GSn, mn GD. This looks like the asymmetrically distorted remnant of a 4 GP-4 GCh reciprocal system.

Jenness has just added the Sekani terms (Bulletin, National Museum of Canada, No. 84, Anthropological Series, No. 20, 1937). There are two GP words, ase and asu, and one for GCh, asa (FL, ML, ese, esu seem related to GF, GM) F, M, Sn, D are abba, ana, se-tchwa', se-tchwe'; the two latter also mean StSn and parallel Np, and StD and parallel Nc, respectively. MB is sase, FSs abedze (see text above); FB=StF is esta, MSs=StM is s-ongwe (perhaps cognate with Californian unkai, South Athabascan ka'a, StM and MSs). MnSsCh is s-azi; for other Np-Nc relationships Ch or Sb terms are used (wn BSn=yB, wn BD=ySs). There are 4 Sb terms by age: hotige, oB; s-ade, oSs; asidle, yB; es-dje', ySs. The last 3 have general Athabascan cognates. All 4 include cousins. In addition, se-ttane means B or ♂ cousin, and se'-tise' Ss or ♀ cousin, irrespective of age or speaker's sex. There appear to be no Sekani cognates to the cross cousin terms that sometimes crop out elsewhere. The list is completed by se-naze, SnL, se-tcha, DL, klaze', SbL. As so often with new material, this list raises as many problems as it answers; especially because of uncertain cognates. The system seems simplified, perhaps mainly under Eastern influences and with emphasis on step-relationship and levirate.

THE RELATIONSHIP OF UTO-AZTECAN AND TANOAN

By B. L. WHORF
AND G. L. TRAGER

I

FOR a long time it has been thought that the Uto-Aztec and Tanoan linguistic families were related. J. P. Harrington has alluded to the relationship,¹ and Sapir includes the two families in his proposed Aztec-Tanoan, in which Tanoan is coupled with Kiowa, and Zuni is given (with a query) as a third component.²

Our purpose in the present paper is to present lexical and phonologic evidence for a rather close relation between Uto-Aztec and Tanoan, providing the basis for further comparisons with other linguistic families. We propose to adopt Sapir's designation, Aztec-Tanoan, modifying it to Azteco-Tanoan for consistency with this type of linguistic designation, but restricting it for the present to the two families here considered. This restriction is based on what appear to us to be scientifically valid reasons. For Zuni there is available no published vocabulary, and Bunzel's recently published grammar³ does not contain enough lexical material to make possible comparisons on an adequate scale; the general structure of Zuni resembles that of Tanoan, but no details of coincidence in morphemes can be cited. As for Kiowa, Harrington has stated that the relationship is close;⁴ but an examination of the "Tewa etymologies" he cites, and a comparison of that material and of the whole Kiowa vocabulary with Trager's Taos (see below), indicate only a small number of very striking resemblances, more to Taos than to Tewa, and a larger list of more distant resemblances. The file of phonetic correspondences which has been prepared for this material is such as to indicate that while Kiowa is related to Tanoan, the relationship is on a different plane from that of the Uto-Aztec. In view of certain possibilities of relationship of our Azteco-Tanoan group to other groups in several directions from it—possibilities adumbrated by significant resemblances, but not ready for publication—we prefer to leave the ques-

¹ *Vocabulary of the Kiowa Language* (Bulletin, Bureau of American Ethnology, No. 84, 1928), p. 1.

² *Central and North American Languages* (Encyclopedia Britannica, 14th ed., Vol. 5, pp. 138-41).

³ Ruth L. Bunzel, *Zuni* (New York, 1935; Extract from *Handbook of American Indian Languages*, Vol. 4).

⁴ *Vocabulary*, p. 1, note 1; *On Phonetic and Lexical Resemblances between Kiowa and Tanoan* (American Anthropologist, Vol. 12, pp. 119-23, 1910).

tion of the inclusion of Kiowa in the Azteco-Tanoan stock⁵ till another occasion.

II

The material we present is of such a nature that we believe it definitely establishes a new level of synthesis of a type which has as yet not been published for American languages. The merging of Powell's groups into larger units such as Penutian and Uto-Aztecan is a different thing; these groups were merely narrow preliminary classifications, and their merging was the result of a rather patent kinship and not such a new plane of synthesis as we posit.

The structures of the two families, Uto-Aztecan and Tanoan, though of similar type, are on the whole different; this is strikingly true of their phonetic systems, Tanoan with its glottalized and aspirated series contrasting strongly with Uto-Aztecan. But by reconstructing the ancestral forms of each family, and then by comparative methods delving still deeper into the past, we discover the common ancestor of both. The fundamental matrix of relationships is exposed, and it becomes possible for scholars to proceed on finer and finer lines in order to make historical deductions and reveal time perspectives.

By going back in time we reach an ultimate continuity of linguistic culture; the two groups are merged in one language. It is well known that such unity of language cannot result from passing contacts, but must come about from a long continuity of cultural tradition, whatever the biological relationships may have been originally. We must suppose then that the cultural ancestors of all the Uto-Aztecan and Tanoan peoples were at one time speakers of a single language which must itself have been related to other languages near or far. A perspective of vast ethnological and historical interest is thus opened.

This perspective has of necessity been formulated on the basis of scattered material, much of it poorly recorded. If well-made records, phonemically correct and morphologically adequate, of the complete vocabularies and grammatical systems of the languages under consideration were avail-

⁵ It is suggested that it is time to begin establishing a definite rank for the various terms used to describe linguistic hierarchies: "dialect" and "language" are fairly clear; "stock" is used by Americanists where others more often use "family;" both terms could perhaps be made use of, one having higher rank than the other (say "stock" higher than "family"); a term of still higher order is needed however, and it may prove convenient to borrow from biology the term "phylum;" we would have then phyla composed of stocks, these composed of families of languages, the latter divided either into dialects, or, for standard languages, into "varieties," with "local variations" as subdivisions of dialects or varieties; the terms "sub-dialect," "sub-family," "sub-stock," "sub-phylum" may also prove useful. The larger relationships of Azteco-Tanoan suggested in the text would constitute a phylum.

able, it would be possible for the linguist working with them to supply a large amount of valuable data for the culture-historian. We feel that a plea for the appearance of these desiderata before the inevitable disappearance of the languages involved is not presumptuous.

III

The Uto-Aztec material presented has been supplied by Whorf, and is based upon a large number of published and unpublished studies.⁶ The orthographies have been unified by him on a basis of phonemic interpretation wherever the evidence made this possible, and the comparisons presented are based on the principles outlined in his recent paper.⁷ A suf-

⁶ The Uto-Aztec sources are:

Aztec: Alonzo de Molina, *Vocabulario de la lengua Mexicana* (Julio Platzmann, ed., Leipzig, 1880); Rémi Seméon, *Dictionnaire de la langue Nahuatl* (Paris, 1885); Fr. Bernardino de Sahagún, *Evangelium epistolarium et lectionarium aztecum sive mexicanum* (B. Biondelli, ed., Milan, 1858); *id.*, Aztec manuscript dictionary in Newberry Library, Chicago (unpublished); B. L. Whorf, *Notes on Modern Aztec of Milpa Alta, D. F., and Tepotlán, Morelos* (unpublished).

Cora: Konrad Theodor Preuss, *Grammatik der Cora-Sprache* (International Journal of American Linguistics, Vol. 7, No. 1, 1932); *id.*, *Wörterbuch Deutsch-Cora* (International Journal of American Linguistics, Vol. 8, No. 1, 1934).

Cahuilla and Luiseño: A. L. Kroeber, *The Shoshonean Dialects of California* (Berkeley, 1907); *Notes on Shoshonean Dialects of Southern California* (Berkeley, 1909).

California Northern Paiute: Jaime de Angulo and L. S. Freeland, *Notes on the Northern Paiute of California* (Paris, 1929).

Hopi: B. L. Whorf, *Hopi Dictionary* (unpublished); Alexander Stephen, *Hopi Journal (with glossary)* (E. C. Parsons, ed., New York, 1936).

Huichol: C. Lumholtz, *Huichol Vocabulary* (unpublished, in American Museum of Natural History, New York).

Luiseño: J. P. Harrington, *Notes on the San Luiseño Language* (unpublished). See also Cahuilla and Luiseño

Northern Tepehuan: J. Alden Mason, *Northern Tepehuan notes* (unpublished).

Oregon Northern Paiute: W. L. Marsden, *Oregon Northern Paiute Texts* (unpublished).

Ópata: D. Francisco Pimentel, *Lenguas indígenas de México* (Mexico City, 1862) [with Ópata vocabulary]

Papago: Juan Dolores, *Papago Verb-Stems* (Berkeley, 1913).

Southern Paiute: E. Sapir, *Southern Paiute, a Shoshonean Language* (Boston, 1930).

Tarahumara: Wendell Bennett, *Tarahumara Dictionary* (unpublished).

Tubatulabal: Charles F. Voegelin, *Tubatulabal Grammar* (Berkeley, 1935); *Tubatulabal Dictionary* (unpublished).

Tepecano: J. Alden Mason, *Tepecano, a Piman Language of Western Mexico* (New York, 1917).

Varohío and Yaqui (also some Ópata and Tarahumara): A. L. Kroeber, *Uto-Aztec Languages of Mexico* (Berkeley, 1934).

⁷ *The Comparative Linguistics of Uto-Aztec* (American Anthropologist, Vol. 37, pp. 600-608, 1935).

ficient body of descriptive material exists for the Uto-Aztecan languages to obviate the need of any phonetic or morphological description here.

The Tanoan material is Trager's. It is based on his unpublished Taos data collected in 1935-1936, and on Harrington's Tewa and other Tanoan material published in works of which several had largely non-linguistic aims.⁵ All the material cited has been unified in orthography; this has been done chiefly by replacing symbols by direct equivalents; only where there were conclusive reasons have interpretations been introduced for phonemic accuracy (this applies in a few Tewa and Jemes cases); phonemic correctness is rather certain for the Taos data, and fairly so for the others, with the possible exception of Jemes.

A brief sketch of Tanoan phonetics and morphology seems in order. Taos appears to have preserved the old system of the languages rather well and can be taken as a type. The Taos phonemes are: p, t, c, k, k^w, ʔ, p', t', p', t', c', k', k^w', b, d, g, m, n, l, r, ɬ, s, x, x^w, h, w, y; i, e, a, o, u, ə, ɪ, ē, ā, ō, ū. The plain stops are voiceless fortis; c and c' are affricates with the sounds [ts, tʃ] and [ts', tʃ'] depending on the following vowel; ʔ is "smooth attack" initially (and is not written, for convenience in alphabetizing) and a weak glottal stop internally; p', t' are strongly aspirated; p', t', c', k', k^w', are lenis glottalized stops with weak glottalization; b, d, g are fully voiced between vowels, do not occur initially, and are voiceless non-released stops in syllable-final position; r is like Spanish short r, and is found only in Tewa, Spanish, and English loanwords; x, x^w are non-rasping, h-like sounds; w, y are semi-vowels rather than fricatives. The oral vowels vary widely ac-

⁵ The Taos data consist of a vocabulary of about 1000 items, of extensive paradigmatic material, and of a few short texts. A phonetic and phonemic presentation of one of these (now subject to correction on some points) has appeared in *Le Maître phonétique*, No. 56, pp. 59-62, October-December, 1936. Field work in the summer of 1937 is expected to make possible the preparation of a grammar and a dictionary. J. P. Harrington's *An Introductory Paper on the Tewa Language, Dialect of Taos, New Mexico* (American Anthropologist, Vol. 12, pp. 11-48, 1910) proved useful.

The Tewa is taken from Harrington's *Kiowa Vocabulary*, and from the following: W. W. Robbins, J. P. Harrington, and B. I. Ireire-Marreco, *Ethnobotany of the Tewa Indians* (Bulletin, Bureau of American Ethnology, No. 55, 1916), J. Henderson and J. P. Harrington, *Ethnobotany of the Tewa Indians* (Bulletin, Bureau of American Ethnology, No. 56, 1914). This is chiefly San Juan Tewa, but occasionally Hano and other Tewa forms are given, and a few from other Tanoan languages.

The Isleta, Isleta del Sur, Piro, San Ildefonso Tewa, and Jemes materials are from Harrington's *Notes on the Piro Language* (American Anthropologist, Vol. 11, pp. 563-95, 1909); this is a reprinting of Bartlett's Piro vocabulary of 180 words, compared with Isleta, Isleta del Sur, Taos, Jemes (Towa), and San Ildefonso (Tewa). Some Isleta forms are from unpublished field notes taken by Dr. Stanley S. Newman in 1930.

according to position: they are long in stressed syllables, short and lax when unstressed; e is always open, a is fronted and raised (French *patte*), o is typically low-back and almost entirely unrounded (American English *hot*), ə is typically mid-back-close unrounded. The nasal vowels are of the usual type, and correspond to the oral vowels, except that *ã* is not so fronted as *a*, and *õ* is slightly rounded. Unisyllabic vowel clusters of i, u, ə plus a, o occur; other "diphthongs" consist of an oral or nasal vowel plus syllable-final w and y; *îê* is also found. All syllables consist basically of CV (including vowel clusters), but the vowel of certain morphological elements may be elided under certain conditions, resulting in CVC syllables; the second consonant of these may only be b, d, g, m, n, l, w, or y (and s in borrowed words). S is infrequent, but is found in some very common words; l has been found in few native words initially, but is very common internally; the other consonants are found freely in the possible positions; a, o, ə are the most frequent vowels; nasal vowels, especially *ã*, are common. Main and secondary stress are phonemic, though in part governed by mechanical rules; there are two, possibly three, pitch stresses.

The Taos noun has absolute suffixes which classify it according to one of three noun-classes (or "genders"); each of these classes is provided with a separate but partly overlapping set of possessive prefixes indicating person (first, second, third), number (singular, dual, plural), and for the third person the noun-class, of the possessor. The noun object is usually expressed by the noun stem incorporated into the verb complex. Verbs are conjugated by means of sets of prefixes, correlated with the possessive prefixes, and indicating person and number (and noun-class for the third person) of the subject and object (direct, indirect, reflexive); there are prefixes in second position indicating interrogation, negation, and other modalities, and suffixes of tense and aspect. Particles are free (independent pronouns, adverbs) and attached (postpositions and others). Adjectival ideas are expressed by static verbs with special suffixes. Paratactic constructions are frequent, and large numbers of particles are used, but there exist several subordinating elements.

The other Tiwa languages, Picuris, Isleta, Piro, are very much like Taos; no material is available on Sandia, also in this group. Picuris, in so far as can be determined from the published material,⁹ has an almost identical consonant system, very similar vowels, and a structure of the same type even in details; Taos informants say they understand it easily. Isleta is

⁹ J. P. Harrington, *Picuris Children's Stories* (Forty-third Annual Report, Bureau of American Ethnology, 1928).

somewhat more different phonetically, though of the same morphological structure (according to Newman's notes, see footnote 8). Nothing can be gathered from the extant Piro material as to its morphology, though the phonetics seems to have been much the same as that of Isleta. Isleta del Sur seems to be almost identical with Isleta.

The Tewa dialects, which apparently differ little among themselves, are quite different from Tiwa. The initial consonants appear to be almost the same, but *s* corresponds to Taos *ʃ* and *s*, and both *x* and *k'* to Taos *x*, and there are pre-nasalized voiced stops (alternating positionally with voiced spirants); but the whole end of the words seems to have been lost by phonetic change, causing widespread nasalization of vowels; there has also been a large amount of change in the qualities of vowels. According to Harrington, Tewa also has developed significant tones (no doubt as a result of phonetic decay). What little can be found about Tewa morphology indicates a prefix system like that of Tiwa, but the suffixes have probably been entirely changed because the original ones were lost.

The only Towa dialect extant, Jemes, is still more different phonetically, showing some evidence of wholesale shifts in the manner of articulation of certain groups of sounds; it too is said to possess significant tones. Nothing can be said about its morphology. Jemes is principally useful in this paper for reconstructing certain Proto-Tanoan sounds not distinguished elsewhere.

Proto-Tanoan as reconstructed on the available evidence seems to have been on the whole much like modern Taos. The sounds were: *p*, *t*, *c*, *k*, *kʷ*, *ʔ*, *p'*, *t'*, *k'*, *kʷ'*, *p'*, *t'*, *c'*, *k'*, *kʷ'*, *b*, *d*, *g*, *gʷ*, *m*, *n*, *l*, *r*, *ʃ*, *s*, *x*, *xʷ*, *h*, *w*, *y*; *i*, *e*, *a*, *ɔ*, *o*, *u*, *ɔ̃*, *ĩ*, *ē̃*, *ā̃*, *ɔ̃*, *ū̃*, *ə̃*.¹⁰ The sound *c* seems to have given rise to both [ts] and [tʃ], but these appear to be one phoneme in all the dialects. The glottal stop may not have existed, and may have arisen independently in the separate languages. The sound *kʷ'* may not have been different from *xʷ*; i.e., one or the other of them may not have existed. The element *kʷ'* is reconstructed to take care of certain apparently related words which have *k*, *k'*, or *kʷ*, there being sometimes two forms in one language; the following vowel is usually *u* or *o*, so that what happened apparently was a loss of labilization or glottalization or both under conditions not yet determined; Picuris has *kʷ'* in one word, Sandia in two, and two Taos words were found in 1937 to have *kʷ'*, one internally and one initially; Tewa does not seem to have the phoneme; the existing cases have not been accounted for historically. Likewise *gʷ* is reconstructed for words having *kʷ* in Jemes and *w* elsewhere (also apparently *ŋʷ* sometimes in Tewa). Also *n*, *l*, *r* are re-

¹⁰ This symbol is used, because of typographical limitations, for a nasalized *a*.

constructed from the comparison of Taos and Isleta:¹¹PT n>Ta n, I n; PT l>Ta l, I r; PT r>Ta n, I r. The element x seems to be needed to explain Tewa x (and h?) corresponding to x in other languages, k' being reconstructed for Te k' corresponding to x. Of the vowels, o and ɔ, and ̄o and ̄ɔ may be merely positional variants of a single vowel in each case—o, ̄o; or ɔ, ̄ɔ may have resulted from a, ̄a. The nasalized vowels must have been separate phonemes, though some nasalized vowels seem to have arisen later in the separate languages from oral vowels after initial nasal consonants or before nasal consonants.

Morphologically Proto-Tanoan may have been like Taos, though there is no reason why the Tiwa morphology may not have grown up later, with Tewa and Towa developing along parallel but not identical lines.

IV

The Azteco-Tanoan sound system so far reconstructed appears to be complete in all important particulars. The set of reconstructed sounds and the relational formulas applying to them, form a matrix of relationships from which can be derived the thousands of exact equations—known or to be discovered—relating Uto-Aztecan and Tanoan words of common origin. For instance, the apparently unrelated facts that the words for *pine-tree* and *grandmother* in Hopi are lökō and so-, whereas in Taos they are wē- and litu-, are shown by this matrix to be regular relationships following from the scientifically valid phonetic laws, or master-equations, expressed by the matrix.

The AT sounds are shown in Table 1.

TABLE 1. AZTECO-TANOAN SOUNDS

<i>Consonants</i>						<i>Vowels</i>		
p	t	c, ʃ	k	k ^w	ʔ	i	ü	u
p'	t'	—	k'	k' ^w	—	e	ɔ	o
p'	t'	c', ʃ'	k'	k' ^w	—	a		ɑ
b	d	—	g	g ^w	—			
m	n	—	ŋ	ŋ ^w	—			
—	l(l), l	r(l)	—	—	—			
v	θ, ð	s	(xʔ)	x ^w	h			
w			y					

¹¹ The following abbreviations are used in the rest of this article: A—Aztec; AT—Azteco-Tanoan; C—Cora; Ca—Cahuilla (Kawiya); CNP—California Northern Paiute; H—Hopi; Ha—Hano; Hui—Huichol (Wichol); I—Isleta; IS—Isleta del Sur; J—Jemes; L—Luiseno (Luisenyo); NTep—Northern Tepehuan (Tepewan); ONP—Oregon Northern Paiute; Op—Ópata; Pap—Papago; Pi—Piro; Pic—Picuris; PT—Proto-Tanoan; SI—San Ildefonso Tewa; SJ—San Juan Tewa; SP—Southern Paiute; T—Tanoan; Ta—Taos; Tar—Tarahumara; Tb—Tubatulabal; Te—Tewa; Tep—Tepecano; UA—Uto-Aztecan; V—Varohío; Y—Yaqui (Yaki).

The sound *c* was probably [ts]; *ç* may have been [tš], but the symbol is purposely non-committal. The symbol *l* is arbitrary, chosen because of typographical availability, denoting what may have been a nasalized *l*. The symbol *l* means "either *l* or *r*, more evidence needed to tell which." The *x* is probable, but there is little direct evidence for it. The vowel symbols are not to be taken too literally: *a* may have been a "back *a*," or an "open *o*" ([ɔ]), or possibly an ordinary [a]; in the latter case *a* was perhaps [æ], or even [ɛ]; *ə* and *u* represent central vowels of some sort, which may or may not have been rounded.

The correspondences of the reconstructed AT sounds to reconstructed UA and PT are shown in Table 2. The numbers refer to the list of examples illustrative of these correspondences, given in section V.

TABLE 2 CORRESPONDENCES IN AZTECO-TANOAN, UTO-AZTECAN, AND PROTO-TANOAN

AT	UA	PT	Examples
p	p	p	38-40, 82-84
t	t	t	47, 48, 89-92
c	s	c	1, 2, 68
ç	c	c	4, 5, 70, 71
k	k	k	16, 17
k ^w	k ^w , k be- fore o	k ^w , k be- fore o	22, 23
ʔ	ʔ	zero or ʔ	65-67, 100-102
p'	p	p'	44, 45, 86-88
t'	t	t'	51, 52, 95
k'	k	k'	20, 21
k ^{w'}	k ^w	x ^w , k ^{w'} (?)	26, 27. See text for PT k ^{w'}
p'	p	p'	41-43, 85
t'	t	t'	49, 50, 93, 94
c'	s	c'	3, 69
ç'	c	c'	6
k'	k	k'	18, 19, 75, 76
k ^{w'}	k ^w	k ^{w'}	24, 25
-b-	zero	b	40
d	t	d	7, 8
g	k	x, y be- fore e, i; g(?)	9, 10, 11, 47, 49, 59. See text for PT g
g ^w	w	g ^w	11, 12, 72

<i>AT</i>	<i>UA</i>	<i>PT</i>	<i>Examples</i>
m	m	m	33-36, 79-81
n	n	n	37
-ŋ-	ŋ, - ⁿ	~	63
-ŋ ^w -, -ŋ ^w	-ŋ ^w , -w, - ^x	~ ^w , ~	20, 42, 48, 54, 57, 66
l(ɭ)	l	ɭ-, -l-	28 (ɭ in 7, 8)
-ɭ-, -ɭ	-ɭ-, - ^x	-l-, ~l-, ~	6, 14, 15, 26, 35, 49
-r-(ɭ), -r	-r-, - ^x	-l-, -zero	5, 13, 36, 55
-v-	v	w	4, 50, 60
θ	t, c be- fore u	s	53-55
ɬ	s	ɬ	29-32, 77, 78
s	s	s	46
x	?	x?	See text
x ^w	w	x ^w	59, 60, 98
h	h	h	13-15, 73, 74
w	w	w	56-58, 96, 97
y	y	y, zero be- fore i, e	61-64, 99
i	i, e	i	2, 5, 12, 27, 30, 31, 36, 56, 59
e	e	e	9, 12, 22, 26, 35, 49, 57, 63, 64, 66
a	a	a	6, 33, 56, 59, 61
ɑ	a	o	6, 18, 20, 23, 24, 28, 34, 38, 39, 41, 51, 62, 65
o	o	o	21, 32, 43, 44, 53
u	u	u	4, 17, 25, 45, 54, 55, 67
ü	i, e	o, u	3, 8, 15, 36, 40, 48, 50, 60
ə	u, o	i, e	1, 8, 11, 29, 42, 58
	e next to h or gut- tural	ə next to h or gut- tural	10, 13, 21, 47, 52, 53
			} See text

It is seen that UA has reduced all the stop series to one, that of unaspirated voiceless stops. The four series must be posited for AT, however, to account for the stops in PT. The defective phonemes ŋ, ŋ^w, ɭ, v are troublesome, but enough examples are in hand to assure their existence; v and b may be a single ultimate unit. The difference between l and r is not always ascertainable, and there are few examples of the latter; the examples for l show only the one initial case, the others being indistinguishable from r; UA r is distinguishable from UA l only in Luiseño and Hopi. The sound x is

expected, to fill out the symmetry of the system, but no certain examples have been found. The consonants of both the constituent families are fully accounted for, with the exception of PT k^w , g , r ; PT k^w may not have existed or may not have been different from x^w , as already noted; PT g may have arisen from g^w before originally rounded vowels, or it may be a reflex of AT g , so that PT x and g (and y before e , i) may show that PT had a sound of the γ -type; some PT x 's may proceed from an AT x ; non-initially there seem to be some cases of AT $g >$ PT zero, but the conditions are not determined. The need of reconstructing PT r has been shown above, but it has been found so far only in suffixes not exactly represented in UA, and there is no indication of its AT origin, unless it should turn out to be a conditioned form of l or n . AT n is seen to be rare initially, but is much used in suffixes and in pronominal prefixes. AT s is very rare; T s is partly from AT θ , and the common UA s of suffixes is of unknown origin. The sound b is certain only in 40, but the parallelism with d , g , g^w strongly suggests its existence elsewhere.

All the UA vowels are accounted for; but PT ɔ and the PT nasalized vowels are not shown in the table. PT ɔ may have arisen from o (as stated), or perhaps in some cases from PT a or AT a ; there is not sufficient T material to establish the rules which might govern such a development. The nasalized vowels arose in at least two ways: before the reflexes of AT η , η^w , l , and after initial nasal consonants; the development is not consistent within the limits of the available evidence, and more T material is needed to formulate the exact rules; there seems also to have been considerable independent development of nasalized vowels in all the T languages from PT non-nasalized vowels. Examples of PT nasalized vowels are to be found in numbers 14, 20, 22, 25, 26, 35, 37, 42, 48, 54, 57, 63, 64, 66, and possibly 33, 34. The T reflexes of the reconstructions numbered 81, 89, 100, 101 also have nasalized vowels. The development of AT ə is complicated: after (and also often before) a guttural in the resulting UA or PT, and next to AT h we get UA e , PT ə , otherwise UA u , o and PT i , e ; but in number 11 PT g^w must either be counted as not a guttural, or we must consider that the following y counteracted its influence. UA $e <$ AT i is perhaps secondary (2, 12), as is $u < o$ (16), and the same can be said of PT $o < u$ (14) and $u < \text{u}$ (3, 8, 48).

The structure of AT appears to be much like that of UA. Roots are of the form CV and still more often CVCV; suffixes of form CV or C may be added, and forms longer than one syllable make changes or elide the final vowel in a rather regular manner and often add suffixes to the elided form; the two consonants thus brought together seem to coalesce so that the

first is lost and the result is identical with the suffixed consonant; but where the latter is a stop it is preceded by something in the nature of an aspiration (denoted ^x) or a nasalization (ⁿ), and these "pre-affected" stops are distinguishable from ordinary stops in their reflexes. Thus the UA root *tula elides to *tul, which as a combinatory factor may be written *tu^x-; if -sa is now added, the result is *tusa, but if -ka is added the result is *tu^xka, which yields SP tuka-, whereas *tuka would have given SP tuɣa-. Tanoan reflects this same original structure, but with still more elision of vowels and disappearance of final consonants than in UA, so that the most usual stems consist of one syllable. The AT, like the UA, final vowel is normally *a mutable to *i or to a repeat of the first vowel; but sometimes it does not follow this pattern and tends to remain unchanged. Occasionally the original first vowel is assimilated to the second (hence usually to *a), and the assimilated vowel seems usually to be "ultra-short" (*ă),¹² as if the assimilation passed through the medium of a short unstressed neutral vowel.

V

The following list of examples illustrates every element in the AT matrix. The elements are arranged in ordinary alphabetical order, special characters being placed thus: c', ç, ç' after c; g^w after g; k', k', k^w, k^{w'} after k; ɭ, ɭ after ɭ; p', p' after p; t', t', θ after t; x^w after x; ? at the end. Roman characters are used for the examples, and italics for the meanings. Where no meaning is given after a cited word, it is the same as that of the reconstructed UA or PT, and where no meaning is given for the latter, it is the same as that given for the AT.

The examples numbered 1-67 are grouped into paragraphs headed by the formula governing their initial and are numbered as cited in Table 2 above. They are given in full and are taken from a total so far worked out of about 140 comparisons, of which only about ten are doubtful. Numbers 68-102, given in AT reconstruction only, indicate some other words common to the two stocks.

AT c > UA s, PT c. 1. cə *nail (finger-)* > UA su-, sũtă, sũtuⁿ:- su- > H so ki; sũtă > L -šla, C šite, A iste-; sũtuⁿ > SP šəčuⁿ-, Tb šuluⁿ-, Tar sutu, Op sutu;—PT -ci-, -ce- > Ta -ce-, I, IS, -ci-, J -sō. 2. ciya, ciy (cey) *cold* > UA se^x-, se^xpa *cold, ice* > SP šəx-, šəpi-, Tb šip-, šəb-, H sə səŋ^wa, C se', A se-, sepa-;—PT ciya > Ta cia-, I ci-im.

AT c' > UA s, PT c'. 3. c'ũ *yellow, orange, etc.* > UA siⁿ- > H siŋka- *yellow*, SP siuⁿ- *light grey*, CNP isi *grey*, Tar sita- *red*;—PT c'ũ- *yellow* > Ta c'ul-wi, I, IS c'u, SI c'e.

¹² *The Origin of Aztec TL* (American Anthropologist, Vol. 39, pp. 256-74, 1937).

AT ɕ>UA c, PT c. 4. ɕu-, ɕu-va. -vi *gather, grasp, harvest*>UA cūva, cūki, ɕuɭa; cūva>H. cōvala; cūki>A cickia, -ckia; ɕuɭa>SP cunupa- *shut a sack*, Tar ɕuluvi *wrap, wind*, Op ɕūrurai *pressing together*;—PT cuwi>Ta cuwi *gather*. 5. ɕiru *bird*>UA cīru, curu>H cīro, Tar ɕulu, Tb ɕuluš- *woodpecker*, A wi-cilin *humming-bird*;—PT cilw->ciɣw->Ta ciwyu-, J seyiw, SI ci-.

AT ɕ'>UA c, PT c'. 6. ɕ'awaɭa *squirrel*>UA cawaɭa-ŋ'e>Tb ca'wane-;—PT c'owala>Ta c'uwala-.

AT d>UA t, PT d. 7. dəɭa>daɭa, da^x-k'u *dark, darkness*>UA tuɭa, tu^x-, təɭa (and tu^ka *night* in nearly all UA languages)>SP tu^x- *black*, A ɭil-, ɭila (<təɭa) *black*. Tb tu'l *charcoal* (=tu-l-l), Op terai *burnt color*, tue *fire out*;—PT dak'u>J ⁿdahu, SI nak'ū-. 8. dəɭu *hen*>UA toɭi, tuɭi>Tar toli, A totolin, Pap ɕuɕuli;—PT dilu>I dirun, Ta lilu-na.

AT g>UA k, PT x, except next to PT e, i>PT y, when initial iy. 9. gena, gen *foot*>UA kena, keⁿ->H kə'kə, and *kəna>kənta *kick*, CNP kəkə, Tb əŋgə-, Hui kiata, Op ke *tread*;—PT iyen>Ta iēn-, Te ʔəŋ. 10. galo, gaɭo *circle*>UA keɭo>koɭo>A koloa, Tar kuli *twist*, H kōŋo *ball*;—PT xali>Ta xəli-.

AT g^w>UA w, PT g^w. 11. g^wəŋeŋ^w *pine*>UA wokoⁿ->Tb wohoⁿ-, H lökö, L wexe^o-tu-t, SP oŋoⁿ-, Tar oko-, Op gok, C huku, A oko-;—PT (g^wiē>) g^wē>Ta wē-, J k^weniš, Te ŋ^wēj. 12. g^wine *stand*>UA (wine>) wēne, wiⁿ->H wānə, Tb əwən-, SP wənə-, Tar wili, Op g^we, C wiše, Tep gī;—PT g^wine>Ta winē, I wi, IS wi, Pir -wien, J k^wi.

AT h>UA h, PT h. 13. həwora *open, make hole or cavity*>UA hora, ho^x-, ho^xta>H horo *be stuck through*, hōta *open*, h^hci *hole*, SP ora- *dig*, o^x- *hole*, L hedi (<*ho^xti), Tar holi;—PT həwol, həw *gulch, arroyo*>Ta həol-, SI hu^ou, he^oe. 14. huɭa, huɭ *cedar*>SP əna- *cedar-like tree*, H ho^x- *cedar*;—PT hū>Ta hū-, Te hū. 15. huɭa, huɭ *breathe*>UA hi^x-, hi^k*i>Tb ixki-, H hi^k*is-, hik^wsi, Tar iwi, Pap i'bə, A iiyō- (<*hihi-);—PT hōla, hō>Ta hōla, Te hā-.

AT k>UA k, PT k. 16. ko^ou *buffalo*>UA ku-, kucu>SP ku^uu-, CNP kucu;—PT ko^oo-n>Ta kon-, SI kō, SJ kō^oŋ. 17. kowa, kow *plant, tree*>UA kuwa, ku^x-, k^wa *tree, wood*>H ko, k^wə-, Tar guš *stick*, Op kuh-t, C ki, A k^wa-wi *tree*, kili *plant*;—PT ko *plant*>Ta ko-, Te ka *leaf*, kōŋ *inflorescence*.

AT k'>UA k, PT k'. 18. k'ə *rest in place, sit, lie*>UA ka, kâte, kaci>Tb hal-, H kätə *sit*, kaci *lie*, SP karə-, C ka, A ka *be*;—PT k'o, k'o^o>Ta k'uo *lie*, Te k'o *lie*, k'u^ou *put*. 19. k'əwo, k'əwora *neck*>UA kura, ku^x->L qara, Ca -qil, Tb kula-, SP kura-, Tar gu'ta, C kipi, A keč-;—PT k'əwo>Ta k'əo-, I, IS k'oa, SI k'e.

AT k'>UA k, PT k'. 20. k'əŋ^w *leg*>UA kă^x- (-si or -pe-si) *leg, thigh*>H

kāsi, Tb hapši-, Tar gasi-, A ikši- *foot, leg*, Pap kahyo;—PT k'ōw, k'ū>Ta xū-, J hō, SI k'ū. 21. k'ə'o, k'ə'o *corn*>UA (ke'o>) kā'o>H kā'ö, SP ka'o, Op kāwotü *pluck corn*, A ka'l- *roasted corn*;—PT k'ə *grain of corn, seed*>Ta xə-, SJ k'e, Ha k'ili.

AT k'>UA k', PT k'. 22. k'ēja, k'ej *hard, hard material*>UA k'ija, k'ia-, k'e- >H k'igavi *oak*, L k'i-la *oak*, SP k'iya- *scrub oak*, A k'ečo *compress*, k'ešaš- *leather*;—PT k'ē *hard, metal, iron*>Ta k'ē-, I, SI, SJ k'ē-.

AT k'ə>UA k'a or ko, PT ko. 23. k'ə *wild canine*>UA k'a-, ko->H k'ε'w *wolf*, A koyo- *coyote*, NTep bami *coyote*;—PT ko-l *wolf*>Ta kol-, I kar-.

AT k'>UA k', PT k'. 24. k'ə *eat*>UA k'a, ko>C k'a, A k'a, Op g'a, Tar go-, goa, Tb welε'h- *swallow*, Pap bah *swallow*;—PT k'ō->Ta k'olā *eat*, kole'ne *food*, Te ko *eaten*. 25. k'ux'iw *chipmunk*>UA (kuwiw>) ku->H ko'na, CNP kooce *gopher*;—PT k'ux'iw>Ta k'uox'iw-, Te kuwiye.

AT k'>UA k', PT x'. 26. k'eja, k'eja *dragging, tail*>UA (k'eja, k'āja>) k'ex-, k'ā- *dragging*, k'asi *tail*>Tb wə'gi'n- *drag*, Op g'ito *limp*, A k'in- *lame*, C k'anaše *be tired*, SP k'asi- *tail*, Tar wasi *tail*, C k'asi *tail*, Pap bahi *tail*;—PT x'ē *tail*>Ta x'ē-, I h'wi, Te x'ē-. 27. k'iya *take, get, pick up*>UA k'iyi, k'ex-, k'ēsa>A k'wi, C č'wi, Tb wiš-, wək-, SP k'wəə-, H k'wəə, Tar wi *harvest*;—PT x'iya>Ta x'ia.

AT l>UA l, PT l, initially l. 28. ləwa, ləw *mouth*>UA lāwa, lēw *speak, say*>L alvo-, Tb ala'w-, H lava'yi, Tar neo-, C niu, A ilwia, Tep nio;—PT lō- *mouth*>Ta lō-, I, IS lā-, J tye-, SI so.

AT l>UA s, PT l. 29. lə *grandmother*>UA su->H so-, Tar usu-, Op su-, A si'-, Pap hu-;—PT li->Ta li-tu *mother's mother*. 30. li *flower, herb, grass*>UA si, siwa>H si-, SP šəə, Tar siwa-, C šušu (<šiw), A šiwi-, šöči-, Tep hioši-;—PT li>Ta li-, I, IS li, J tyu-. 31. liwa *woman*>UA siwa, siw-, su->A siwa-, sowa-, L šuḡa, Ca suḡama *daughter*, H siwa *younger sister*;—PT liw->Ta liw-, I liu-, IS liu, J tyo. 32. lō *tree, wood*>UA so, söho>H söhö'vi *tree, cottonwood*, CNP sobi- *cottonwood*, C su *oak*, A šome- *elder tree*;—PT lō *wood*>Ta lō-, I, IS lā, J tyiš.

AT m>UA m, PT m. 33. ma *hand*>UA ma in nearly all languages, and ma- *action with the hand* in all known;—PT ma-n>Ta m̄n- I, IS man, J m̄te, SI m̄ḡ. 34. m̄awa, m̄aw *see*>UA mawa, ma', mawi, mai'->Op m̄awa *eye*, C mua *know*, Tb ma'g- *know*, H ma'ci *be visible*, ma'ta *show*, Tar ma'ci *be visible, know*, A mati *know*, SP mai'- *find*;—PT mow>mu>Ta m̄, I, IS, J m̄, SI muu. 35. meja, meḡ *go, walk*>UA meja, me', meya>L mona, CNP mia, SP mia-, Tb miy-, ma'ig-, Tar ma, C me, A mika, Tep m̄ar *run*;—PT mē>Ta mē, Te mē, I m̄i *come*. 36. m̄uri, m̄u'm̄uri

turn > UA *meri*, *mera* > H *məri twist, curl*, L *morwapiš- firedrill*, A *malina turn*, *malaka- spindle*, *mamalwas- firedrill*;—PT *mowoli* > Ta *muoli return*.

AT n > UA n, PT n. 37. *ne'a*, *ne'* I > UA *ne'e*, *ne'*, *ne*, in nearly all UA languages;—PT *nā* > Ta, I, IS, SI, J *nā*.

AT p > UA p, PT p. 38. *pā older brother* > UA *pa*, *pa-va* (= *pa-pa*), *pa-ci* > H *pa'va*, CNP *pabi*, Tb *paži-*, Tar *vači*, Op *vā*, C *haci*, A *ačka-*;—PT *po*, *po-po* > Ta *popo-*, I, IS *papa-*, Pi *-pupu-*, SI *pare*, J *pap-ā*. The Zuni word is *papa*. 39. *pāhi*, *pāhi-wa*, *pai three* > UA *pahi*, *pahiw*, *pai* in all UA languages, e.g., H *pahiw*, *pay-*;—PT *poyuwo* > Ta *po'yuo*, IS *pačua*, I *pačo*, SI *poye*. 40. *pūbi flower* > UA *pi* > Tb *ibi- flower*, *ibi'- to bloom*, A *ic-*, *ic-molini to bud*;—PT *pobi* > Ta *pobe-*, Pic *pom-*, Te *poβi*. See above for AT b.

AT p' > UA p, PT p'. 41. *p'a water* > UA *pa* in nearly all UA languages (*pa- pertaining to water* in all);—PT *p'o* > Ta *p'o-*, I, IS, J *p'a*, SI *p'o*. 42. *p'a'a*, *p'a'a-ŋw road* > UA *poe* > *powe*, *po^x-* > Tb *poh^x-*, L *pe^x-*, H *pö^x-*, SP *po'-*, Tar *vowe-*, Op *vowe*, C *huye*, A *owi*, *o'-*;—PT *p'iyē* > Ta *p'iē-*. 43. *p'o younger brother* > UA *po*, *poni*, *poci* > Tar *voni*, V *poci*, Op *vo-*, C *hu*, *huci*;—PT *p'o(-y-)* > Ta *p'ōy-*, I *p'ai-*, J *pe-tue* (for *p'e-re?*).

AT p' > UA p, PT p'. 44. *p'o*, *p'oho hair* > UA *pōho*, *po^x-* > H *pōhō down*, Tb *po'ŋ- cut hair*, *po'h^x- body hair*, Op *powa-t hat*;—PT *p'o-* > Ta *p'o-*, I, IS *p'a*, J *fwola*, SJ *p'o hairy*. 45. *p'u blow* > UA *pu-*, *puya*, *puca* > H *povoya* (< **puya*), Tb *pušk-*, C *hice*, A *pica*;—PT *p'u*, *p'uci* > Ta *p'uci*, I *p'u*, Te *fe-re* (a writing for *p'e-re?*).

AT s > UA s, PT s. 46. *se'a jay* > UA *sā'a* (by assimilation) > H *sā'a*,—PT *se* > Te *se bluejay*. But if the Te word is cognate with Ta *su'-le'na bluebird*, as is possible since Te *e* may correspond to Ta *u*, then the PT form must have been **su*- < **sowa-*, and the AT could only have been **sü'a*: cf. no. 55.

AT t > UA t, PT t. 47. *təgə deer* > UA *teke* > SP *təγəə-*, CNP *təhəč*, ONP *təhəca*, Tb *tohi-* (= a L form, loan?);—PT *tə(x) elk* > Ta *tə-*, Te *ta*. 48. *tūŋ'a*, *tūŋw speak* > UA *tiŋ'a*, *teŋ'a*, *teŋ-* > Tb *tiŋ'a- name*, H *təŋ'a to name*, Tar *qewe be named*, SP *tənia- tell*, A *tenewa tell*;—PT *tōw* > *tū* > Ta, I, IS, SI *tū*.

AT t' > UA t, PT t'. 49. *t'ega*, *t'egela*, *t'egel cut* > UA *tēka*, *tekaḷa*, *tēke^x-* > Tb *təha*, H *təko*, *təkə^x-*, SP *təyani-*, CNP *cəaw*, A *teki*;—PT *t'eyē* > *t'ē* > Ta *t'ē*. 50. *t'üva*, *t'üva-l nut*, *pine-nut* > UA *tēva^x-* > SP *təva^x-*, H *tēva*, Tb *təbə^x-*;—PT *t'ow* > Ta *t'ow-*, Te *t'o*.

AT t' > UA t, PT t'. 51. *t'āwa*, *t'āw sun, day* > UA *tawa*, *ta^x-*, *ta-* > H *ta'la*, *ta'wa*, Tb *ta-*, SP *ta-*, *tava*, Tar *qawe day*, A *ḡawia shine*, *ḡap-dawn* (< **ta^xpa*);—PT *t'ow-* > *t'u-*, *t'u-le* > Ta *t'u-le'na*, I, IS *t'uri-*, and

probably Te t'āṇ. 52. t'əho *flour, grind* > UA (t'əo >) tu-, tusi, tusu > Tb tu'ir-, u'd-, tuš-, H tosi, SP tušu-, Tar ɬusi-, Op tüh, C tiši, A te'si;—PT t'əo > Ta t'əo- *flour*.

AT θ > UA c before u and t elsewhere, PT s. 53. θəho *man, person* > UA teho, te- *man*, tiyu, tu *boy* > Tar ɬehoi *man*, H tə-wat *someone*, tiyo *boy*, A te- *someone*, Tb tu'ɪlam *boy*, C tiri *child* (< *tiyu);—PT səo-ne *man* > Ta səo'nena, I, IS səoni-, J šol, SI sē. 54. θuŋ^w *water* > UA cu^x-, cuya, cu^xta > Pap šu'daki, Tep su'di, Op cütutai *drip*, A čipi-, čito- *drip*;—PT sū(w) *drink* > Ta, I, IS sū, SI sūŋ^we, J šyū. 55. θurə *bluebird* > UA curu > H co'ro;—PT sule > Ta su'le'na; but cf. no. 46.

AT w > UA w, PT w; in PT this w non-initially and that arising as a glide between vowels combines with the preceding vowel, but w proceeding from AT v remains (cf. 4, 50). 56. wa^xk'i *be dry, wasted, thin* > UA wa^xki > Tb wa-g-, wa-gi[?]-, H la'ki, Tar waki, Op g^wai, A wa'ki;—PT wok'i > Ta wok'i *thin* (but if this is merely wo *not* + k'i-mā *thick*, then the AT reconstruction must be discarded). 57. weŋ^we, weŋ^w *comb, brush* > UA weŋ^we, weⁿ-, wesi > Tb wəŋə^x-, H wəsi, SP wəšia- *feather*;—PT wē- > Ta wē-t-. 58. wə, wəye *two* > UA wo, woye > Tb wo-, L we-, H lö-, löye, Y g^woi, Tar o-ka, Op go-, gode, Hui hotat, A o-me[?];—PT wi, wiyi > Ta wi-, wi'ina, I, IS wisi, J wiš, SI wiye.

AT x^w > UA w, PT x^w. 59. x^wiga *hoe* > UA wika > H wika, A wik-, Tar wika- *planting stick*;—PT x^wiya > Ta x^wia-d-. 60. x^wüva *to whip, beat* > UA wēva, wi^x- > H wəva, wi'pi, Tb wup, wuba-, Tar wipi-, C ve, A wewe- *drum*, witeki *beat, whip*, Pap gəgə;—PT x^wow > Ta x^wönā *beat*, x^wön-tu *a whip*; the nasalization of the Ta vowel and the disappearance of the PT w are not accounted for, unless that be the reflex of *ow before n. See no. 25 for internal x^w.

AT y > UA y, PT y (zero before e or i). 61. ya^x, ya^xpewi *sleep* > UA ya^x-, yä^xpewi > Tb yahta-muŋ-, SP apəi-, H (*ypəwi >) pə'wi;—PT ya, yapiw > Ta yiapiw-, I, IS ya, SI yo-k'o. 62. ya *sing* > UA ya- > Tb ya'n-, SP yaŋa- *yell*, C raxna *hum*; cf. UA yaⁿ-pa *mocking-bird* (H, SP);—PT yo- > Ta yo-t'o (t'o *do, make*, i.e., *make a song, a singing*), J ɬa(?). 63. yena, yeⁿ- *go, run* > UA yena, yeⁿ-, yewa > SP yuni- (< yəni), H yə'tə, Tar eye'na, Op de, C ri, rix, Tb ɔ'yəw- *stray*, A yewa *rush*;—PT ē *come* > Ta ē, Te ʔe' (?), I, IS, J i. 64. yeŋa, yeŋ *sit* > UA yāŋ- > yānsa > Tb yanz-, H ye'sə, Y yesa, Tar asi, C ras, Tep dah;—PT ē > Ta ē, I i, SI ʔē, SJ ʔēŋ.

AT ʔ > UA ʔ, PT zero (or ʔ). 65. ʔa *bathe, wash* > UA ʔaɬa, ʔa^x-, ʔa^xki, ʔasi > H ʔa'si, L a'ši, Op ahra *rain*, Tar aake *swim* (< *ʔa^xki), A altia *bathe*, aaki *swim*;—PT o > Ta o *wash*. 66. ʔeme, eŋ^we, eŋ^w *you* (sg. and pl.) > UA eme, eŋ^we > forms in most UA languages (these are not cited because they

are complicated by grammatical additions);—PT ē(w) > Ta ē, I, IS i, SI ū'ū. 67. 'uhu *child* > UA 'u-, hu- > CNP oha'a, Tar u'ta, Op ussi, Hui ui-mali *young girl*, H -hoya *little*;—PT uu-, u- *child, little* > Ta u-, I uu-, SJ 'e'.

68. ciga *cut*. 69. c'ewa *spruce*. 70. çila *see*. 71. çiyu *dog*. 72. g'w'ya *sharp point*. 73. hi *what*. 74. hāra *go*. 75. k'ə'a *point, tooth*. 76. k'āwa *good*. 77. lə'a *boil*. 78. lōwa *stop, end*. 79. meka *give*. 80. mura *roll*. 81. musa *cat, feline animal*. 82. paguyu *fish*. 83. pəwa *die, cease*. 84. piya'li *heart*. 85. p'ya *peak, mountain*. 86. p'a *fire*. 87. p'ə'la *wrap, tie*. 88. p'ə'a *hold*. 89. taŋ'wa *man, father*. 90. toŋ'i *winter*. 91. tu *tree*. 92. tü *buy*. 93. t'ə *strike*. 94. t'ūna *antelope*. 95. t'ewa *see, find*. 96. wə *plant, stem*. 97. wəhəra *flee*. 98. x'ūla *shake, wave*. 99. ya *go, carry*. 100. 'a'leŋ'w *ant*. 101. 'eŋ'wa, 'e'la *blood*. 102. 'i'a *this*.

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LINGUISTIC DISTRIBUTIONS AND POLITICAL
GROUPS OF THE GREAT BASIN
SHOSHONEANS

By JULIAN H. STEWARD

IN 1925, Kroeber¹ indicated the general distribution of the divisions of the Shoshonean family, but was handicapped by lack of detailed information from the Great Basin of Nevada, Idaho, and Utah. The nature and location of the political divisions of these groups, moreover, was imperfectly known except in a few, generally marginal regions,² and nothing had been recorded concerning the Shoshoni and their immediate neighbors occupying the central portion of the area. Present data,³ however, make it possible to bound the main linguistic divisions of the Great Basin with some accuracy and to define and locate most of the political divisions of the Shoshoni and their neighbors.

LINGUISTIC DISTRIBUTIONS

The area is occupied by three divisions of the Shoshonean linguistic family:

1. Northern Paiute, Kroeber's Mono-Paviotso division,⁴ extends from eastern California in Owens Valley (formerly called Eastern Mono), northward through western Nevada and northeastern California into southern and eastern Oregon to an undetermined distance. Lewis and Clark reported "Snakes" (probably Northern Paiute) on the Deschutes River⁵ and Ogden reported "Snakes" (again, probably Northern Paiute) on the John Day

¹ A. L. Kroeber, *Handbook of the Indians of California* (Bulletin, Bureau of American Ethnology, No. 78, 1925), fig. 52.

² R. H. Lowie, *The Northern Shoshone* (Anthropological Papers, American Museum of Natural History, Vol. 2, pp. 169-306, 1909); *idem*, *Notes on Shoshonean Ethnography* (same series, Vol. 20, pp. 191-314, 1924); J. H. Steward, *Ethnography of the Owens Valley Paiute* (University of California Publications in American Archaeology and Ethnology, Vol. 33, pp. 233-338, 1933); Isabel T. Kelly, *Ethnography of the Surprise Valley Paiute* (same series, Vol. 31, pp. 67-209, 1932); *idem*, *Southern Paiute Bands* (American Anthropologist, Vol. 36, pp. 548-60, 1934).

³ These data were gathered during 1935 on a trip financed by the University of California and a grant-in-aid from the Social Science Research Council and during 1936 on a trip for the Bureau of American Ethnology. Identity of linguistic groups was established by forty test vocabularies of one hundred words each, procured from Shoshoni of California, Nevada, Utah, and Idaho, and from Northern Paiute, Bannock, Gosiute, Ute, and Southern Paiute.

⁴ A. L. Kroeber, *Shoshonean Dialects of California* (University of California Publications in American Archaeology and Ethnology, Vol. 4, pp. 66-165, 1907).

⁵ R. G. Thwaites, ed., *Original Journals of the Lewis and Clark Expedition, 1804-1806* (New York, 1905), Vol. 3, pp. 147-49.

River and in the vicinity of Malheur Lake.⁶ The language is very similar throughout this entire area, varying from locality to locality only in minor features. Dialectic divisions do not appear to coincide with political divisions.

The tradition that a non-Paiute people (Pit River?) inhabited the vicinity of Lovelock, Nevada, about three generations ago was reported by a Northern Paiute informant from near Winnemucca.⁷

A vocabulary from the Bannock, who occupied the Snake River in the vicinity of Fort Hall jointly with Shoshoni, is almost identical with those obtained from Northern Paiute.

2. The Shoshoni, Kroeber's Shoshoni-Comanche division, extended from the region of Little Lake and Death Valley in California, northward across most of the eastern half of Nevada into northern Utah, southern and eastern Idaho, a little of adjoining Montana and western Wyoming. The vocabularies show that the language is substantially the same throughout this area, varying, like Northern Paiute, only slightly from one locality to another. It differs, moreover, but little from Comanche which was claimed by most informants who had heard it to be intelligible to them.

The peoples near Death Valley designated by Kroeber as Koso⁸ speak the same language as the Nevada Shoshoni. Panamint Valley and probably the southern portion of Death Valley, however, were occupied by Kawaiisu, who were mixed with Shoshoni in the northern ends of each valley.⁹ Kawaiisu is of the Ute-Chemehuevi division.

It is probable that peoples living just south of Little Lake and called Wavits ("mean") by Shoshoni were Tubatulabal who had thrust a short distance into the desert. No informant of this group, however, could be found.

Shoshoni occupied Nevada as far west as Columbus Salt Marsh, Ione

⁶ T. C. Elliott, *The Peter Skeene Ogden Journals* (Quarterly, Oregon Historical Society, Vol. 10, 1909), trip of 1826, p. 349; (*ibid.*, Vol. 11, 1910), trip of 1826-1827, p. 208.

⁷ L. L. Loud and M. R. Harrington (*Lovelock Cave*, University of California Publications in American Archaeology and Ethnology, Vol. 25, pp. 1-183, 1929) give several versions of this tradition (pp. 165-69).

⁸ *Handbook*, pl. 1 and pp. 589-92.

⁹ The Panamint Valley Kawaiisu were called Mũgũnũwũ by Shoshoni. Two vocabularies were obtained from Tom Spratt, one-quarter Shoshoni, one-quarter Kawaiisu, one-half White. His "Shoshoni" vocabulary is very close to Kroeber's Shikaviyam or Koso (*Shoshonean Dialects*, pp. 71-89) and both are the same as my various Shoshoni vocabularies. Tom Spratt's Mũgunuwũ vocabulary is almost identical with Kroeber's Kawaiisu (*ibid.*) and was said to have been spoken also in the South Fork (Kings River?) Valley, in Kelsey Valley, and in the Tehachapi region, California.

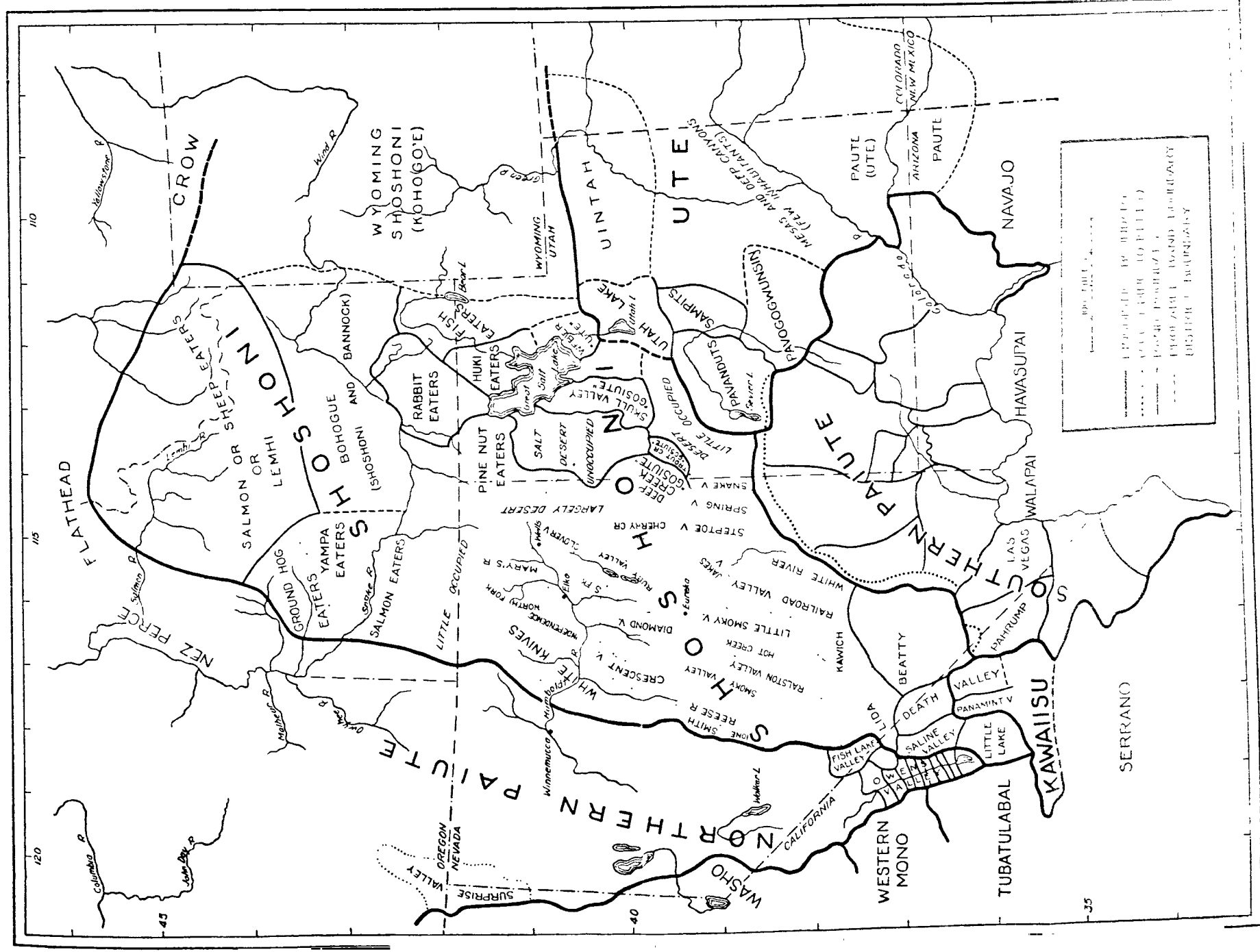


FIG. 1. Distribution of Shoshonean groups in the Great Basin.

Valley, Smith Creek Valley, Reese River Valley, and Battle Mountain. The high region in the vicinity of Owyhee on the Nevada-Idaho border was not permanently settled but was visited in summer by Humboldt and Snake River Shoshoni and by a few Northern Paiute. Shoshoni occupied the Snake River in Idaho to the vicinity of Boise, where they mixed with Northern Paiute and sometimes with visiting Nez Percé. They were also in the mountainous portion of the upper Salmon River and in a small strip of Montana across the continental divide. The Wyoming Shoshoni cannot be definitely bounded because their somewhat nomadic habits and their clashes with Crow and Blackfoot made their location somewhat unstable. The Shoshoni of eastern Idaho and Utah and of Wyoming, especially the bands which possessed horses at an early date, were often called Snake, though this name has also loosely been applied to other Shoshoni and sometimes to Oregon Paiute.

On the east, the Shoshoni adjoined Southern Paiute in southern Nevada, where the boundary according to my informants differed but slightly from that given by Kelly.¹⁰ North of this, they abutted Ute in the Sevier Desert of Utah, at Utah Lake, and, in northeastern Utah, were separated from them by the Uintah Mountains, which run east and west. Shoshoni on the southeastern edge of Great Salt Lake were called "Weber Ute," though the language is similar to that of other Shoshoni. Vocabularies from the Gosiute, who were somewhat isolated in the vast deserts lying south and southwest of Great Salt Lake, are also Shoshoni. Their distinctiveness seems to have resulted from their isolation, and their extreme cultural poverty, but their language is in no way unique. Gosiute is from gossip: (dust, from the alkali flats) + Ute.¹¹

3. Southern Paiute belong to Kroeber's Ute-Chemehuevi division. Their location on the accompanying map is taken from Kelly,¹² though the Las Vegas "band" is subdivided according to my own information.

The Ute, belonging also to Kroeber's Ute-Chemehuevi division, occupied eastern Utah, Colorado west of the Rocky Mountains, and, according to Southern Ute informants, traveled seasonally by horse to the east of the Rocky Mountains. They were also formerly in a small portion of northeastern Arizona that is now occupied by Navajo and in a part of northern New Mexico.

¹⁰ *Southern Paiute Bands*.

¹¹ Informants could offer no explanation of the designation Ute for these people. All Shoshoni call themselves num' or num'. The origin of Shoshoni is unknown though a plausible guess derives it from so (much) sonip (grass), referring to the covering of house floors.

¹² *Southern Paiute Bands*.

POLITICAL GROUPS

It is not wholly revealing to record merely that a group had a chief or considered itself a band, for neither the nature and extent of the authority delegated to the chief nor the kind of solidarity among members of the band is self-evident. Moreover, novel conditions and concepts introduced by the white man often radically altered native groupings, bringing solidarity and chieftain's authority where it had not previously existed.

A definition of Shoshonean groups in terms of those economic, social, and religious activities which produce group cohesion and of the political control required for those activities, places some "bands" in a new light and demonstrates that there were at least two very unlike types of political groups in the area: (1) village organization, in which habitual association and cooperation was limited to the inhabitants of a single village; (2) band organization, variable in its social and economic foundation, but always entailing cooperation, some centralized political control, and a sense of solidarity among inhabitants of a well-defined territory.

Village organizations occurred among Shoshoni of Nevada, western Idaho, western and northwestern Utah, and probably among many Northern Paiute and Southern Paiute. There is reason to suspect that prior to the introduction of the horse, it may have occurred among some of the eastern Idaho and Utah Shoshoni and among some of the Ute.

Among the Nevada Shoshoni, restriction of political organization to the village is a function of social and economic activities. These Shoshoni were primarily gatherers. Their habitat is a high, semi-arid steppe, which consists of a monotonous succession of long, sage-covered valleys separated by lofty mountain ranges which run north and south. The valleys yielded only sparse crops of brush and grass seeds; the mountains, receiving greater rainfall, supported juniper and pine nut trees and various species of edible seeds and roots. Game, everywhere scarce, consisted of rabbits and antelope in the valleys, deer and mountain sheep in the mountains. The scarcity of foods and the simple devices for procuring, transporting and storing them restricted population to an average of one person to fifteen or twenty square miles. A few, exceptionally fertile localities had one person to two square miles, while abnormally arid regions, like the Great Salt Lake Desert, had one person to fifty or sixty square miles. Poor transportation facilities made it physically impossible for large aggregates of people to assemble for any considerable time. Winter villages, consequently, comprised only two to ten or fifteen families living near their food caches and ordinarily several miles from neighboring villages. From spring to fall,

individual families, or at most two or three related families, wandered together foraging for food.

It might seem that the inhabitants of each valley, which is an isolated topographic unit, would tend to associate with one another in such a manner as to form a band. As a matter of fact, they did associate sufficiently to have slight unity and each area of this kind is indicated on the map as a "district." But the unity was incomplete, people of one valley often cooperating with residents of neighboring valleys for various reasons.

Probably the most important factor bringing together people from neighboring areas was the pine nut. The pine nut, which was without question the major food, was erratic in its yield from year to year. A given locality yielded a crop only once in two, three, or four years, but when it did yield, the abundance was many times what the local population could have harvested. People having poor crops in their own region therefore travelled to places of plenty and it would have been absurd for the residents of the favored locality to repel them for poaching. There was, in fact, no concept whatever of group ownership of food territories.

The pine nut, therefore, induced a comparatively unsettled life: a family journeyed each year to areas where the crop was most convenient or the harvest most promising. Although it customarily returned to its winter home if economically feasible, it frequently found itself wintering with people from the west one year, with people from the east the following year, in widely separated localities.

Other economic and social activities failed to introduce sufficient regularity in Shoshoni associations to offset the effect of pine nut gathering. The annual communal rabbit drive, usually held in the fall, was undertaken by people who found themselves together at pine nut time and was led by the most experienced and capable person available. Likewise, the spring antelope drive brought together people who had wintered in the proximity of antelope country and was led by whatever antelope shaman was present. Dances usually accompanied these activities, but if they were held at other times, people within convenient distance assembled for a few days. There were no gatherings for purely religious purposes. The difficulties of transporting food to central locations made it impossible to maintain large gatherings for communal activities for more than a few weeks during each year. In fact, most gatherings occurred when cooperative collecting produced an abnormally large food supply for a brief period. The most stable political group among Nevada Shoshoni, therefore, was the small winter village with its somewhat shifting population and its informal headman. But even village cohesion was loose and the head man had little authority

except to arrange minor, local dances and to decide when people should go to collect seeds and pine nuts. He might direct hunts, though often a special man led rabbit drives and perhaps some other man took charge of deer or mountain sheep hunts.

The village population naturally comprised many related persons, but as circumstances of food supply, size of an individual family, choice of residence for various personal reasons, and other factors made postmarital residence variable and entailed frequent changes in residence, each village was not a single lineage. There was no rule of village exogamy.

Aggregates of people larger than the village were not only necessarily transient but, in successive years, often brought together very different families under different leaders. From southern Nevada to southern Idaho, consequently, Shoshoni society resembled a vast net, the people of each village being linked to those of villages on all sides by varied economic and social activities as well as by marriage. There were no land-owning bands, no important property rights, no exogamy other than that connected with the bilateral family.

The transformation of Shoshoni political groups wrought by the arrival of the White man contrasts sharply with the native organization. In the Humboldt River Valley, where the racial impact was most severe, the introduction of horses and other features of the White man's economy made possible the amalgamation of formerly independent villages, and warfare, which was unknown in aboriginal days, provided a motive for banding together. A loose organization developed and Tūmok, a former nonentity, became chief. When the wars were over, however, this band was dissolved, the Indians became attached to White communities, and Tūmok promptly lost all authority. Indians now speak of Tūmok as a great chief who led a large band, but careful inquiry shows clearly that his sole functions pertained to matters incident to the arrival of the White man and that so vast a band could not have existed under native conditions—a fact demonstrating the need of careful investigation of the dynamic aspects of native political institutions.

Western Idaho, though north of the habitat of the pine nut, maintained a type of Shoshoni society very similar to that of Nevada. Salmon, the principal food, was very abundant in the Snake River and supported a series of small villages which were slightly more stable than those in Nevada. Also, fish weirs and traps, used only by their builders who were members of the same village, tended to fix group ownership of fishing places. But seed areas, like those in Nevada, were free to all. The Snake River Shoshoni had no bands, for there were no factors to give cohesion to groups

larger than the village. Communal hunts were much less important than in Nevada and dances which were sometimes attended by the inhabitants of several different villages gave only a very temporary alliance.

Throughout most of this area of village organization, people were designated only as inhabitants of a named locality. In the north, however, there was some tendency to name people after a conspicuous food of their area, e.g., Salmon Eaters on the Snake River, though a given locality was often named differently by its various neighbors.

Present evidence suggests that most of the Northern Paiute had village rather than true band groups, though none have been described in terms which permit classifying them according to present definitions.

Band organizations rests upon somewhat different conditions in the western and eastern portions of the Great Basin. In Owens Valley, California, where the population was unusually dense—one person to two square miles—the terrain was divided into small areas, each owned and defended against trespass by its inhabitants. Solidarity was produced among band members by the proximity of their more or less permanent habitations and by habitual cooperation in rabbit drives, deer hunts, antelope hunts, irrigation, much seed gathering and dances. The main function of the band chief was to arrange these communal functions and to send invitations to outsiders to join. Actual direction of each activity usually fell to some person of special ability.

Shoshoni of the Death Valley and Little Lake regions had a somewhat similar organization, though the concept of band ownership of land rapidly disappears among Shoshoni. To some extent, a sense of solidarity among inhabitants of a given region may have diffused from Owens Valley. In the Death Valley region, however, habitual cooperation with one's neighbors was virtually a necessity caused by the physical impossibility of traversing the wide, waterless deserts for frequent association with other people. In like manner, some of the Gosiute Shoshoni inhabiting oases in the vast deserts south of Great Salt Lake approximated band organization.

Among eastern Shoshoneans, activities pertaining to band life usually involve the horse. There is reason to suspect that, with the exception of groups occupying country with abundant buffalo, many eastern Shoshoni were once very similar to Nevada Shoshoni, and that the early introduction of the horse brought a changed ecology which provided a basis for band organization. In 1832, Bonneville¹³ noted a contrast between the Shoshoni

¹³ Washington Irving, *The Adventures of Captain Bonneville, U.S.A., in the Rocky Mountains and the Far West* (Pawnee Edition, 2 vols., New York, 1898), Vol. 1. pp. 329-34.

above and below Twin Falls on the Snake River, expressing amazement at the impoverished and disorganized condition of the latter. Twin Fall is the eastern limit of salmon and the western limit of bottom lands where horses could be grazed. Horses were already common in the Fort Hall region at the time of Bonneville's visit. Other travelers have noted the extraordinary differences between the Ute and Gosiute. Escalante, in 1776,¹⁴ seems to have encountered horses among many Ute and definite bands and chiefs in the vicinity of Utah Lake. By the arrival of the Mormon pioneers in 1847, Ute were travelling widely over the country on horseback.

The importance of the horse in primitive economy and the consequent social and political effects should not be underestimated. The horse makes it possible either to transport food to a central point where a large population may assemble and live more or less permanently or for members of separate villages to communicate and cooperate with one another. It is an empirical fact that the western limit of the horse also was the western limit of true bands.

Bands of the eastern Shoshoneans are bilateral or composite,¹⁵ that is, consist of many families which, being unrelated, permit band endogamy. Political control is vested in one or more chiefs, certain men having special authority for warfare, hunting, dancing, and other activities. Thanks to strong Plains influence, war honors carried great prestige value and gave their possessors considerable civil as well as military authority. Although each band occupied a fairly well defined territory within which it usually ranged for food, there was little if any band ownership of territory. In fact, the great distances travelled seasonally on horseback entailed frequent association of neighboring bands (as of Idaho and Wyoming Shoshoni bands, which sometimes united temporarily), much traversing of neighbors' territories, and, indeed, invasion, even by Idaho Shoshoni and Utah Ute of buffalo country east of the Rocky Mountains, sometimes within the range of hostile tribes.

Fairly exact information is now available on the location of most of the eastern Shoshoni bands. The Salmon River Shoshoni (Salmon Eaters, Mountain Sheep Eaters, or, more commonly, Lemhi Shoshoni) were aboriginally similar to the peoples of the lower Snake River. They lived in five or more independent villages, isolated in the mountains, and became

¹⁴ *Diary and Travels of Fray Francisco Antonasio Dominguez and Fray Silvestre Velez de Escalante to Discover a Route from the Presidio of Santa Fe, New Mexico, to Monterey in Southern California* (in W. R. Harris, *The Catholic Church in Utah*, Salt Lake City, 1909, pp. 136-84).

¹⁵ In *The Economic and Social Basis of Primitive Bands* (*Essays in Anthropology in Honor of Alfred Louis Kroeber*, Berkeley, 1936, pp. 331-50) I contrasted this type with patrilineal, localized, exogamous bands.

welded into a single band only when, at the instigation of the government and after the acquisition of many horses, they settled on the Lemhi River, where a small band, possessing a few horses had previously lived.¹⁶ Even then, however, many families remained in the mountains.

The greater part of southern Idaho was occupied by the Bohogue' (bohovi, sage brush + gue', butte) band, which consisted of Northern Paiute (Bannock) and Shoshoni, wintering in the vicinity of Fort Hall, and traveling on horseback as far as Camas Prairie to the west, Wyoming to the east. A single chief, usually a Bannock, directed these movements, aided by various other men who took charge of different activities. Raids by Black-foot and some warfare with Ute further welded the unity of this band.

Other smaller, but similar bands of Shoshoni were the Rabbit Eaters (Kamu duka) of the Port Neuf River and vicinity, the Huki Eaters (Hukün duka, from hūki, a wild seed) of the Bear River, Utah, the Fish Eaters (Pajwi duka) of Cache Valley and vicinity, and the "Weber Ute" of the region of Salt Lake City. It is possible that there were other, small bands in this general area.

Wyoming Shoshoni within historic times seemed to have formed a single band under the chieftainship of Washakie, though it is probable that several distinct native bands were united when Plains warfare, which was intensified by dislocation of tribes, the introduction of fire arms and other factors incident to the coming of the White man, made amalgamation a virtual necessity.

It is now possible to map Ute bands only in central Utah and southwestern Colorado. There is little question, however, that, excepting a few small, scattered groups which were isolated in some of the inaccessible by-ways of the almost impenetrable portions of the upper Colorado plateau, the Ute ranged on horseback in strong bands. Warfare, especially with Arapaho and Crow, stimulated band growth.

Some of the Ute bands are: Utah Lake (Tūmpanagots, from tūmbi, stone, panagots, canyon mouth); Sevier Lake (Pavandüts or Pahvant Ute, "water people"); Sampits (probably named from a chief); Pavogogwunsiñ, of the upper Sevier River and Fish Lake Regions; the Uintah, of the Uintah Basin;¹⁷ the White River Utes, probably to their east in Colorado; the Uncompahgre Utes, probably to their southeast in Colorado; the Pa

¹⁶ Lewis and Clark (*op. cit.*, Vol. 2, p. 347) observed a small band with about four hundred horses on the Lemhi River.

¹⁷ *The Ashley-Smith Explorations and the Discovery of a Central Route to the Pacific, 1822-1829* (Cleveland, 1918, p. 151) records meeting Ute with horses at the mouth of the Uintah River. The Indians claimed a territory 150 miles long, 100 miles wide, the mouth of the river being its center.

Utes (water Utes) in southeastern Utah, northeastern Arizona, southwestern Colorado; the Wi'namanute, in the valleys of the Animas, Los Pinos, and Piedra Rivers in southwestern Colorado; the Kapota on the headwaters of the Rio Grande east of the last in Colorado and northern New Mexico. An amalgamation of most of the Colorado bands, including the Uncompahgres, took place under the leadership of Ouray within historic times.

The Southern Paiute must remain in some doubt until Kelly's full data are published. Although she has mapped fifteen "bands," defined as "dialectic units with political concomitants," it is not certain that a more complete definition would correspond with that used here. So long as the Southern Paiute remained on foot, it is difficult to see how people inhabiting so vast a region as that allotted to some of the bands could possibly have cooperated with one another in a sufficient number of enterprises to produce a truly centralized political control and a sense of solidarity with other occupants of the territory. Data have not been advanced to show that the bands were functional in other respects.

My own investigations among Kelly's "Las Vegas band," show that it actually comprised at least three bands of the kind defined here (so entered on map).¹⁵ These bands were not unlike those of the Shoshoni of the Death Valley region, except that the villages were given somewhat greater fixity by the practise of a small amount of horticulture. It is likely that a greater number of political units existed among pre-horse Southern Paiute than the fifteen bands recorded by Kelly and that Powell's and Ingall's list of thirty-one "tribes" may have been more nearly correct. There are indisputable records that political groups were consolidated into larger units among all other Shoshoneans after the influence of the White man was felt.

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¹⁵ Baldwin Möllhausen (*Dairy of a Journey from the Mississippi to the Coast of the Pacific*, London, 1858, Vol. 2., p. 296) noted that Southern Paiute in the Mohave desert region did not have horses in 1858.

AFRICAN GODS AND CATHOLIC SAINTS IN NEW WORLD NEGRO BELIEF¹

By MELVILLE J. HERSKOVITS

THE tendency of native peoples who have had long contact with Catholicism to achieve a syncretism between their aboriginal religious beliefs and the doctrines and rituals of the Church has received notice in the case of various folk. Best known in this connection are the Indians of Central America, Mexico, and the southwestern part of the United States, where the phenomenon has been emphasized in the literature. The somewhat more thoroughgoing assimilation of Christian and pagan beliefs which has taken place among New World Negroes has, however, gone in large measure unrecognized. In Mexico and among some Indian tribes of the Southwest, assimilation has generally taken the form of the survival of aboriginal custom in a system of belief and ritual practises the outer forms of which are predominantly Catholic. In the case of the New World Negroes who live under Catholic influence in Brazil, Cuba and Haiti, however, the exchange has been less one-sided, and the elements ancestral to the present-day organization of worship have been retained in immediately recognizable form.

This phenomenon has been studied with care in Cuba and Brazil,² and somewhat less systematically in Haiti.³ In all three countries it is marked by the following characteristics: the Negroes profess nominal Catholicism while at the same time they belong to "fetish cults" which are under the direction of priests whose functions are essentially African and whose training follows more or less well recognized channels of instruction and initiation; the ceremonialism and ideology of these "fetish cults" exhibit Catholic elements more or less prominently; and everywhere specific identifications are made between African gods and Catholic Saints.

It is the last of these characteristics that will be treated in this paper, since here can be most immediately recognized the manner in which these Negroes, in responding to the acculturative process, have succeeded in achieving, at least in their religious life, a synthesis between aboriginal African patterns and the European traditions to which they have been

¹ Presented to the Second Afro-Brazilian Congress, Bahia, Brazil, November 15-20, 1936.

² For Cuba: Fernando Ortiz, *Los Negros Brujos*; for Brazil: Nina-Rodrigues, *L'Animisme Fétichiste des Nègres de Bahia* (Bahia, 1900), Etienne Ignace, *Le fétichisme des nègres du Brésil* (Anthropos, Vol. 3, pp. 881-904, 1908), Arthur Ramos, *O Negro Brasileiro* (Rio de Janeiro, 1934), especially Chap. 5.

³ Price-Mars, *Ainsi Parla l'Oncle . . . Essais d'Ethnographie* (Port-au-Prince, 1928), and E. C. Parsons, *Spirit Cult in Hayti* (Journal, Société des Américanistes, Vol. 20, pp. 157-70, 1928).

exposed.⁴ The emphasis, as far as actual data are concerned, will be placed on information gathered in the course of field work in Haiti; but because of the resemblance between Haitian syncretization of African and Catholic gods and that found in Cuba and Brazil, the material from these countries will also be summarized to permit comparisons.

The historical background of the phenomenon is obvious, since efforts were made everywhere in the New World to convert the slaves to Christianity, and in Haiti, at least, baptism into the Catholic church was required for all those who were unloaded from the holds of the slave ships. In Cuba and Brazil, as in Haiti, the course of history has enabled Catholicism to continue to play a major role in the life of the people as their official religion. And it is this fact, together with the present day vestiges of the fear, constantly present in the minds of the Europeans during the time of slavery, that the African cults offered a focus for revolt, that explains the inferior social position held by these "fetish cults" wherever they are found. It is here also that explanation may be sought for the conditions under which African rituals are carried on, since at best they obtain but passive acquiescence on the part of the authorities and, more often, must be conducted under the greatest secrecy.

In the case of these African religious systems, handicapped by social scorn and official disapprobation, the followers are almost inevitably split into local groups, each of which is dominated by the personality of the priest whose individual powers furnish the principal drive toward any outer organization the cult-group under his charge may achieve. This in turn makes it difficult to maintain anything more than a local hierarchy of priests, and is reflected in a resulting confusion of theological concept. Hence in all these countries a general frame of reference concerning the supernatural has been handed down from Africa, and within this a variety of beliefs and modes of worship exist.

In the Haitian *vodun* cult, this takes the form of differences of opinion not alone from region to region, but within a given region even between members of the same group concerning such details of cult belief and practise as the names of deities, modes of ritual procedure, or the genealogies of the gods, to say nothing of concepts regarding the powers and attributes of the African spirits in their relation to one another and to the total pantheon. As a case in point, there may be cited the three separate lists of names of deities which were collected in Haiti from a single valley in the

⁴ In Haiti this synthesis marks practically all phases of the life of the Negro peasant; there is no reason to assume that a similar assimilation has not taken place in Cuba and Brazil. Except for folk-lore, however, all studies of Negro life in these latter countries have been almost exclusively concerned with religious practises.

interior, the valley of Mirebalais. When these three lists were compared with each other⁵ and with the published roster of names of *vodun* deities given by Dorsainvil,⁶ it was seen that while certain designations were found in all lists, there were extreme divergencies as well. Some names were present in all of them, it is true, and these represented the more important deities worshipped over the whole of Haiti, being gods derived from Dahomey and, to a lesser extent, from Nigeria and those other cultures of West Africa which have predominated in determining the form and functions of Haitian *vodun* worship. But the differences between these lists were much greater than the resemblances; and since this had to do only with names of gods, it is not strange that in identifying deities with Catholic saints, an even greater divergence of opinion was found.

Two methods were employed in the field to obtain this material. In some cases African deities were equated with Catholic saints in the course of discussions of general theological problems, or, as has been done in Brazil,⁷ invocations of songs were recorded which coupled the name of a given saint with that of its corresponding pagan god. The other means used to obtain this information was more direct. As elsewhere in the New World the imagination of the Negroes seems to have been taken by the ordinary chromolithographs found widely distributed in Catholic countries, which depict the saints and are hung in the houses of the faithful. It was possible to present a collection of these *images*, as they are termed in Haiti, to the natives and to obtain information concerning the manner in which the saints are envisaged by the people, and those *loa* or African deities they are believed to represent, by asking the necessary questions.

We may now turn to the correspondences themselves. Legba, the god who in Dahomey guards crossroads and entrances to temples, compounds, and villages, is widely worshipped in Haiti where, as in Dahomey, he must "open the path" for all other supernatural powers and hence is given the first offering in any Haitian *vodun* ceremony. Legba is believed by most persons to be the same as St. Anthony, for the reason that St. Anthony is represented on the *images* as an old man, poorly dressed, carrying a wand which supports him as he walks. Some hold that Legba is St. Peter, on the basis of the eminently logical reason that St. Peter, like Legba, is the keeper of keys and opens the door. By most persons, however, St. Peter is usually believed to be a *loa*, or *vodun* deity, without any African designa-

⁵ M. J. Herskovits, *Life in a Haitian Valley* (New York, 1937), pp. 309-19. The setting of the special traits of Haitian culture described here is to be found in this volume, Parts II and III.

⁶ J. B. Dorsainvil, *Vodun et Nèvrose* (Port-au-Prince, 1931), pp. 174-75.

⁷ E. g., Ramos, *op. cit.*, pp. 121, 125, etc.

tion, being called the *loa* St. Pierre, though this again is disputed, the *loa* St. Pierre being held by still others to constitute the spirit that validates the neolithic celts which in Haiti as in other parts of the New World and in Africa, are held sacred as "thunderstones."

Damballa, the Dahomean rainbow-serpent deity, is one of the most widely worshipped and important Haitian *vodun* gods. The question of the active existence of the serpent cult in Haiti is one which cannot be considered in this place, but to the extent that it does exist either in actuality or in the sacredness with which serpents are regarded, their worship is undoubtedly associated with this god Damballa, who also retains his aboriginal character of being the rainbow.⁸ The saint identified with Damballa is St. Patrick, on whose *image* serpents are depicted. Following this logic further, Moses is held to be the "father of Damballa" because of the miracle he performed before Pharoah when he threw down his staff on the ground and turned it into a serpent.

The Ogun *loa* include several gods who are generally regarded as brothers. Ogun Ferraille is held to be St. James, while Ogun Balandjo, a deity who gives "remedies" to cure the sick, is identified with St. Joseph because the picture of this saint shows him holding a child, his hand raised in the blessing which heals. Gran' Erzilie is by most persons believed to be Mater Dolorosa, though one informant expressed the belief that this saint is another *loa* named Erzilie Freda Dahomey. The wide-spread identification of Gran' Erzilie with Mater Dolorosa, however, is based on the attributes accorded the African goddess, since she is believed to be the richest of all the deities, so that the chromolithographic representation of Mater Dolorosa showing her as richly clothed, surrounded by many evidences of great wealth, and wearing many rings and necklaces, is quite in keeping with the wealthiness of Gran' Erzilie. The Dahomean sea god, who has retained his aboriginal function in Haiti, is equated with St. Expeditius. The *marassa*, spirits of twins, are believed to be the twin saints Cosmas and Damien, and St. Nicholas, because of the figures of children on his representations, is regarded as the "protector of the *marassa*." Simbi, who unlike the deities of predominantly Dahomean origin already mentioned is a Congo god, is believed by some to be St. Andrew, though others state that this saint is Azaka Mede, a *loa* which clearly derives its name from that of the river across which Dahomean belief holds that all dead must pass to reach the next world. One special member of the Simbi group, Simbi en Deux Eaux, is believed to be the equivalent of St. Anthony the Hermit,

⁸ Cf. Price-Mars, *op. cit.*, pp. 118 ff ; Herskovits, *op. cit.*, pp. 140-41.

Although this again is disputed by those who hold this saint is rather the one named 'Ti Jean Petro.

The Haitian, however, does not stop merely at identifying saints with African gods, for saints are occasionally themselves conceived as *loa*, or as natural phenomena such as the sun, moon, and stars, which are regarded as saints and occasionally worshipped. Thus St. Louis, the patron of the town of Mirebalais where this field work was carried on, is a *loa* in his own right. Similarly two of the kings who figure in the *image* that depicts the Adoration of the Christ Child, Balthazar and Gaspar, are also held to be *vodun* deities. La Sirène, a character derived from European mythology, is believed to be a water goddess and is identified with Nôtre Dame de Grace, while the *loa* Kpanyol, or Spanish *loa*, is equated with Nôtre Dame d'Alta Gracia.

St. John the Baptist is a powerful nature spirit worshipped as the *loa* St. Jean Baptiste, and is believed to control the thunder and lightning. The chromolithograph depicts this saint as a sweet-faced child holding a lamb, in striking contrast to the great power he is supposed to wield, and the irresponsibility that characterises his actions. Yet this identification becomes understandable when it is realized that in Dahomean mythology, which has influenced so much of Haitian belief, as in Yoruban concept, the ram is the emblem of the god of thunder; while the basis of the conception of the *loa* St. Jean Baptiste as the thunderer becomes even clearer when it is pointed out that the ram is the sacrificial animal of this *loa* in Haiti. The following myth is told of this deity:

On a given day of the year, God permits each saint to have control over the universe. St. John the Baptist, however, is so irresponsible, and his rage so violent, that God fears for the consequences were he allowed to exert his power on his day. Plying him with drink the day before, he is therefore made so drunk that when he falls asleep he does not awaken until the day after. When he is told his day has already passed, his rage is terrible, and he causes great storms to flay the earth; and it is a commonplace in Mirebalais that this day is marked by tempests of almost hurricane proportions, with great displays of thunder and lightning. Though he can do some damage, his power is now limited, however, to his own sphere.

Concerning the tendency to regard the phenomena of nature as supernatural beings we find St. Soleil (St. Sun), Ste. la Lune (St. Moon), Sts. Etoiles (Sts. Stars) and Ste. la Terre (St. Earth) among those worshipped under this category. Even the conception of a force such as the power that can bring reverses to a man may be anthropomorphized and worshipped, as the belief in the existence of a supernatural being known as St. Boule-

versé indicates. An *oraison* to this "Saint," well known throughout Haiti, reads as follows:

Saint Bouleversé, vous qui avez le pouvoir de bouleverser la terre, vous êtes un saint et moi, je suis un pêcheur, je vous invoque et vous prenez pour mon patron dès aujourd'hui. Je vous envoie chercher un tel; bouleversez sa tête, bouleversez sa mémoire, bouleversez sa pensée, bouleversez sa maison, bouleversez pour moi mes ennemis visibles et invisibles; faites éclater sur eux la foudre et la tempête.

En l'honneur du Saint Bouleversé dites trois Pater et trois Ave Maria.

Satan, je te renonce, si tu viens de la part du démon, que le démon t'emporte et te jette dans l'abîme et dans l'inférieur séjour.

Bête méchante, langue de vipère, langue pernicieuse, si tu viens de la part de Dieu pour me tromper, il faut que tu marche de terre en terre, de coin en coin, de village en village, de maison en maison, d'emplois en emplois comme le juif errant, l'insulteur de Jesus Christ.

Seigneur, mon Dieu, viens chercher à perdre un tel, afin qu'il soit disparu devant moi comme la foudre et la tempête.⁹

The data which have been sketched from Haiti will be strikingly familiar to those conversant with the literature on Cuba and Brazil, though the names of the Haitian deities will be unfamiliar to them, and the correspondences, Catholic saint for saint, and African god for god, somewhat different. Thus Legba, the Dahomean trickster held to be St. Anthony or St. Peter in Haiti, appears under his Yoruban name Elegbara, being held in Brazil to be the equivalent of the Devil, and of the Blessed Souls in Purgatory or the Anima Sola in Cuba. Shango, identified with Santa Barbara both in Brazil and Cuba, is not represented in Haiti by his Dahomean counterpart, Xevioso; it is to be remarked, however, that in Dahomey itself, among those natives of the city of Abomey who are members of the Catholic Church, this same identification is made between Xevioso and Santa Barbara. Mawu, the Great God of the Dahomeans, has not been retained in Haiti in the way in which Obatala, her Yoruban counterpart, has lived on in Brazil and Cuba, and though the Nigerian-Dahomean Ogun (designated Gu in Dahomey) has persisted in all three countries, differences are found in the saints with which he is identified in each. The table that accompanies this discussion shows in concise form the reconciliations that have been effected between gods and saints. It has been abstracted from the available literature on Brazil, Cuba, and Haiti, and in addition is supplemented by data recorded during field-work in the latter country.

⁹ This *oraison* is also given in full by Price-Mars, *op. cit.*, pp. 183-84, together with one of these popular prayers to Ste. Radegonde, regarded by the Haitians as an associate of the god who rules the cemeteries, Baron Samedi.

CORRESPONDENCE BETWEEN AFRICAN GODS AND CATHOLIC
SAINTS IN BRAZIL, CUBA, AND HAITI*

<i>African deities as found in</i>	<i>Brazil</i>	<i>Cuba</i>	<i>Haiti</i>
Batala		(O) Virgen de las Mercedes; the Most Sacred Sacrament; Christ on the Cross	
Obatala; Orisalá; Orixala (Oxalá)	(I) (N) (R) "Nosso Senhor de Bomfim" at Bahia; (N) Saint Anne; (R) "Senhor do Bomfim" at Rio ("because of the influence of Bahia")		
Grande Mambo			(M) Saint Anne
Batala			
Shango	(I) (N) (R) Santa Barbara at Bahia; (R) St. Michael the Archangel at Rio; (R) St. Jerome (the husband of Santa Barbara) at Bahia (see Yansan below)	(O) Santa Barbara	
Elegbara, Elegua, Alegua		(O) "Animas benditas del Purgatorio"; "Anima Sola"	
Legba			(M) (H) St. Anthony; (W) (H?) St. Peter
Esú	(I) (N) (R) the Devil		
Ogun	(I) (R) St. George, at Rio; (N) St. Jerome; (I) (N) (R) St. Anthony, at Bahia	(O) St. Peter	
Ogun Balandjo			(M) St. James the Elder; (H) St. Joseph
Ogun Ferraille			(H) St. James
Osun	(N) Virgin Mary; N. D. de Candeias	(O) Virgin de la Caridad del Cobre	
Yemanjá	(N) Virgin Mary, (R) N. S. de Rosario (at Bahia); N. D. de Conceição (at Rio)	(O) Virgin de Regla	

* In this table, the initials before the names of the saints indicate the sources from which the correspondences have been derived.

(H) Herskovits, field data (see also *Life in a Haitian Valley*, Ch. 14) (N) Nina-Rodrigues, *op. cit.*

(I) Ignace, *op. cit.*

(O) Ortiz, *op. cit.*

(M) Price-Mars, *op. cit.*

(P) Parsons, *op. cit.*

(S) Seabrook, W. B., *The Magic Island* (New York, 1929).

(R) Ramos, *op. cit.*

(W) Wirkus, E., and D. Taney, *The White King of la Gonave* (New York, 1931).

<i>African deities as found in</i>	<i>Brazil</i>	<i>Cuba</i>	<i>Haiti</i>
Maitresse Erzulie; Erzilie; Erzilie Fre-da Dahomey			(M) (S) the Holy Virgin; especially the Holy Virgin of the Nativity; (F) Santa Barbara (?). (H) Mater Dolorosa
Saponam	(I) the Sacred Sacrament		
Osa-Osê (Oxóssi)	(I) (N) (R) St. George, at Bahia; (R) St. Sebastian, at Rio	(O) St. Alberto; (occasionally) St. Hubert	
Oloru; Omolú	(R) St. Bento	(O) St. John the Baptist	
Agomme Tonnere			(M) St. John the Baptist
Ibeji (Brazil and Cuba), Marassa (Haiti)	(R) Sts. Cosmas and Damien		(H) Sts. Cosmas and Damien
Father of the Marassa			(H) St. Nicholas
Orumbila (Odumbila?)		(O) St. Francisco	
Loco	(R) St. Francisco		
Babayú Ayí		(O) St. Lazarus	
Ifá	(R) the Most Sacred Sacrament		
Yansan (wife of Shango)	(R) Santa Barbara (wife of St. Jerome)		
Damballa		(W) (H) St. Patrick	
Father of Damballa		(H) Moses	
Pierre d'Ambala		(M) St. Peter	
lou St. Pierre		(H) St. Peter	
Agwe		(H) St. Expeditius	
Roi d'Agoueseau		(M) St. Louis (King of France)	
Daguy Bologuay		(M) St. Joseph	
la Sirène		(M) the Assumption; (H) N. D. de Grace	
lou Christalline		(H) Ste. Philomena	
Adamisil Wedo		(H) Ste. Anne	
lou Kpanyol		(H) N. D. de Alta Gracia	
Aizan		(H) Christ (?)	
Simbi		(H) St. Andrew	
Simbi en Deax Eaux		(H) St. Anthony the Hermit	
Azaka Mede		(H) St. Andrew (?)	
Ti Jean Petro		(H) St. Anthony the Hermit(?)	

In a sense, the disparities that exist between the identifications made by the Negroes who live in different countries emphasize the theoretical importance of the materials presented in this paper. Were a given African god everywhere found to be identified with the same Catholic saint, there would be great probability that this had resulted from contacts between slaves subsequent to their arrival in the New World, and thus represented a diffusion from one country to another. As it is, there can be little question that these syncretizations have developed independently in each region where they are found. In the two lands where gods of the same African (Yoruban) tribe predominantly survive—Brazil and Cuba—distance and the absence of historic contacts of any significance make any other explanation untenable. And though Haiti is relatively close to Cuba, the fewness of the contacts between the Negroes of the two countries except in very recent times, added to the fact that in the syntheses that have been achieved in each country the gods of different African tribes figure, make the same point. Considered as a whole, therefore, the data show quite clearly to what an extent the inner logic of the aboriginal African cultures of the Negroes, when brought in contact with foreign traditions, worked out to achieve an end that, despite the handicaps of slavery, has been relatively the same wherever the forces making for change have been comparable.

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KOKOPELLI, OF THE PREHISTORIC SOUTH- WESTERN PUEBLO PANTHEON

By FLORENCE HAWLEY

THE age of the masked spirit or katchina concept among the South-western Pueblos is as difficult to determine as is the related problem of what type of spirits was believed in by the prehistoric Pueblo people.

The seventeen super-imposed layers of colored mural drawings discovered on the interior of a Pueblo IV kiva at Kuaua,¹ near Bernalillo, New Mexico, were marked as pre-Spanish by the absence of late types of Kiva Grande glaze wares within the kiva. The heads on the most prominent of these figures appear to represent the bodies of fish and suggest no parallel with known modern Pueblo religion except in reminding us of Parsons' brief note on the Tewa ceremonialists being referred to as the "fish people" but neither she nor Harrington was able to find the significance or origin of the term.²

A well known modern figure whose existence can be traced back over a thousand years is Kokopelli, the hunch-backed figure anciently depicted as playing a flute but now without his instrument. Fewkes³ gave a short description to accompany his Hopi painting of a modern Kokopelli, and in the summer of 1936 at Shipaulovi, I purchased a katchina doll representing Kokopelli, the picture and the doll being very similar in details. He has a long pointed snout, a black head with a single feather on the top, and black and white segmented circles on the side of the head. The hump is always apparent on the back. The figure is dressed in buckskin leggings and embroidered sash but with the male genitalia uncovered and prominent, an unusual feature in katchina dolls. This katchina doll and the pictured katchina of Fewkes' report are without a flute.

To my inquiries among the Hopi concerning the function of Kokopelli in religious observances, it was explained only that "he comes in [to dance] from the spring with the dancers." Fewkes was told that in olden times many of these appeared together, but he never saw one represented. Evidently this is no longer a spirit of major consequence. The appearance of the uncovered figure may have some connection with fertility, but why is Kokopelli always humped? And what was the significance of the ancient flute? Kokopelli does not appear in the modern Hopi Flute ceremony. The Kokopelli girl or mana has a slender snout similar to that of the male. Otherwise she is not distinctive from other katchina manas.

¹ To be described in a publication of the University of New Mexico

² E. C. Parsons, *The Social Organization of the Tewa of New Mexico* (Memoirs, American Anthropological Association, No. 36, 1929), p. 141.

³ J. W. Fewkes, *Hopi Katchinas* (Twenty-first Annual Report, Bureau of American Ethnology, pp. 3-126), p. 86.

The disappearance of the flute depicted in ancient times may be more apparent than actual. The ancient Kokopelli had a flute but no snout; the modern figure has a snout but no flute. Is it possible that the end-blown flute has come to be depicted as a snout? Parsons⁴ found the snout of a San Jaun katchina referred to as a "nose whistle stick." The explanation was that those katchinas "whistle this when they come." The derivation of a "nose whistle stick" from an end-blown flute would not appear difficult.

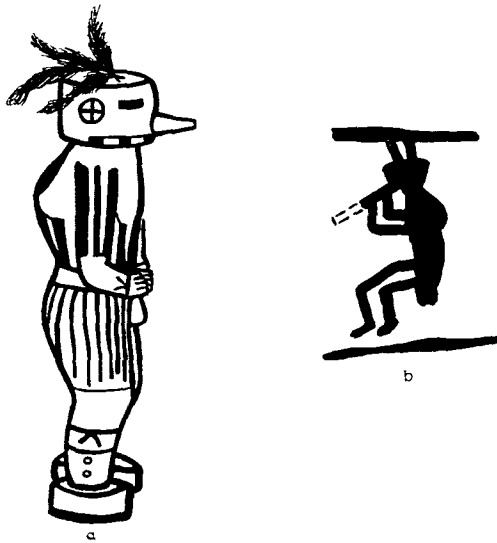


FIG. 1. Kokopelli figures. a, Modern katchina from Shipaulovi; b, Figure on a Pueblo I sherd, Mound 50, Chaco Canyon, New Mexico.

The importance of Kokopelli in the prehistoric past is indicated by numerous pictographs, such as the famous figure in Fewkes Canyon, Mesa Verde;⁵ the group of phallic figures at Flute Player House,⁶ in northeastern Arizona; the humped phallic figures playing flutes in a Pajarito Plateau cave room;⁷ in the erotic humped figures at Casas Grandes, Chihuahua;⁸ and by the figures used in some early pottery designs.

The hunch-backed flute player was found on two sherds of a Pueblo I Black-on-white vessel from Mound 50, Chaco Canyon, New Mexico, in the

⁴ E. C. Parsons, *op. cit.* p. 155.

⁵ J. W. Fewkes, *The Cliff Ruins in Fewkes Canyon, Mesa Verde National Park, Colorado* (Holmes Anniversary Volume, Washington, 1916), fig. 2 and pl. 7.

⁶ A. V. Kidder and S. J. Guernsey, *Explorations in Northeastern Arizona* (Bulletin, Bureau of American Ethnology, No. 65, 1919), p. 196, fig. 96, and pl. 93.

⁷ *Idem.* p. 196.

⁸ A. V. Kidder, *The Pottery of the Casas Grandes District, Chihuahua* (Holmes Anniversary Volume, Washington, 1916), p. 259 and pl. 3.

1936 excavations of the University of New Mexico.⁹ This mound dates at about the ninth and tenth centuries A.D. I have seen Kokopelli figures on Red-on-buff sherds of the Colonial period in the Hohokam area of southern Arizona which are probably of very similar date. Evidently this spirit or deity was popular even beyond the realms of the Pueblo area proper at this early period. In the petroglyphs in the northern Pueblo area Kokopelli is usually depicted as shooting mountain sheep, which Kidder has suggested may indicate that the supernatural Flute Players were associated with the hunt as well as with fertility.¹⁰ Kidder points out that most of the northern Pueblo Flute Players are depicted in a reclining position: no explanation for this has been hazarded. Morris took a drawing of Kokopelli from one of the rooms at Aztec.¹¹ The drawing suggests phallic symbolism. Some similar figures are found pecked into the cliffs of Chaco Canyon.

Satisfying as it is to trace this one figure back into antiquity, it leaves us with a puzzling question. If this one prehistoric deific figure was drawn, why were no others drawn on the Pueblo I pottery? Less distinctive figures might be more difficult to identify as deities or spirits, but, except in the Mimbres area, human figures on pottery are rare. Only one other figure which might possibly be conceived of as human was found on a Mound 50 sherd. This was in a scene in which a large bird reached out its long neck, apparently to pick up by the upraised hair a smaller figure which might have been animal or human, the hair suggesting the latter although the body looked more like that of a lizard.

One other hint of ancient deities came from Mound 50 in the form of a plaque of wood heavily painted on both sides in turquoise green. The shape of the plaque suggests the profile of a head, and at the appropriate place on each side is painted an oval eye, front view, in black and white. A dark line, perhaps representing hair, encircles the back of the head. This figure may have been a fetish rather than a kachina.

Little is yet known of the deific personages and of the religion of the prehistoric Southwest, but here where archaeology and ethnology meet, conscientious ethnologic interpretation of the growing collections of archaeologic finds offers some insight into a pantheon old when the first Spanish priests zealously attempted conversion of the Pueblo "heathen."

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⁹ Report on excavations in press at the University of New Mexico.

¹⁰ Kidder and Guernsey, *op. cit.*, p. 196.

¹¹ Earl Morris, *The Aztec Ruin* (Anthropological Papers, American Museum of Natural History, Vol. 26, Pt. 1, 1919), p. 92, fig. 66a.

TEMPORAL ORIENTATION IN WESTERN CIVILIZATION AND IN A PRE- LITERATE SOCIETY

By A. IRVING HALLOWELL

I

IN all human societies we find that certain classes of events have become established as formalized reference points¹ to which it is customary to relate past, present, and future occurrences or, in terms of which temporal intervals of greater or less duration may be expressed. Calendars, of course, immediately come to mind. Yet unsystematized, but no less customary points of reference such as "a sleep,"² are employed by many preliterate peoples as units in estimations of temporal length. Events in the life history of individuals—birth, marriage, or other significant occurrences—are constantly evoked to which other events may be related. Even in western civilization, despite the fact that our cultural heritage provides us with the alternative of employing exact dates for all such events, similar unformalized reference points are in use.

Whether formalized or not, the characteristic reference points employed by the individuals of different human societies are relevant to a full understanding of the functioning of temporal concepts. They are basic cultural phenomena of the utmost importance in the ordering and coordination of human activities. It is impossible to picture any human society without them. In terms of individual experience, they are orientational. The individual's temporal concepts are built up in terms of them; he gets his temporal bearings by means of them, and his temporal perceptions function under their influence. It is impossible to assume that man is born with any innate "temporal sense." His temporal concepts are always culturally constituted.

Like other cultural phenomena, temporal frames of reference vary profoundly from society to society. This fact is as important psychologically as it is culturally. Thus Dagobert Fry³ in a study of spatial and temporal concepts of the Middle Ages and the Renaissance asserts that no two peoples live conceptually in precisely the same kind of space and time. For those of us reared in contemporary western civilization

the dazzled but hospitable mind of twentieth-century man is offered a vast array of new discoveries, new theories, new intuitions having to do with the temporal in all its aspects, [for] not until the present era does there seem to have converged

¹ See M. Sherif, *The Psychology of Social Norms* (1936), Chap. 3.

² Cf. M. P. Nilsson, *Primitive Time-Reckoning* (1920), p. 15.

³ *Gotik und Renaissance als Grundlagen der Modernen Weltanschauung* (1929).

upon the problem in all its ramifications such varied and intense interest—philosophical, psychological, logical, and scientific.⁴

This paramount interest in Time is explicable on cultural-historical grounds. We moderns are habituated to a uniquely elaborated scheme of temporal norms that impinge upon our lives at every point. We not only possess a scientifically adjusted calendar, subdivided into months, weeks, days, and hours;⁵ since the middle of the fourteenth century the hours of the day have been subdivided into sixty units of equal length and these units again subdivided, a development which became of more and more practical importance as clocks and watches became common.⁶ By means of these devices, as well as a highly systematized calendrical scheme, individuals are enabled to maintain as exact a temporal orientation as is desired. And because it has become customary to "time" so many human activities and events with precision, a high level of *conscious* temporal orientation is inescapable. Consequently as Parkhurst states:⁷

That experience, that increased pervasive awareness of time as a super-sensible medium or container, as a stream, or an infinitely extended warp upon which the woof of human happenings is woven, is without question a notable characteristic of present-day consciousness.

Time extends beyond the range of our personal observation and experience, or that of the life span of any one generation of human beings. Through the device of successively numbering the years that have elapsed before and since the assumed birth of Christ, it is possible to "date" events in the remote past and to conceptualize the past history of humanity in units of comparable length (years, centuries, millennia) and likewise the history of the earth and the solar system. Not only the past, the future is likewise part of the same temporal continuum. This structuralization of future time permits the exercise of foresight by individuals, or even nations, in planning

⁴ Helen Huss Parkhurst, *The Cult of Chronology* (Essays in Honor of John Dewey, 1929), p. 293.

⁵ Despite the solar basis of our calendar, however, the temporal divisions of the day as conventionally adjusted in terms of Standard Time, adopted by U. S. railroads in 1875 (see Lewis Mumford, *Technics and Civilization*, 1934, p. 198) and Daylight Saving Time transcend "sun-time" and symbolize the importance of the cultural factor in our temporal frame of reference.

⁶ Mumford, *op. cit.*, p. 16. This author maintains (pp. 14 ff.) that "the clock, not the steam engine, is the key-machine of the modern industrial age." For an historical résumé of time-keeping devices, see A. P. Usher, *A History of Mechanical Inventions* (1929), Ch. 6, 10, and Bibliography, pp. 379-80.

⁷ *Op. cit.*, p. 293.

and coordinating all kinds of future activities in detail, a possibility excluded for societies with time systems of a less developed order. Intervals on this time-scale can be measured and quantitatively expressed in orders of any magnitude.

This modern notion of time is also the matrix of derivative concepts that characterize western civilization.

When one thinks of time, not as a sequence of experiences, but as a collection of hours, minutes and seconds, the habits of adding time and saving time come into existence. Time took on the character of an enclosed space: it could be divided, it could be filled up, it could even be expanded by the invention of labor-saving instruments.⁸

Time, in short, became reified to a considerable degree. It came to assume a commodity value.⁹ To waste "time" is still almost a heinous sin unless confined to sacred days, holidays, vacations, or other formally defined periods.¹⁰

The use of a graduated scale of *small* temporal units, moreover, and the quantitative measurement of temporal intervals made other characteristic developments possible. Human activities could be accurately rated in terms of *speed*. Thus speed itself has risen into prominence as a value of western society and functions as an important factor in the motivation of individuals. The cultural matrix, and hence the psychical relativity, of speed as an incentive in behavior is not always recognized as a derivative of our own temporal concepts. Psychological tests, standardized with respect to speed in performance, have been given to native peoples without due regard for the simple fact that speed does not have the same value for them.¹¹ No wonder then, that their scores rate lower than those of in-

⁸ Mumford, *op. cit.*, p. 17.

⁹ *Ibid.*, p. 270 "Under capitalism time-keeping is not merely a means of co-ordinating and inter-relating complicated functions; it is also like money an independent commodity with a value of its own."

¹⁰ Those "masters of regimentation," as Mumford (p. 42) calls the new bourgeoisie of the 17th century, "reduced life to a careful, uninterrupted routine: so long for business; so long for dinner, so long for pleasure—all carefully measured out, as methodical as the sexual intercourse of Tristram Shandy's father, which coincided, symbolically, with the monthly winding of the clock. Timed payments; timed contracts; timed work; timed meals; from this period on nothing was quite free from the stamp of the calendar or the clock. Waste of time became for Protestant religious preachers, like Richard Baxter, one of the most heinous sins. To spend time in mere sociality, or even in sleep, was reprehensible."

¹¹ Cf. O. Klineberg, *Race Differences* (1935), pp. 159-61. This author points out that "the large majority of tests of intelligence depend at least to some extent upon speed." He also stresses the fact that "indifference to speed is cultural and not innate," a conclusion that is

dividuals reared in western culture. Yet the results of such tests have been interpreted as an indication of *racial* rather than cultural differences.

In western civilization, too, we find an approach to an apotheosis of Time typified by its elevation to a position of supreme importance in certain philosophical systems. Bergson, for example, who took issue with the Newtonian idea of time, has been celebrated as the first philosopher of our day "to take Time seriously,"¹² as one "who finds in Time conceived as *durée*,¹³ in distinction from Time as measured by the clock, the *animating principle of the universe*."¹⁴ Time has even been called the "Mind of Space."¹⁵ It is hardly surprising then to find a *tour de force* written by Wyndham Lewis¹⁶ in which he links such philosophers with Spengler and with literary figures such as Gertrude Stein, James Joyce, *et al.* and characterizes them as typical representatives of a 20th century Time Cult.

The psychological significance of time-consciousness in western civilization also emerges with great clarity when we consider pathological cases of temporal disorientation. A person so disoriented as to be unable to give the year, month, or day of the week is almost sure to be a case of amnesia, senility, or some psychotic disorder. Thus temporal orientation is of diagnostic value in mental disorders,¹⁷ although the cultural nature and consequently the relativity of the reference points used as a standard, are not always recognized as such. In other societies the disorientation of individuals would have to be judged by different temporal norms.

From personal experience I may say that "regression" to temporal norms less elaborate than our own is an entirely painless and not unpleasant process. During the summer of 1932 when I spent most of my time up the Berens River with the Pekangikum Indians, I lost track of the days of the month, since I did not have a calendar with me; the days of the week became meaningless, since, in two settlements, there were no missionaries and hence no Sunday observance or other activities that differentiated one

supported on the one hand by the absence of any "physiological basis for a racial difference in speed of reaction" and on the other by the fact that "American Indian children who have lived a long time among Whites or who attend a busy and progressive school show a definite tendency to approximate White behavior in this respect . . ."

¹² S. Alexander, *Space, Time and Deity* (1920), Vol. 1, p. 44.

¹³ I.e. as a process, rather than as a mechanical succession of separate instants.

¹⁴ *Ibid.*, p. 36. Italics ours.

¹⁵ *Time and Western Man* (1928), Preface, p. xi.

¹⁶ *Op. cit.*

¹⁷ See George H. Kirby, *Guides for History Taking and Clinical Examination of Psychiatric Cases* (1921), p. 69. For abnormalities in the judgment of temporal intervals see P. Schilder, *Psychopathology of Time* (*Journal of Nervous and Mental Disorders*, Vol. 83, pp. 530-46, 1936).

day from another and, as my watch stopped running, I had no way of keeping track of the hours of the day. My "disorientation," of course was only such relative to the reference points of western civilization to which I was habituated. Once the usual mechanical and institutional aids to these were removed, the relativity and provinciality of western time concepts became obvious. But the significant fact is that since I remained associated with human beings it was a very simple matter to make their temporal reference points my own. My re-orientation simply involved the substitution of new, less elaborate but no less culturally determined, reference points for the old.

Ella Winter¹⁸ gives an example of the relative ease with which it is possible to adapt oneself to a new frame of temporal reference. On her second day in Russia she was invited to a party "on the sixth," She asked what *day* of the week it was but the reply was, "I don't know. They've abolished the week and we never think about the names of days any more." The author insisted that since she was not a Russian she must know. "No American," she comments, "could forget the names of the days of the week¹⁹ just because the Russians had introduced the five-day week and abolished Sunday." But a month later when the author was asked to tea by an American friend—"next Wednesday"—almost unthinkingly she inquired, "What date is that?" The Russians have simplified our scheme of temporal references by omitting one item.

Individuals of course ultimately acquire the temporal frames of reference characteristic of their society along with the rest of their cultural heritage. But this acquisition is a process, not a mechanical transference of temporal concepts from one generation to another. Binet noted this many years ago and Sturt carried out an investigation designed to throw light upon the genetic development of some temporal concepts in children of different age groups.²⁰ Detailed studies of individual children such as those made by the Sterns and Decroly and Degand indicate how gradually the time-concepts of western civilization are acquired. A summary quotation²¹ based on the work of the observers mentioned illustrates this.

Recognition that *yesterday*, *today*, and *tomorrow* had reference to certain days was gradually developed during the fourth year by both Hilde and Suzanne, but a clear grasp of the relationship symbolized by these terms was still confused and *only be-*

¹⁸ *Red Virtue* (1933), pp. 171-72.

¹⁹ "The word Saturday is still used, but means not a definite day of the week, but any one of his free days the worker gives to additional voluntary work" (p. 172).

²⁰ Mary Sturt, *The Psychology of Time* (1925). A questionnaire method was employed

²¹ F. Lorimer, *The Growth of Reason* (1929), p. 114.

came established in the fifth year. The correct use of *yesterday*, *to-day*, and *to-morrow* as names of days, and a fixed order of seven days making a week, involves abstract chronological schemata which first become fixed late in the pre-school period and in the early school grades. These words, to be sure, appear in the vocabulary much earlier, but they are used indiscriminately or with reference to continuous undifferentiated past as such, continuous present, or continuous future.

In some American Indian languages the terms for day-before-yesterday and day-after-tomorrow are the same.²² It would be interesting to know when children in these societies learn to employ these words with different temporal meaning.

Once we step outside of our own society and examine the frames of reference that are relevant to the temporal orientation of other peoples, the cultural constituents of human temporal experience are thrown into even greater relief. Although astronomical events, as Sherif points out, furnish us with very convenient and stable frames of reference for a calendar. Nevertheless, we must not think that there is absolute necessity for using astronomical events as reference points for time-reckoning.

Other objects, events, and activities can be and have been used. The Andamanese furnish a striking example of this.

In the jungles of Andamans [writes Radcliffe-Brown] it is possible to recognize a distinct succession of odours during a considerable part of the year as one after another the commoner trees and leaves come into flower. . . . The Andamanese have therefore adopted an original method of marking the different periods of the year by means of the odoriferous flowers that are in bloom at different times. Their calendar is a calendar of scents.²⁴

In certain parts of the Pacific torches have been utilized as time-reckoning devices,²⁵ in other places market days or sacred days which occur at regular intervals have defined temporal periods similar in principle to our "week," while other human activities and the appearance of certain animals at regular seasons of the year or meteorological changes²⁷ have elsewhere been used as traditional reference points for temporal orientation. Beyond the

²² L. Cope, *Calendars of the Indians North of Mexico* (University of California Publication in American Archaeology and Ethnology, Vol. 16, 1919), p. 125.

²³ *Op. cit.*, p. 11.

²⁴ A. Radcliffe-Brown, *The Andaman Islanders* (1922), pp. 311 ff.

²⁵ W. Hough, *Time Keeping by Light and Fire* (American Anthropologist, Vol. 6, 1893), p. 207.

²⁶ H. Webster, *Rest Days* (1916), pp. 193 ff.

²⁷ B. Malinowski, *Lunar and Seasonal Calendar in the Trobriands* (Journal, Royal Anthropological Institute, Vol. 57, 1927).

mediate observation and experience of individuals mythology may express chronological relations between past events or even outline a definite evolutionary sequence in the development of natural objects and man.²⁸

II

In what follows it is my purpose to examine in some detail the cultural constituents of temporal orientation among the Berens River Saulteaux. These are an Ojibwa-speaking group of hunters and fishermen living in the forested region east of Lake Winnipeg. Today they number some 900 individuals, grouped in three bands that occupy the river from its mouth to Lake Pekangikum, a distance of about 260 miles. Each band was allotted a reserve subsequent to treaty negotiations with the Dominion Government in 1875 but since the country does not permit farming, the Indians congregate in fishing settlements in or near the Reserves during the summer months and scatter to their hunting grounds for the fall and winter. The Berens River Band proper, located at the mouth of the river and close to Lake Winnipeg, are the most sedentary, the settlement there being occupied all year around by the women, children, and old people, the men making periodic trips to their hunting grounds. This band was the first to be Christianized and during the last half century acculturation has proceeded with great rapidity. Yet very few members of this band speak English and up the river none of the Indians do. The Pekangikum Band, farthest inland, is not yet fully converted and native customs and belief flourish there with the greatest vitality. The Grand Rapids Band, living midway between the two mentioned, is only superficially converted to Christianity and is almost as aboriginal in most respects as the Pekangikum Band.

Movements of the Sun. According to Saulteaux belief the earth is flat and each day the sun travels from east to west above it. This is the period of daylight. It may be said to constitute a temporal unit for which the native term is *pezagógi-jik*, "one day." When the sun disappears behind the western edge of the earth, it travels eastward beneath the land, to reappear at dawn. It is during this part of the sun's journey that darkness ensues. This period is recognized as another temporal unit, *pezagwatabik* "one night."

Strictly speaking, there are no standardized *durational* units of these alternating periods of light and darkness which, at the latitude 52° N., vary greatly in length at different seasons of the year. For the period of daylight a succession of discrete moments are recognized. These are crystal-

²⁸ As in Polynesian mythology. See R. B. Dixon, *Mythology of All Races, Volume 9 Oceanic* (1916).

lized in more or less standardized phrases that indicate the position of the sun,²⁹ or refer to the relation of its light to discernible objects. At the beginning of the day some very fine distinctions are drawn. The intervals between the discrete points recognized vary enormously in temporal length as measured on our absolute time scale. But this is irrelevant to the Saulteaux, and of course, it is possible to employ the intervals between any two of the points recognized as a crude measure of temporal length.

When streaks of light, distinguishable in the east, announce the first signs of coming day, although darkness still reigns on the earth, this is *pi'tában*, dawn. When darkness is dispelled so that one can discern terrestrial objects at some distance, but the sun has not yet risen above the tree tops, it is *tcí'bwasaágàtik*, "before coming out from the trees (the sun)." Soon the light from the rising sun reddens the treetops. This point of time in the new day is called *miskóanagáte*, "red shining (reflected) light." In addition, there are two other expressions that refer to the position of the sun before it emerges into full view. One of these connotes the point in time when the sun is still behind the tree-tops, literally, "beneath trees when hangs (the) sun," *ánámatikèpi'ágotcinggí'zis*; the other, when it reaches the tops of the trees, "tops of trees when hangs (the) sun," *ékwánákak èpi'ágotcinggí'zis*.

Once above the trees but still low enough in the eastern sky for its position to be judged with reference to them, there are two further expressions used that involve rudimentary units of *spatial* measurement. The first refers to the fact that the sun "hangs" in the sky "the breadth of my hand" above the trees, *ni'oníndjèpi ápi'tagotcing*. The second, that it hangs *pezagwákwagan èpi'ápi'tagotcing*, a thumb-middle-finger-stretch above the tree-tops, a distance of about twice that of a hand-breadth.

The position of the sun in the sky during the remainder of the day is differentiated with respect to much larger temporal intervals and in a less refined fashion. The following expressions are used: *eáni-ketci ski'qbakwit*, "as high as it goes up;" *eàptagí'zigak*, "half-day (midday);" *eàptawí'názit*, "half-way to setting;" *pəngíciman gí'zis*, "falling (out of sight) (the) sun" [sunset]; *poní'aními'gí'jigan*, "disappearing underneath day." The last term applies when a band of light still rims the horizon, after the sun itself has disappeared. For dusk, which in summer is especially prolonged at this latitude, the term *naní taga* is used,

²⁹ The term *gí'zis* is applicable to both sun and moon. Generically therefore, it may be translated "luminary." In actual use it is defined by its context so that in the English rendering of terms I have used sun or moon. The Saulteaux themselves sometimes use a term which means "night luminary" for the moon. The translations of native words embody the meanings understood by the Indians. They are not based on refined etymological analysis.

Movements of the Stars. While the Saulteaux do not have a highly elaborated star-lore they have names for a number of the constellations and they have observed the movements of these during the night. The appearance of the morning star, wábanānang, is also noticed. They know Polaris and they have noticed the rotation of the circumpolar stars, particularly Ursa Major, which they call k'tci·otcī·ganang, Great Fisher. They have also observed that from December to May the Belt of Orion (odádawaämok, three young men) comes up from the horizon, mounts in the sky and disappears before dawn. From the position of the former constellation in summer and the latter in winter a rough temporal orientation is obtained, but the only term that I know of which expresses any particular point during the night is kegáepi'tatabíkak, "nearly half (the) night" [mid-night]. Night, therefore, lacks the *formalized* points of reference established for the daylight period,³⁰ but direct observation of stellar movements makes possible unformalized nocturnal orientation.

Night and day, then, are distinct temporal units,³¹ formally subdivided by traditionally established discrete points of reference, but not reckoned in standardized units of duration. "Nights" or "sleeps," rather than days, are customarily used as measures of temporal length³² and of *distance*. A man leaving camp will tell his wife that he will return after a certain number of "sleeps" or he may express the distance to a certain point in terms of "sleeps" or "nights." This rendering of distance in temporal units reminds us of the astronomer who finds it convenient to make the vast distances beyond experience intelligible by translating them into the language of time.³³

Timed Daily Activities and Special Events. It seems likely that sleep has proved a convenient point of reference because of its periodic and regular recurrence. In summer, the camps quiet down at dusk, unless there is some unusual event in progress, and the relatively short period of darkness is equivalent to the period of sleep. If any of the women or girls have been visiting, one sees them making for their own dwellings as darkness ap-

³⁰ In the Southwest, on the other hand, nocturnal points of reference are elaborated, as certain songs are customarily sung at certain intervals during the course of ceremonies held at night (Cope, *op. cit.*, p. 126)

³¹ Our "day" of twenty-four hours is a conventional unit for which we have no specific term. By calling it a "day" we employ the principle of *pars pro toto*. Cf. Nilsson, *op. cit.*, pp. 11 *et seq.* But this author is mistaken when he asserts that reckoning in "nights" among primitive peoples involves the same principle. Since they do not entertain the concept of a day-night period as a unit, "night" cannot be regarded as a symbol of the whole, but simply as a discrete recurring phenomenon that can be counted

³² There are likewise many instances in the mythology.

³³ I.e., light-years.

proaches and the men and boys soon follow them. This pattern of returning to one's own camp before night-fall is so well established even among the Indians of the mouth of the river that, on several occasions, the family with whom I lived thought that I must be lost in the bush when I did not show up at the expected time. In winter, of course, different conditions prevail. The men are out on their trap lines or hunting long before daybreak and often do not return until after darkness has fallen.

In western urban culture, at least, eating at regular intervals has come to be an established pattern that in itself provides unformalized reference points in our temporal orientation. Being hunters and fishermen, the sources of food supply among the Saulteaux are precarious and meals are irregular. Hence eating cannot function, like sleeping, as a relatively stable reference point in daily activities. Since my own day in the field was organized on a routine three-meal basis, I sometimes arranged to have one informant come to my tent in the "morning" and another in the "afternoon." But it happened more than once that an informant would come so late in the morning that it was almost time for the noon meal and on one occasion the man expected in the afternoon showed up a few minutes after the one scheduled for the morning session arrived. Neither informant, of course, had any sense of being "late" or "early," and I could not have said to either one, "come to see me as soon as you have eaten." The lack of common reference points made it difficult to coordinate our activities efficiently.

Since almost all the Indians in the upriver settlements set their nets at night and lift them in the morning, what I did say to the informants expected in the forenoon was, "Come over as soon as you have lifted your nets."

On the whole, however, there are no set times for daily activities. Their rhythm is elastic in the extreme and except when motivated by hunger or necessity they are dictated to a large degree by external circumstances and by whim.

With respect to such activities as conjuring, dances, and ceremonies, however, there is a definite temporal patterning. Conjuring, for example, is always done after dark. The *wabanówi-win*, too, was formerly held at night but nowadays it takes place only during the day. As soon as night falls the dancing stops, to begin again the following morning. The give-away dance (*mandáiti-win*), no longer performed, was also held at night. Today the only dance held after dark is the *potáte*; it is the most purely social affair that the Indians up the river have. The *midéwi-win* was the ceremony with the most exactly defined temporal limits. The lodge was entered at sunrise as a song with the words, "the one that's going to rise, I'll travel with him," was chanted. And it closed at sunset with a salute to the sun.

The day set for the midéwi·win was, of course, decided beforehand by the leader and word sent to his assistants. This likewise occurs today in the case of the wabanówi·win which may be held on one to four successive "days." But the time of day when it starts is not set. When the leader is ready he starts to sing and drum. Those wishing to attend come whenever they are inclined. Even the singers that the leader has asked to help him do not all come at once. They dribble in one by one. If the leader has begun in the morning all of them may not have arrived until afternoon.

In the case of the potáte dance, which is "owned" by several different men of the Grand Rapids settlement, and may be started by any one of them with the help of four singer-drummers, the signal for attendance is the drumming itself, begun at dusk. The songs are recognized and so everyone knows what is going on. If only a few people come the dance may cease. I was present once when this occurred.

Acculturation of Timepieces, Named Days, and the Week. Contact with traders for a hundred years and the advent of missionaries at the mouth of the river in 1873 have been the chief sources of profound changes in certain aspects of the life of the Berens River Indians. But so far as time reckoning is concerned these influences have had little effect on daily life, except at the mouth of the river. On the Reserve in this locality, e.g., there are both Protestant and Catholic missions, both of which include day schools in their program. The bell of the Catholic mission rings at six A.M., noon and six P.M. at night so that the Indians have come to recognize these hours as punctuating certain divisions of the day. School bells also summon the children to their lessons³⁴ and adults to church services on Sunday. A similar situation obtains, however, in only one of the five settlements up the river. There are no other occasions when the collective attendance of any group of individuals is demanded at a certain hour. For, as we have already pointed out, the attendance at native ceremonies is much more flexible in this regard.

While clocks are not a novelty at the mouth of the river,³⁵ by no means every household owns one and up the river I remember seeing only two or three. At Lake Pekangikum one family had recently purchased an alarm clock, the deferred ting-a-ling of which seemed to fascinate them, rather than its utility as a time-reckoning mechanism. There are perhaps a dozen men at the mouth of the river who own watches. But they seldom carry them about, to say nothing of using them in the regulation of daily activi-

³⁴ But Mr C. D. Street, the Protestant schoolmaster told me that even in the coldest weather a dozen or more pupils are on hand an hour before school starts. They are not guided by the bell, or by time-pieces, but set out for school as soon as they have had breakfast.

³⁵ Where the children are taught to tell time in school.

ties. I remember, too, the pride with which a young fellow of the pagan Duck Lake settlement showed me his recently-acquired gold-filled watch that contrasted more than favorably with my Ingersoll. And in 1936 I found that a young girl at Grand Rapids had acquired a wrist watch. The chief of the Berens River Band, however, constantly carries a watch and frequently consults it.

In the aboriginal cultural pattern, "days" and "nights" are not grouped in any temporal unit of a higher order. There is no "week." Nor are there any named "days," although there are special terms for yesterday, day before yesterday, tomorrow, and day after tomorrow. Under missionary influence, however, the emphasis upon the Sabbath as a day of rest and religious observance³⁶ made it necessary to instruct the Indians in the calculation of its periodic recurrence. Consequently the week is now recognized as a unit of time among the Christianized natives and there is a term for it. It is interesting to observe, however, that this temporal unit was assimilated as part of a new *religious* orientation, rather than as a secular temporal concept as such. Sabbath observance is such a tangible and fundamental tenet of Protestant Christianity that it was one of the first things taught to the Indians by the missionaries. Egerton R. Young, the first resident missionary on the Berens River, tells of a visit he once received in the summer from an Indian woman who lived some distance inland. She had heard of the "Great Book" and had come for information.

Before she left [says Rev Young],³⁷ I gave her a sheet of foolscap paper, and a long pencil, and showed her how to keep her reckoning as to the Sabbath day. I had, among many other lessons, described the Sabbath as one day in seven for rest and worship; and she had become very much interested and promised to try to keep it. [The following winter he visited the woman's camp. During the course of a meal which he took in her wigwam, the old woman] inserting one of her greasy hands in the bosom of her dress . . . pulled out a large piece of soiled paper, and unfolding it before me, she began in excited tones to tell me how she had kept the tally of the "praying days," for thus they style the Sabbath. . . . Imagine my delight to find that through the long months which had passed since I had given her that paper and pencil, she had not once missed her record. This day was Thursday and thus she had marked it. Her plan had been to make six short marks, and then a longer one for Sunday. [Then the woman spoke] "Missionary, sometimes it seemed as though I would fail. There were many times when the ducks or geese came near, and I felt like taking my gun and firing. Then I remembered that it was the praying day, and so I only put down the long mark and rested. I have not set a net, or

³⁶ Aiyamæyegí jigān, praying day.

³⁷ Egerton R. Young, *By Canoe and Dog Train Among the Cree and Saulteaux Indians* (1890), pp 263-64.

caught a fish,³³ or fired a gun, on the praying day since I heard about it at your house so far away."

An Indian family I once visited in the Pauingessi settlement were keeping a similar record of the days of the week and their count was likewise accurate. But I fear that I did not exhibit the enthusiasm of Reverend Young when I verified it for them. To-day, no one ever sets out on a journey from the mouth of the river on Sunday and even the Christian Indians at Grand Rapids have become so completely acculturated to this periodic holy day that on one occasion when a gold strike was reported nearby and the missionary made a visit to the spot to look it over with a view to staking a claim, he was subjected to open criticism on this account.

The spread of Christianity, then, has been responsible for creating the basis for a new temporal unit in the minds of the Indians. But even so the concept of the week as such does not seem to function very significantly in their life and thought. It is the periodicity of the Sabbath, signalized by going to church and abstaining from certain secular activities, that is the important reference point in their lives.

Following the establishment of this reference point, the day-naming pattern, with which we are so familiar, developed. But the Indians did not attempt a rendering of English names for the days of the week. Consequently their series of names have a quality all its own. Monday is literally "cease praying day" (*póni·aiyàmāyegí·jigan*), Tuesday is two days "after" (*ní·jogi·jigan*), Wednesday is "half (week) gone" (*api'taúwase*), Thursday, the "great half gone" (*k'tci·api'taúwase*), Friday, "approaching day" (*ení·ogi·jigan*), and Saturday, *pakwéjigangí·jigan*, "flour (bread) day." This last designation, so I was told, arose from the fact that it was the custom of the Hudson's Bay Company to pay their employees in kind on Saturday.

These terms have been in use at least fifty years at the mouth of the river. At Grand Rapids they are known but not commonly used. Farther up the river, especially in the pagan settlements where Sunday is not observed, they would, of course, have no meaning.

It seems to me that one can observe in the naming process that has accompanied the acculturation of new features, the fundamental pattern of native temporal orientation. I refer to the emphasis upon particular concrete events as basic reference points. Thus Sunday becomes characterized as "praying day" and Saturday as "ration day," while the other days relative to these are neutral in character, because they are signalized by no

³³ Nowadays nets are lifted if people are short of food; otherwise not.

outstanding events. But the whole psychological focus of the day names is obviously Sunday. A religious event colors the whole series, with the exception of Saturday, in which case an event of economic importance overshadows it.

The same underlying principle is reflected in the native nomenclature of the next highest order of temporal unit—the “moon.”

Lunar Changes. As in the case of the day, a “moon” is not a division of continuous time, it is a recurring event. The period when the moon is visible, and its changing appearance as it waxes and wanes is a “moon.” Twelve named lunations form a loosely coordinated succession with which no day count is integrated. The moons are differentiated by names which refer to such non-celestial phenomena as the appearance in the country of certain birds, the condition of plant life, the rutting of animals, human economic activities, etc. These are all seasonal periodicities, subject to considerable variation, that are loosely coordinated with particular lunations. There is an interesting episode in one of the myths in which an attempt to identify a moon by trying to catch sight of an eagle beguiles an old cannibal to his death. The latter is asked what moon it is and replies, “Mid-winter moon.” “No, you’re wrong. This is Eagle Moon.”³⁹ Look! There is an eagle now passing behind you.” When the old man turned to look, his throat was cut and his murderer remarks, “Did you expect to see an eagle at this season?”

This episode both reflects the chronological uncertainty connected with the aboriginal lunar calendar and the court of appeal in case of doubt. Since in any solar year there are more than twelve and less than thirteen lunations, a seasonal dislocation arises which the Saulteaux correct by adding an unnamed moon to the series.⁴⁰

Thus the lunar calendar itself is intrinsically flexible because its real emphasis is less upon the successive waxing and waning of the moon, than upon non-celestial phenomena. The succession of events of the latter class provides the real temporal guide. But the correlation of non-celestial phenomena with lunar periodicities by means of a conventional nomenclature, defines *limits* of elastic, yet standardized, divisions of time which the mere observation of the arrival of different species of birds in the spring, the ripening of berries, etc., would not in themselves be sufficient to establish.

³⁹ The one following Midwinter Moon.

⁴⁰ Cf. J. G. Kohl, *Kitchi-gami* (1860), p. 120: “. . . they add every now and then a thirteenth nameless moon in order to get right with the sun again.” and “. . . it is often comical to listen to the old men disputing as to what moon they are in.” Cf. Cope, *op cit*, pp. 131, 137-39.

Although a nuclear group of characteristic names for the lunations seems to typify the Ojibwa-speaking peoples, the other names employed seem to show considerable variation.⁴¹ The calendrical nomenclature, as might be expected, seems as a whole to be closely connected with local conditions and appears to vary accordingly. Of the nuclear group of names, one referring to the appearance of vegetation, usually the blossoming of flowers, is always to be found, one or more that have reference to the ripening or gathering of berries, others that indicate the appearance of certain birds, especially the wild goose, etc. On the Berens River, for example, three lunations are named for birds which make their local appearance at that time of year. Roughly, these lunations correspond to our months of March (migazí-wi-gí-zis, Eagle Moon), April (ni'ki-gí-zis, Goose Moon), and May (mángogí-zis, Loon Moon). The periodic reappearance of the geese in April is attested by the records kept for a series of years by the Natural History Society of Manitoba.⁴² They usually appear during the first week of this month and are to be seen only until the second week in May. They do not reappear until August, after which they are seen no more until the following April.

Most of the moon names, as might be expected, are identical among all the Berens River bands. But it is noteworthy that not all of them are. Those which differ are the following:⁴³ January: (B.R.) kictcopabi-watakinam; (G.R.) kaginwasi-getgi-zis, Long Moon; February: (B.R.) api'tapi-bungi-zis, Half-winter Moon; (P.) ki-jé-gi-zis, Kind Moon;⁴⁴ July: (B.R.) ati'ktemini'kawi-gi-zis, Ripe berries gathering Moon; (G.R.) wabagwani-wi-gi-zis, Blossom Moon; September: (B.R.) manómini'kawi-gi-zis, Wild rice gathering Moon; (P.) amanozówi-gi-zis, Rutting Moon; December: (B.R.) opa'piwatcagenazis; (G.R.) pitci-babunwi-gi-zis, Early winter Moon.

It will be noted that although different names are used, the terms for both July moons refer to the condition of plant life which is likewise reflected in the name for the June moon (sagi'bagauwi-gi-zis, "leaves coming forth moon"). The reappearance of vegetation at this season of the year

⁴¹ See the compilations in Cope, *op. cit.*, pp. 165-66 where seven series of names for the lunations are given.

⁴² *Bird Calendar*, designed and compiled from the records of A. G. Lawrence, Ornithological Secretary.

⁴³ For the sake of convenience they have been correlated with our series of month names to which they roughly correspond. The letters in parenthesis preceding each native name indicates the band in which its use is found. B. R., Berens River Band, G. R., Grand Rapids Band; P., Pekangikum Band.

⁴⁴ So-called because the winter is beginning to moderate

after the disappearance of snow and ice transforms the external aspects of the country so radically that it would seem to be an almost inescapable standard of reference in any scheme of temporal divisions based on the general pattern characteristic of the Saulteaux. July marks the peak of vegetation development.

On the other hand, there are marked local variations in the occurrence of wild rice in sufficient quantities to make it worth harvesting.⁴⁵ This is the explanation of the difference in nomenclature for the September moon at the mouth of the river and at Grand Rapids, as compared with Pekangikum. Within the habitat of the latter band wild rice is so scarce that it is never harvested. Hence, one would hardly expect to find it in their calendrical scheme since, as I have pointed out, the nomenclature of the lunations is intrinsically elastic, and likewise pragmatic. It may also be remarked in passing that there are no sugar maples in this country so that, as compared with other Objibwa peoples in whose calendar a "sugar making moon" is found, the Berens River Saulteaux lack a lunation of this name.

Ceremonies are not standardized with respect to performance in particular moons and while some individuals know the moon in which they were born, others do not. At the mouth of the river English month names are known and utilized to some extent so that the aboriginal calendar, as such, is being supplanted.⁴⁶

Within each lunation discrete points are recognized, but these are not conceptualized as periods of temporal duration. In principle they parallel the points recognized in the changing position of the sun during the course of the day. But in the case of the moon, reference is made to differences in its size as it waxes and wanes. The following terminology is employed: *eoskagotcing*, newly hanging; *eanimitcapikizazit*, bigger; *eàptawábkizit*, half; *kegáewáwi ezit*, nearly round; *èwáwi'ezit*, round; *epákwezit*, going; *eàptawábkizit*, half (-gone); *eagasabikizit*, getting small; *emetasi'get*, it is going.

Seasonal Changes. Observable changes in temperature, vegetation, and other natural phenomena define the seasons for the Saulteaux. Six are recognized. The names of these differ from those of the lunations in being non-descriptive terms. The Indians cannot translate them. But natural phenomena chosen, define the limits of each season quite specifically and no inter-

⁴⁵ A. I. Hallowell, *Notes on the Northern Range of Zizania in Manitoba* (Rhodora, Vol. 37, pp. 302-304, 1935).

⁴⁶ Children in school are taught the English names of the months. Mr Street said: "They always prize a calendar and follow it studiously at home. Some homes use our names altogether; others never."

calation is ever necessary. It is recognized, too, that the moons fall into seasonal units. This is quite possibly the chief conceptual mechanism by means of which they are kept adjusted to the solar year.

The passing of winter, for example, is signalized by the appearance of the migratory birds, the first of which are seen in March. Now the moons that correspond to our months of March, April, and May are those with bird names and these three lunations together constitute the season called *äsígwán*, the spring season of the Saulteaux. The following season, *ní·bín*, equivalent to our summer, does not begin until the ice has completely disappeared from the lakes and rivers and there is no more snow to be seen on the ground. Since slightly over one inch of snow is to be expected at this latitude in May, *ní·bín* normally begins in June and continues through August and into early September. It is significant that the June lunation, the first moon of the summer season, bears a name that refers to the re-appearance of leaves on the deciduous trees and that up the river July is called the "moon of blossoms." It is in this latter month that the highest mean annual temperature occurs.

The next season recognized, *tagwágin*, begins when the leaves of the poplars and birches start to fall. It is a short season, never longer than a "moon." It is immediately followed by another short but named interval, *pí·gí·kánaan*, defined by the fact that, although the trees have lost their leaves, the winter has not yet set in. This is also the period of so-called "Indian Summer" after which "freeze up" occurs and the first heavy snows of the winter are due. These normally do not occur until November, the term for which, "freezing moon," indicates that the winter, *bi·bún*, has actually set in, to continue through three additional moons. Two sub-season winter units comprising two moons each are named. The first part of the winter season is called *oskí·bibun*, "new, fresh, winter;" the latter and most severe half when the temperature drops the lowest, is called *megwábi·bun*.

Ceremonies like the *midéwi·win*, the *wabanówi·win*, the "give-away" dance, etc., were always held during the summer season. This was the period when the Indians congregated in their summer fishing settlements. With the falling of the leaves began the dispersal to their hunting grounds.

Seasonal names occur in the mythology and, in the myth accounting for the origin of summer, it is explained how it came about that winter was reduced to only five moons (*sic*) in length. In one myth, too, the *passage of time* is conveyed not with reference to nights or moons but in terms of seasonal change. *Mikinak* (the Great Turtle) chases a moose and although there is snow on the ground when he starts the pursuit he does not catch

up with the animal until open water. Length of time is of importance in this story because it is one of the contributing factors to the humor of the tale.

In conversation that has reference to past events seasonal names, too, appear to be more frequently employed than "moon" names. Although less exact, these larger units are sufficiently precise and they function in much the same way among ourselves. Despite the instrumental value of our exact time scale, for certain purposes constant references to month, day, and hour of past events would appear pedantic even in our society.

Annual Cycle. As Cope, speaking of the Indians in general, says,⁴⁷ the year may be regarded as the *interval* between recurrent events, since no attempt is made to compute its length in days,⁴⁸ and since the number of moons is somewhat uncertain in the native mind.

In native Saukteaux thought the concept of such an interval, reckoned with reference to the recurrence of winter, but not conceived as the sum of a series of smaller divisions of time, is undoubtedly present. But this concept is by no means identical with our concept of the year as a temporal unit of continuous duration reducible to smaller measurable units which we conceive to have a precise beginning and ending reckoned from the stroke of midnight on December thirty-first. When asked to name the "moons," for example, the Saukteaux will begin almost anywhere in the series but usually with the current "moon." Consequently the question whether the Indians, in the absence of such a concept of temporal continuity, actually reckon a beginning or ending of their annual cycle is irrelevant.⁴⁹ What is significant is whether some recurring solar event, like a solstice, or some terrestrial occurrence, is traditionally recognized as a discrete point of reference with respect to some temporal interval that is of a higher order of magnitude than the lunation or the season. Such an interval was an integral part of the temporal concepts of the native Saukteaux but was of little practical importance. Yet its recognition explains why at present the Indians often employ the term "winter" in reckoning their ages or to place events in a supra-annual time scale. "Winter," as a recurrent annual event can be used

⁴⁷ *Op cit*, p. 136 Italics ours

⁴⁸ It is hard to say, of course, whether such practise as notching a stick for every day of the year, carried out by the father of one of Densmore's informants, was a native custom or one suggested by contact with the whites. See Frances Densmore, *Chippewa Customs* (Bulletin, Bureau of American Ethnology, No. 86, 1929), p. 119.

⁴⁹ Cf. Cope, *op cit*, map 2, where the "beginning of the year" among various peoples of native North America is indicated. Cf. Nilsson, *op cit*, pp. 267 *et seq.* for a further discussion of this question.

to symbolize a "year," if reckoning in such terms becomes of interest. And if one wishes to say, as Cope does in interpreting the calendrical schemes of other northern peoples, that winter signalizes the "beginning" of the "year" such a statement is also intelligible since it is the recurrence of this season that has received formal emphasis with reference to an annual cycle.

Today the Saulteaux of the Berens River Band proper are familiar, as I have said, with the white man's mode of temporal reckoning. And they use the term *pezígoa'ki*, "one earth," for one year. But I think this is a new term, a judgment that receives correlary support from the fact that Baraga makes "year" and "winter" synonyms.

Although winter is the formalized reference for computing yearly intervals in the aboriginal conceptual scheme, the recurrence of the other seasons provide unformalized points of reference, that punctuate equivalent intervals. And among human activities the annual performance of the *midéw-i-win* was once prominent. Today, the payment of annuities each year by the Dominion Government is another regular occurrence and at the mouth of the river the Indians know that there are legal holidays, like Dominion Day and Labor Day, that recur annually.

Reckoning of Past Events. The recognition of a yearly interval by no means implies that the year as a temporal unit functions very actively in native life and thought. In terms of aboriginal life, in fact, there was little, if anything, that demanded calculation in annual units of time. Consequently I believe that Cope⁵⁰ rightly emphasizes the fact that

although it is often loosely stated the Indian could tell his age by the expression "so many winters had passed over his head," or that he was so many winters old, this expression is no doubt developed through contact with civilized peoples. The expression more in keeping with the Indian calendric systems is that found in so many tribes: "I was so large when a certain event happened." This event may be a year of famine, a year of some epidemic, the growth of a particular tree or grove, or some remarkable exploit. . . . Such vague statements or references as these are probably as near as the Indian, of himself, ever came to considering his age.

Among the Saulteaux, so far as age is concerned, instead of a chronological year count, the life cycle of individuals was divided into a number of terminologically distinguished age grades corresponding to maturation stages. The generic term for child, *apí'nondji*, includes the viable foetus within its connotation, while the infant from birth until it begins to walk is called *ockapí'nondji*, fresh (new, young) child. Sexual differentiation is expressed in the terms *i'kwéžs*, little girl, and *kwí'wi-zs*, little boy, applied

⁵⁰ (*Op. cit.*, pp. 136-37.

to children under puberty. The next age-grade terminologically recognized is what may be called youth. After puberty and before marriage a male is *ockí ni ge* and a female *ockíní'gi'kwe*. Bachelors and spinsters are rare and it is interesting to note in connection with a discussion of time concepts that the words used for such unmarried individuals are the youth terms with a prefix, *kete-*, meaning "old" (*ketèockí'ni'ga*, "old young man," and *ketèockí'ní'kwe*, "old young woman") a kind of temporal paradox. For succeeding periods of maturation there are no terms with a chronological connotation used until old age is reached⁵¹ (*mintímoye*, "old woman," *áki'węzi*, "old man.")⁵²

Such terms, correlated with references to occurrences extrinsic to the individual, such as the signing of the Treaty, the advent of the missionaries in different settlements, the tenure of Hudson's Bay Company post managers and the Great War,⁵³ or events in the life history of individuals—marriage, journeys, former hunting grounds, customary camping sites, etc., are sufficiently exact, though unformalized, points of reference for purposes of native temporal orientation. They occur again and again in conversation and in the personal reminiscences I have collected. It is in the use of such unformalized reference points that the Saulteaux are most like ourselves,⁵⁴ the difference being, of course, that they lack the more exact frame of temporal reference that we possess which permits time measurement in precisely defined units and temporal comparisons of a more accurate order.

Events in the past are also frequently correlated with the life-span of certain deceased relatives of the living or other deceased persons. So long as the names, personal characteristics and activities of deceased individuals are carried in the memories of living persons a useful, although non-quantitative and unformalized, frame of reference for past events is maintained. The collection of extensive genealogical information has convinced me of the accuracy of the knowledge that is the basis of this human frame of reference. Through the assimilation of a considerable portion of it myself, I found that I was able to use the information acquired with reference to the temporal sequence of certain events much as the Indians do themselves. But in quantitative temporal terms retrospective genealogical information

⁵¹ There are only terms for married man, *onabemi ma*, and married woman, *wí wi'mán*.

⁵² To indicate extreme old age the augmentative prefix *k'tci* - may be added to these terms.

⁵³ Because several Berens River Indians enlisted.

⁵⁴ Cf. Nilsson, *op cit*, p. 105: "Whoever looks back over his past life sees chiefly the more important events, not the dates of the years, and to these he joins the more peripheral events and so finds his way in the labyrinth of memory."

of this sort completely fades out in less than two centuries. One hundred and fifty years is the outside limit of any genuine historic past so far as the Berens River Indians are concerned. Events attributed to so distant a past that they cannot be connected with any known generation of human individuals are simply described as having taken place "long ago." Consequently we are plunged into a bottomless mythological epoch that lacks temporal guide posts of any conventional sort. As a matter of fact, it would be more accurate to assert that once we enter the mythological world of Saukteaux belief, temporal concepts actually lose most, if not all, chronological significance.

One of the reasons for this is the fact that the most prominent anthropomorphic characters of mythology like *wisakedjak* and *tcakábec* are not only living beings, they are conceived as immortal. They were alive when the earth was young and they assisted the Indians then. They are still alive today and continue to aid mankind, this latter fact receiving empirical demonstration in dreams and by the manifestation of the presence of such beings in the conjuring lodge. The conventional pattern of dream revelation and the conjuring lodge are, then, institutional means of keeping mythological beings and spiritual entities of other classes constantly contemporary with each new generation of individuals, despite the passage of "time." Such spiritual entities, in fact, are actually more "real" than distant human ancestors no longer remembered. Mythology itself sometimes reflects this emphasis on the contemporaneity of such beings by incorporating episodes with modern "trimmings" in narratives that contain nuclear elements not only found among the Saukteaux but over wide areas in North America.

It is true, nevertheless, that certain episodes in the mythological narratives provide a basis for certain broad temporal inferences. And independently of immediate mythological references, I found that the Indians entertain similar chronological notions. It is noteworthy, for example, that in many of the mythological narratives, the form of the name given to familiar animals contains the augmentative prefix. There are references to the Great Snake, the Great Mosquito, the Great Beaver, the Great Trout, etc. This has a temporal significance. Formerly the earth was inhabited by many of these monster species now only represented by smaller varieties of their kind. In the myths there are likewise accounts of how certain of these great animals became extinct (the Great Mosquito) or how the familiar variety of the species came into being (as, e.g., small snakes). It was explained to me that the mythological characters had power enough to overcome the monster fauna but that ordinary human beings would be constantly harassed if they had to live on the earth with such creatures

today. Nevertheless, a few such species still survive according to the firm conviction of the Indians. There are Indians now living, in fact, who have seen them. But the events in the myths which involve the monster animals are conceptualized as occurring in a far distant past. They took place "long ago" in a period when the earth was "new." Consequently a temporal distinction is recognized between those days and the present.

Another temporal clue is afforded by the transformation in the appearance of certain animals by *wísakedjak*. The latter made the kingfisher much prettier than he once was, shortened the tail of the muskrat, gave the weasel a white coat in winter, etc. Here again a temporal inference lies in the fact that there was once an epoch far distant in the past when the familiar animals of today had not assumed their contemporary characteristics.

Human beings, too, were not always like they are now, either in appearance or knowledge. Until *tcakábéc*, after being in the belly of a fish, was scraped clean by his sister, all human beings were covered with hair. All women once had toothed vaginas and, until *wísakedjak* by accident discovered the pleasure of sexual intercourse, no one knew about it.

The myth of the theft of summer likewise contains the assertion that at one time winter lasted all the year round.

The flood episode in one of the narratives of the *wísakedjak* cycle also has chronological implications since *wísakedjak* and the animals previously inhabited an earlier land mass. But the Indians themselves do not appear to follow through such temporal implications with reference to their mythological corpus as a whole. I found that they were not willing to commit themselves to any chronological relationship of the "flood," for example, and the adventures of *tcakábéc*, although they agreed to the obvious fact that the birth of the winds must have preceded the contest between the North and South Wind that appears in another myth. As a rule, however, the temporal sequence intrinsic to the events of each narrative is accepted without reference to the temporal sequences of any other narrative. Even the narratives of the *wísakedjak* cycle are not systematized chronologically. But I have received the impression that the narratives with anthropomorphic heroes like *kaíánwe*, *aási*, etc., are conceptualized as occurring on the earth in the post-fluvial epoch.

On the whole, then, events that are believed to have taken place "long ago" are not systematically correlated with each other in any well-defined temporal schema. They are discrete happenings, often unconnected and sometimes contradictory. Yet the past and the present are part of a whole because they are bound together by the persistence and contemporary reality of mythological characters not even now grown old.

III

For the Saukteaux, as we have seen, temporal orientation depends upon the recurrence and succession of concrete events in their qualitative aspects—events, moreover, which are indications, preparatory symbols and guides for those extremely vital activities through which the Saukteaux obtain a living from the country which they inhabit.

Durations, too, are interwoven with, and experienced as, events in all their individuality. Night is darkness, the stars and their movements, sleep, quietness. Day is the light, the journey of the sun across the sky, the round of domestic duties. A “moon” is the waxing and waning of the moon which occurs when, for instance, the wild rice is being gathered, when activities spanning a number of days are pursued. Any comparison of such durations must be by metaphors and not by exact measures.

Ideas of speed and magnitude necessarily belong to the same category. The Saukteaux are confined to gross time estimates and relatively simple qualitative judgments about speed based upon the observation and comparison of objects in their immediate environment. It would be impossible for them to measure the rate of moving objects at all. Any idea of length of time must be confined to extremely narrow limits. Just as they will reply to the query: How many children have you? by naming them, a direct request for the number of “moons” will result in the naming of them one after another. An answer to: How long ago? becomes: When I was a child; when my father was young, and so on.

All these means of temporal orientation are *local*, limited in their application to the immediate future, the recent past, immediate activities, phenomena known and dealt with in their own environment. Beyond these all is vague and loosely coordinated temporally.

In western civilization similar undifferentiated experience of time remains, but it is also transcended by abstract quantitative measures which enable us to think far differently about it. We can think in terms of abstract units of temporal duration: of a day in terms of *hours*, detached from the phenomena themselves, or of a month as a variable unit of time made up of a certain number of days.

Time conceived in this abstract fashion, in continuous and quantitatively defined units, is the basis of an intellectual order of temporal concepts available for use as a standard of reference, or measurement, for all classes of events. Time assumes for us an autonomous character and we are free to manipulate temporal concepts instrumentally, without constant reference to specific events. Thus we can think of it as infinitely divisible, a means for coordinating activities of all sorts with great precision. It likewise makes

possible the measurement of exact temporal intervals and the rate and speed of moving objects.

These contrasting differences in the temporal orientation of Saulteaux culture and of western civilization undoubtedly imply profound differences in the psychological outlook which is constituted by them. Such differences are not functions of primitive mentality or racial make-up. They are a function of culturally constituted experience. In these terms our temporal orientation in western civilization is likewise a function of experience in a cultural tradition with radically distinct patterns and entirely different historical roots.

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA, PA.

REPORT

AMERICAN ETHNOLOGICAL SOCIETY

The annual meeting of the American Ethnological Society was held January 25, 1937 at the American Museum of Natural History, New York City. The following report of the Secretary was read and adopted.

REPORT OF THE SECRETARY

Membership:

1936—Life members 14, members 95, fellows 121, affiliates 3 233
1937—Life members 15, members 128, fellows 124, affiliates 3 270
(1936 figures as of February 1936; 1937 figures as of the date of this report—January 25, 1937.)

For the year 1936, 17 members, 19 fellows, and 1 life member were added. For 1937, as of this date, 15 new members, 19 new fellows, and 1 life member have been added. During the year 16 members and fellows were removed from the rolls, including 5 resignations and 11 dropped.

The Society lost through death during the year Albert B. Reagan, a member since 1925.

In addition to the increased membership, the most marked change is in the proportions of members and fellows. The former category is now for the first time the larger. This has resulted from the addition of new members, but also from a considerable number of changes of status from fellow to member

Meetings:

Regular meetings of the Society were held at the American Museum of Natural History in conjunction with meetings of the Section in Anthropology of the New York Academy of Sciences. The following programs were presented:

February 24, 1936 Past and Present in Southeast Africa. Heinz Wieschoff.

March 23, 1936. Problems of Indonesian Ethnology. Raymond Kennedy.

April 27, 1936. Music of Bali. Colin McPhee.

October 26, 1936. Research Leads in American Anthropology. W. D. Strong.

November 23, 1936. Anthropological Problems in the American Northwest. Melville Jacobs.

January 25, 1937. Archaeology of the Valley of Mexico. George C. Vaillant.

Publications:

Caddoan Texts, by Gene Weltfish (Volume 17 of the Publications) has been unfortunately delayed, but should be issued shortly.

The Editor's proposal to print a volume of Arapesh by Reo Fortune as Volume 18 has been approved by the Executive Council.

Sales of publications the past year have been disappointing in view of continued circularizing. It is believed the market has for the most part been reached at present, and that renewed circularization should await the issuance of further numbers and possibly the appearance of a new series of Monographs of the American Ethnological Society which has been approved by the Executive Council and now awaits the approval of the members of the Society.

Respectfully submitted,

ALEXANDER LESSER, *Secretary*

The following report of the treasurer was read and referred to an auditing committee of Clark Wissler, *Chairman*, George C. Vaillant, and Edward Kennard.

REPORT OF THE TREASURER
January 22, 1936 to January 21, 1937

CURRENT FUND

On deposit, Corn Exchange Bank, January 22, 1936. . .	\$1,638.16
Current funds, Bowery Savings Bank, January 22, 1936	750.00
Petty cash on hand, January 22, 1936 . . .	2.03

Receipts: \$2,390.19

Dues, 1932-35, Members and fellows	\$ 71.10	
1936, Members and fellows	1,043.90	
1937, Members and fellows	221.00	
1938, Fellow	6.00	
Life membership	100.00	\$1,442.00
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Interest, Bonds of N. Y. Mortbon Corp.	63.74	
Interest, Bowery Savings Account	43.02	
Gross sale of publications, Stechert	121.10	
Check issued, uncollected	5.00	1,674.86
		<hr/>
		\$4,065.05

Expenditures:

American Anthropological Association, dues	\$1,074.75	
J. J. Augustin, on account of Publications, Vol. 17	200.00	
Charged to permanent funds for life membership	100.00	
G. E. Stechert Co.:		
Commissions	\$ 21.54	
Charges for shipping, etc	17.07	
Credited on account 1935 overpayment	82.49	121.10
<hr/>		
Expenses of meetings, American Ethnological Society.	66.60	
Printing circulars and stationery	76.20	
Postage	46.14	
Supplies and miscellaneous	2.11	
Clerical assistance.	43.50	
Safety-box rent	6.60	
Bank-charges, collections	1.15	
Cable charge	3.02	
Refund	1.00	\$1,742.17
		<hr/>
Balance, Corn Exchange Bank, January 21, 1937.	\$ 775.83	
Current funds, Bowery Savings, January 21, 1937 . .	1,543.02	
Cash on hand	4.03	\$2,322.88
		<hr/>
		\$4,065.05

PERMANENT FUND

Bonds of New York Mortbon Corporation..	\$2,550.00
Permanent funds, Bowery Savings Account.	1,150.00

 \$3,700.00

Respectfully submitted,

ALEXANDER LESSER, *Treasurer*

The Nominating Committee presented the following slate for 1937:

President: George C. Valliant

Vice-Presidents: Elsie Clews Parsons, George Herzog

Secretary-Treasurer: Alexander Lesser

Editor: Franz Boas

Directors: Clark Wissler, Ruth F. Benedict, Clarence L. Hay

The Society instructed the Secretary to cast a ballot for these nominees.

The Secretary presented a proposal, which had been approved by the Executive Council at a special meeting, December 23, 1936, that a new series to be known as the Monographs of the American Ethnological Society be established, supplementing bi-annual volumes of the Publications of the American Ethnological Society, with annual monographs in the general field of ethnology. Motions were adopted approving the new series, and referring questions of the selection of an Editorial Board and practical details of publication to the Executive Council.

The Society voted to amend Article IV, Section I of the Constitution of the Society (see *American Anthropologist*, Vol. 19, p. 110, 1917) to read:

"The annual dues of members shall be nine dollars (\$9.00). The annual dues of fellows shall be six dollars (\$6.00)."

Other changes in the Constitution of the reading of Article III, Section 3 (*ibid.*) and Article IV, Section 3 (*idem.*, p. 111) were proposed, and referred for a vote to a later meeting in the spring of 1937.

A proposal to change the date of the annual meeting from January to November, in order to coincide more closely with activities of the American Anthropological Association, was considered, but postponed, in order to give due notice to all members of the Society, as explicitly required by the Constitution.

ALEXANDER LESSER, *Secretary*

BOOK REVIEWS

NORTH AMERICA

The Eskimos. KAJ BIRKET-SMITH. (vii, 250 pp., 32 pls., map. \$5.00. New York: E. P. Dutton, 1936.)

Dr Birket-Smith's volume is another illustration of the fruitfulness of his method in handling the extensive data now available concerning the life of peoples of the North. The distinction accorded him by scientific bodies of Europe as a scholar and an explorer, through the publication of his many reports and monographs on Eskimo life and the country, will be applauded by his American readers as this last contribution becomes more widely known. The eulogy upon the author's standing in explorational science, so candidly phrased in the Foreword of the book by Diamond Jenness of the National Museum of Canada, is well merited.

In this volume Dr Birket-Smith has reached a culmination in his series of contributions which have built up so extensively a knowledge of the Eskimo people whom he understands so well and intimately. His effort has been to compress within the bounds of a general treatise of library size the essential features in the life of a race primarily important to students in all branches of the social sciences. To review the contents of the descriptive section is quite unnecessary for ethnologists who would need to consult the monograph directly for matter bearing upon the special interests which all entertain.

A wealth of data is provided, amounting to a thesaurus of sources for the investigator of modern problems of Eskimo administration, showing the sharp contrasts in Scandinavian, Canadian, and American methods of dealing with native minority groups from the standpoint of ethnopolitics.

In respect to the question of priority of cultural typology in Eskimo history, Birket-Smith still stands where he cast his dice in 1930, namely with the advocates of the belief in the earlier inland, caribou-hunting, ice-fishing stage. In support of this earlier decision he now adds the accepted evidence of Shapiro's discovery of apparent racial similarities between typical Eskimo and Chippewyan. He regards them as indications that the Eskimo at some time in the past were "dominant in the Athabaskan and Great Slave Lake region."

One of the most fertile and suggestive chapters is that concerned with the origin and development of Eskimo culture. In the brief compass of some twenty odd pages the author ranges over the whole field of theory in this complicated and exacting field and draws such conclusions as seem justified at the moment. Not the least interesting of the opinions he advances is that which would resurrect certain aspects of the theory of Dawkins, Sollas, and others as to the possible affinities between Magdalenian and Eskimo culture. Wisely pointing out that the racial and cultural aspects of the problem deserve separate treatment, he yet insists, and with justice, that once the ancestors of the Eskimo are seen in their true light as the bearers of an inland hunting culture, their later specialized aspect may be discounted. Thus their original culture can be made to fit in more readily

with the main features of an old and widely distributed boreal complex from which, in time, many specialized developments have arisen.

In the light of recent developments post-dating this volume, one cannot help but look forward impatiently to the time when the recent finds in Alaska, and the Folsom complex now known from the high plains, may be seen in their proper perspective in this picture. Particularly is this true in view of Howard's recently discovered evidences of bone artifacts associated with the Folsom industry; artifacts at least suggesting Eskimo methods of splicing shafts and foreshafts. It is possible to multiply theories as to the place of certain of these early traits in the old and generalized circumboreal cultures, but the reserve and the caution of Dr Birket-Smith are evidence of his sound scholarship. There are, as yet, too many missing links in the chain, which, when filled, is likely to reveal the Folsom complex itself, for example, to be a specialized development out of previous generalized forms whose affinities, however, quite possibly lie in a northerly direction.

At the risk of wandering far afield the reviewers express the hope that accumulating information may lead Dr Birket-Smith to break his customary reserve on some of these more nebulous and distant problems. In the meantime, a perusal of this careful and authoritative work should prove of benefit to all students of circumboreal and related problems, both as they concern the specialized cultures of the later Eskimo and those broader reaches of cultural history which lie back of them.

F. G. SPECK

L. C. EISELEY

UNIVERSITY OF PENNSYLVANIA

Cheyenne and Arapaho Music. FRANCES DENSMORE. (Southwest Museum Papers, No. 10. 111 pp., 7 pls., 1 fig., 72 songs. \$1.00. Los Angeles: Southwest Museum, 1936.)

Miss Densmore here publishes a collection of seventy-two songs recorded among the Southern Arapaho and Cheyenne now living in Oklahoma. Most of the songs in the categories of war, the Sun Dance, curing, societies, dance, and peyote share the general Plains style.¹ The presence of the Great Basin paired style² in both hand game and Ghost Dance is not surprising, for it seems that these institutions had already fused among the Arapaho before they passed the complex on to the Pawnee.³ Paired patterns in other songs (e.g., Nos. 58, 61, 72), as well as numerous "doubtful" cases (see particularly Grass or Omaha Dance, and "gift" songs), show that there may have been a considerable infiltration of the foreign style. The intensity

¹ George Herzog, *Plains Ghost Dance and Great Basin Music* (American Anthropologist, Vol. 37, pp. 403-19, 1935).

² Herzog, *op. cit.*, pp. 409-10.

³ Alexander Lesser, *The Pawnee Ghost Dance Hand Game* (Columbia University Contributions to Anthropology, Vol. 16, 1933).

of the Arapaho Ghost Dance may have favored deeper penetration. Possibly some of these songs are part of a hitherto unexamined Ghost Dance renaissance comparable to the Pawnee situation. These songs would then actually be more recent than would appear from their subject matter, and would probably share the Ghost Dance style.

As the number of recorded and published songs from different tribes increases, we are able to examine the interesting problems of musical growth and the integration of a given song into different tribal styles. Individual variability in the abilities and style of the singer, and also in the quality of the different transcriptions are difficulties, but are not insuperable. A case in point is No. 35, an Arapaho Ghost Dance song, given by Curtis⁴ (p. 208), while Mooney gives the text⁵ (p. 1030) though as a Cheyenne song. Curtis' music parallels exactly the Mooney text, both showing clear *aabb* structures. The Densmore version shows a musical structure changed to *a¹ba²a²ba²a²b*, possibly a change in text pattern, and considerable melodic and rhythmic modification. In the thirty years between recordings there may have been a drift away from the Great Basin style towards a more Plains-like form. On the other hand, we should not overlook the possibility that thirty years ago there existed a more Plains-like variant with which Miss Densmore's version should be equated. Compare also No. 33 with Mooney (p. 958), No. 1. If we are considering the same variant, and text, position in the Ghost Dance ritual as opening song, formal structure, and general movement imply that we are considering a single song, melody and rhythm have then varied practically beyond recognizability. The text of No. 34 is given by Mooney (p. 975). Although there is no necessary correlation between text and music, the perfect parallelism of his words in contrast to the imperfect parallelism of certain parts of the music may be the result of musical growth.

With a sketch of the peyote ceremony introduced by the Kiowa, Miss Densmore gives several of the fixed songs. No. 50 (Curtis, p. 188), the opening song, when compared with its Kiowa version,⁶ shows simplification but no great modification. A comparison of Nos. 51, 52, 54 with Kiowa renditions shows, however, very great divergences in pitch level, structure, rhythmic variety, and general movement, and particularly in expansion of range and increased ornament among the Kiowa. It may be that these three songs are not really to be equated. Miss Densmore recorded only the first of certain groups of four songs sung at particular points in the ceremony. My informants stated that except for the opening song, it was the terminal song in each group that was fixed. The first three might be any three peyote songs. Therefore the terminal songs of the Cheyenne and Arapaho groups might show greater similarity to the Kiowa equivalents.

⁴ Natalie Curtis, *The Indian's Book* (1st ed., New York and London, 1907).

⁵ James Mooney, *The Ghost-Dance Religion and the Sioux Outbreak* (Fourteenth Annual Report, Bureau of American Ethnology, 1896).

⁶ Unpublished songs, recorded by the reviewer.

Miss Densmore draws on the published literature to add to her sketch of the ethnographic setting of the songs, and to her description of the instruments used. We are grateful to her for this welcome contribution to our knowledge of Plains music.

JANE RICHARDSON

NEW YORK CITY

Rediscovering Illinois: Archaeological Explorations in and around Fulton County.

FAY-COOPER COLE AND THORNE DEUEL. (University of Chicago Publications in Anthropology, Archaeological Series. xvi, 295 pp., 36 pls., 37 figs. \$2.00. Chicago: University of Chicago Press, 1937.)

Rediscovering Illinois deals with archaeological investigation in and around Fulton County, located in west-central Illinois. It combines an appeal to the interested intelligent layman and a report to the specialist, with a natural leaning toward the latter. The first three chapters (Introduction, the Survey-Methods of Excavation, Classification of Cultures) tell in simple, clear terms the why and how of archaeological study, and the basis of the recognition of major archaeological patterns and their subdivisions.

The bulk of the volume is devoted to a complete and technical analysis of each site excavated and studied by the University of Chicago field parties, 1930-32. The central theme is the identification of the sites according to the terminology generally accepted: pattern, phase, aspect, focus, component. There are special chapters on classification of pottery and projectile points, classificatory method (Deuel), human crania and faunal remains (G. K. Neumann), and shells (Frank C. Baker). This comprehensive treatment has rendered the report of value in the classroom: the student of North American archaeology has now at his command a text outlining method and procedure in careful and logical exactness.

Two patterns are recognized and described in detail, based on an earlier report by Deuel.¹ These are the Mississippi and the Woodland, and their detailed manifestations in Fulton County are recorded for each site. This cultural analysis is extended in the discussion of the classificatory method as it applies to the Mississippi Valley generally. The Mississippi pattern is divided into two phases: Upper and Middle Mississippi. These in turn are classed according to their several aspects and the latter are then reduced to distinctive foci.

The application of the taxonomic method to archaeology is an important advance in recovering order from chaos. Admittedly there have been far too many "cultures" based upon individual sites, and described by investigators whose knowledge of comparative data has been inadequate. A taxonomic framework, as it were, admits of classification in terms of inter-relationship. But herein lies a certain amount of danger; the tendency to fit facts into a previous general scheme;

¹ *Basic Cultures of the Mississippi Valley* (American Anthropologist, Vol. 37, pp. 429-45, 1935).

to give variants or minor deviations an equal rank with distinct elements. Classification cannot pay too much attention to detail, as, for example—to borrow an analogy from the field of zoology—where subspecific or even specific rank has been given to pelage differences which proved to be naught but seasonal changes. Again, it has been lately shown in the field of comparative myology that a basic similarity outweighs an adaptive change. These same rules must apply to archaeological taxonomy: elements in and of themselves do not have diagnostic value except as measured against the larger complex.

In great measure the present volume has avoided these pitfalls. There is an evident plasticity in the report so that conclusions as to ultimate relationship are tentative, pending additional material. It might be added of the taxonomic method, based upon traits and trait-complexes, that the work of Griffin on percentages (archaeological data) and Klimek on elaborate statistical analysis (ethnological data) have placed it upon a firm basis. More important still there has been an attempt at the evaluation of variability so that an emphasis can be placed upon differences in kind rather than in degree.

The problem of sequence in Fulton County is facilitated by the fact that stratification was found in at least five mounds and two village sites. The tentative sequence is as below:

5. *Middle Mississippi phase* (Mississippi pattern)
4. *Hopewellian phase* (Central Basin, Woodland)
3. *Morton focus* (Central Basin, Woodland)
2. *Red Ocher phase* (Woodland?)
1. *Black Sand focus* (Central Basin phase, Woodland pattern)

The Black Sand appears to be the earliest culture, but there are slight indications that the Red Ocher may have been contemporaneous. The picture is complicated still further by the fact that certain traits in Black Sand are found as a continuum in Hopewellian. At all events the four earliest cultures are considered as Woodland. Middle Mississippi and Hopewellian may have overlapped in time.

The report on the crania gives detailed measurements and a summary for the material found at each site. The earliest Black Sand people were a long-headed type equated by Neumann with Hooton's "pseudo-Australoids" from Pecos. The Red Ocher people were round-headed, the Hopewellians middle-headed, the Middle Mississippi people were predominantly middle- and long-headed, and the Late Woodland people generally long-headed. As a whole the report on human remains is conservative to the point of inconclusiveness.

Rediscovering Illinois is the first of a series of reports in Illinois archaeology. It is concise but not dogmatic in its analysis. As the study of but a single county it does not presume to substitute the part for the whole. Co-workers await future reports with confidence in both content and conclusions.

W. M. KROGMAN

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Excavation of the Riana Ruin and Chama Valley Survey. FRANK C. HIBBEN. (Bulletin, University of New Mexico, Anthropological Series, Vol. 2, No. 1, 1937.)

Although a number of the larger sites in the upper Rio Grande area have been previously excavated, Mr Hibben's report is the first to record work in one of the smaller ruins. This will, without doubt, prove of considerable value in more clearly defining conditions in a ruin which had not been subjected to disturbances by later occupations.

In Part I, the author enumerates the various types of archaeological remains in the surrounding region, based on a preliminary aerial reconnaissance.

Part II presents an account of the excavation which has every appearance of having been carefully done and exactly recorded. An interesting feature described is a subterranean passage connecting a room in the living quarters with the kiva, an arrangement which has not hitherto been noted in this cultural area. There are, however, a few inaccuracies and inconsistencies to which attention should be directed as they are of primary import.

On page 40, Santa Fé Black-on-white pottery is classed as an intrusive trade ware. This is contrary to the evidence secured from a collection of sherd material obtained at this site (Laboratory of Anthropology, 920) a few years previous to excavation, which shows too high a percentage of that type, when compared to that of the accompanying Wiyo Black-on-white, to be considered in such a light. Such a view is even more inconsistent when it is recalled that the Santa Fé type is known to have been earlier in line of development. The possibility of two occupations appears to have been overlooked; all this would then tend to modify the statement that this ruin was a "pure" Wiyo site.

In a description of Yugeuinge on page 11, the author is very plainly referring to a small 15th century site that lies 0.2 miles north of the large quadrangular ruin which properly bears that name.

Of perhaps lesser importance is the repeated use of the terms flint and chert instead of chalcedony. The latter, being quarried in the immediate neighborhood, can be demonstrated to be the material most frequently employed.

A number of inferences of doubtful worth might better have been omitted. Such a one is the statement, on page 35, concerning the application of blood to pottery before firing, for which no information is given to substantiate a belief that such a custom existed.

Part III consists of a detailed study of the tree ring material by W. S. Stallings, Jr. In the table of dates on page 54, third line from the top, a date two centuries earlier than the others listed is incorrectly given. While checking this matter, the reviewer was informed that the author had not been furnished an opportunity to correct proof. An obvious lack of competent editing may be seen in several parts of the report.

Aside from a few such defects this paper furnishes a great deal of new and important information on a particular phase of Pueblo development in this region.

Pleistocene Man in Minnesota: a Fossil Homo Sapiens. ALBERT ERNEST JENKS. (xiii, 197 pp., 89 figs. \$7.50. Minneapolis: University of Minnesota Press, 1936.)

In an excellently executed volume Prof Albert Ernest Jenks has enriched the science of Physical Anthropology by a careful and systematic study of the fossil remains of what he considers a Pleistocene human upon American soil. The skeleton is that of a female of about fifteen years of age and so far as the constituent parts are concerned, remarkably complete however worn in various degrees. Judging from its deeply settled position in enveloping silt, it is assumed that the young woman was drowned in glacial Lake Pelikan, Minnesota, immediately east of the Big Stone moraine of the Wisconsin glacier.

In the present volume the find of the Minnesota "Man" is accounted for in the following sequence of treatment: Documentary Evidence, Pleistocene Geology, the Skeleton as a Whole (pp. 1-47); Craniology (pp. 48-112); Appendicular Skeleton (pp. 113-50); Accompanying Artifacts, etc. (pp. 161-69), and Conclusions, Appendix, Bibliography, and Index (pp. 170-97).

The evaluation of the find rests on its apparent geological age and the so-called morphological primitiveness. Under specific circumstances both complexes are fundamentally associable provided that the time distance is sufficiently large and can be substantiated by factual evidence as in human phylogenetic history. The geological evidence seems to be well-established in the present case and, if taken at its face value, could well serve the purpose of geological identification. Yet those familiar with geological conditions, particularly in regions of geophysical instability, into which the present situation seems to fit, are aware of the trickeries played by them. The second complex, i.e. the morphological habitus of the find, even if it were associable with indisputable reliability of the geological background, would not derive much evidential proof from the latter. Except for a few definitely recognizable type expressions of morphological inferiority (primatoid, australoid, eskimoid, etc.), distinction and evaluation on the basis of morphological primitiveness lacks systematic definiteness in the Western Hemisphere as compared with the Old World morphological standards spread over a vast period of phylogenetic development. With its long list of primitive characters, most of them definitely primitive in the sense of comparative morphology of the highest primate emancipation, Minnesota "Man" falls into the range of variation of recent Indian typology. This refers even to such strongly expressed features as prognathy where the facial angle in ear-eye orientation attains 87° . The reviewer's findings in this metrical quantity upon undeformed material from the North Pacific Coast (Jesup Expedition) covers the values from 77° - 88° in the female crania. In alveolar prognathy however Minnesota Man presents an exceedingly low angle of 65° which brings it in close association with the Australians. The morphologically primitive "Affenrinne," Macalister's orygom-craspedoty, so well pronounced in the skull under discussion, occurred in the reviewer's above mentioned material in 3.1 percent in the females in contrast to 1.5 percent in the males. The preservation of this feature as coupled with strong alveolar prognathy is doubtless a mark of primitive morphology. The significance

also of the protrusion of the superior occipital squama, in itself of primitive morphology, may in its evaluation be exceeded by the inial flexure in association with simple protrusion as it obtains, e.g., in the Punin and certain Eskimo crania.

It would lengthen the review unduly to discuss in further detail the morphological primitiveness in the various regions of the cranium and the other parts of the skeleton. Since, however, the condition of primitiveness in association with geological age is at issue, it seemed necessary to point out at least by some characters their relative degree of significance.

The problem of American human antiquity is replete with possibilities of identification, but also with disappointing fallacies. Since in the final analysis it is the morphological type which is of decisive significance, the associated geological conditions are of somewhat lesser importance. In the case of Minnesota Man one does not seem to be prompted by the other, nor does the typological status of the find for the same reason seem to justify the establishment of a Pleistocene American Man.

This criticism objectively arrived at after weighing the large array of carefully collected evidence is not meant to detract from the meritorious investigatory work of the author. Prof Jenks' work is exemplary and may well serve as a model for similar studies in the science of Physical Anthropology.

BRUNO OETTEKING

COLUMBIA UNIVERSITY

MEXICO AND SOUTH AMERICA

Milla: Town of the Souls. ELSIE CLEWS PARSONS (xix, 590 pp., 51 pls., 11 figs., 3 maps. \$4.00. Chicago: University of Chicago Press, 1936.)

In her preface Dr Parsons speaks of this analysis of the Zapotec village of Mitla as being primarily "concerned with acculturation, with what the Indian culture took from the Spanish" (pp. xii-xiii). In this endeavor Dr Parsons has done a good job, but the reviewer submits that she has too long a tradition of competent ethnographic work to do this only: rather is *Milla* a richly textured and thorough ethnography. A review is perhaps not the place to argue the merits of acculturation versus ethnography, but in the same paragraph Dr Parsons, after pointing out that the social product of Spanish-Indian contacts in Mexico is a new and changing blend, remarks that: "Nevertheless, ethnological interest may, in fact must, center on certain points or phases of change." Does not the heart of the problem lie in these words? If, as is common today, we deny complete stability to culture and minimize local invention in favor of external influences, is not every ethnography but a fixed point in a process of acculturation? At certain times and places the process of change and blend may be accelerated and dramatized by the meeting of highly contrasted cultures. Such is the case in Mexico and probably in all Latin America, where blending has occurred rather than the shock and disintegration or extinction more characteristic of our North American experiences. In such a situation processes

become accented and he must indeed be in a methodological strait-jacket who does not find it necessary to give more attention to processual phenomena than is customary in a picture either of more stable relatively untouched native communities or of a dying Indian culture of North America.

In point of method, Dr Parson's interest in acculturation takes very nearly the form of Spier's dynamic approach (if I have correctly understood the latter). To a large degree process or mechanism dominates Dr Parson's viewpoint throughout, although she has well recognized the elementary but often overlooked fact that description and comparison precede understanding of process. Consequently *Mitla* is comprehensive in its text and elaborately footnoted with comparative data. The latter are usually objectively presented, leaving the reader to draw what conclusions he will.

Mitla is of unusual interest in the Mexican field for several reasons. It is the first Mexican ethnography by a field worker thoroughly experienced in other areas, particularly the Southwest. This background has incidentally contributed penetrating insights into the functioning of many a seemingly elaborate Mexican institution and will do much to place the Pueblo cultures in an understandable relation with Mexico. It is, moreover, ethnography done as it should be, in leisurely fashion over several years with several visits. As a consequence, as far as I can judge from a ten-day visit to Mitla under Dr Parson's tutelage, she has reproduced the flavor and tempo of life in this unique and kindly village of farmers and traders with fidelity and sympathy. The 92 page chapter on "Town Gossip" is as unusual as it is illuminating. Finally, she has not shirked the onerous problem of Indian versus Spanish origins. She has made meticulous distinctions in a final chapter which is fraught with suggestion for workers in every field where culture is rapidly changing under external stimulus and most particularly for students of Mexico. In her concluding pages is presented a valuable summary reconstruction of Zapotec culture.

Dr Parsons is to be congratulated upon a work which will enhance her already enviable reputation, the University of Chicago upon adding to its valuable series of monographs on Mexico.

RALPH L. BEALS

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Nueva Corónica y Buen Gobierno (Codex Péruvien Illustré). FELIPE GUAMAN POMA DE AYALA. (Travaux et Mémoires, Institut d'Ethnologie, No. 23. xxviii, 1179 pp. 250 fr. Paris, 1936.)

The Institut d'Ethnologie of Paris has reproduced in facsimile the original manuscript, including the black and white drawings of the Peruvian mestizo, Poma de Ayala. The manuscript contains about 1179 pages and innumerable drawings many of which contribute nothing at all to the history of Peru but others of which are of great importance. It is dated as about 1613.

The original manuscript is in the Royal Library of Copenhagen. Its importance was announced in 1908 by Richard Pietschmann, who published several articles

about it, and other notes and comments have followed. Some of the most important illustrations from the ethnological point of view have been reproduced by Gösta Montell and by Philip Ainsworth Means.¹

In 1922 Mr Means examined the original manuscript and later published an article *Some Comments on the Inedited Manuscript of Poma de Ayala*.² In this article he evaluates the manuscript as an historical and ethnological document. Although its rating in history is not high, the ethnological commentary and illustrative detail is extremely useful. The comments made by Means in 1923 are of greater significance today with the facsimile of the manuscript available.

In the drawings of Poma de Ayala are many interesting scenes and depictions of the life of the times. The type of plow and its use are illustrated. Hunting and fighting scenes indicate the use of bolas, nets, clubs, shields, knives, and other weapons and tools. A long series of pictures of the Incas, their wives, and their leading warriors furnishes important information on the costumes used by males and females as well as class differentiation in costumes. Montell has already utilized some of the leads on spinning and weaving technique as well as the costume information. L. Leland Locke has written an article *The Ancient Peruvian Abacus* commenting on the drawings of quipus and a calculating device.³ Other drawings furnish information on the treatment of mummies, burials, and burying places. Close examination of the pictures and translation of the bilingual text will undoubtedly prove of great benefit to the Peruvian scholars.

Much of the book is devoted to religious themes which pertain to the life of Christ and the rise of the church, and still another section relates incidents and illustrates activities of the Colonial period of Peru. In all, however, the reproduction of this manuscript provides a rich source of new material for ethnologist and historian.

WENDELL C. BENNETT

AMERICAN MUSEUM OF NATURAL HISTORY

Archaeological Researches in the Department of La Candelaria (Prov. Salta, Argentina).

STIG RYDÉN. (Ethnologiska Studier, 3, pp. 5-329, 150 figs. Goteborg, 1936.)

Stig Rydén reports on the results of his archaeological research during 1932 in the Department of Candelaria of the Argentine Province of Salta. His work consisted of the examination of a large number of sites rather than any intensive excavations. Because of the fragments of pottery everywhere, Candelaria appears extremely rich archaeologically speaking but evidences for stratigraphy are lacking. The collections from Candelaria are all more or less of the same character and typically comprise clay vessels, stone mortars and pestles, grooved axes and adzes, and shell beads. Occasionally one finds clay pipes, figurines, bone objects, or a piece

¹ *Dress and Ornaments in Ancient Peru* (Goteborg, 1929); *Ancient Civilizations of the Andes* (New York, 1931).

² *American Anthropologist*, Vol. 25, pp. 397-405, 1923.

³ *Scripta Mathematica*, September, 1932, pp. 37-43.

of bronze. It remains uncertain who were the agricultural people responsible for Candelaria culture but Rydén believes them earlier than the Diaguitans or the Incas.

Rydén's work was instigated by Alfred Métraux and aided by the late Baron Erland Nordenskiöld whose influence it shows. After a well-written introduction, there follow running descriptions of sites, interspersed with pleasant comments on the country and frequent discussions of distributions and technology. A longer section deals rather meticulously with the various artifacts. Good illustrations include a colored plate. There is little doubt but that this work will prove extremely useful to those working in the general field of South American archaeology as well as to specialists on the Argentine.

CORNELIUS OSGOOD

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Ethnological Studies 1, 2, 3. WALTER KAUDERN (ed). (Göteborg: Göteborgs Museum, 1935, 1936.)

This new publication of the Göteborgs Etnografiska Museum, edited by its Director, Dr Walter Kaudern, is in the format, and of the typographical excellence of Nordenskiöld's *Comparative Ethnographical Studies*, and may be considered a continuance of these. Unlike the latter, however, each issue contains several articles which may be entirely unrelated and by different authors; it contains studies on regions other than South America, and is to appear once or twice a year. Three parts have appeared to date: No. 1 in 1935, Nos. 2 and 3 in 1936. Naturally, because of this Museum's special interests, the majority of the articles that have appeared to date are concerned with South America, and the present review includes only these. Seven of the ten articles are upon this region; the other three are two by Kaudern on Celebes and one by Santesson on arrow-poisons in Burma and Yunnan. Seven of the ten are in English, three in German.

The largest and most important article is that by Stig Rydén, *Archaeological Researches in the Department of La Candelaria (Province of Salta, Argentina)* in Part 3, pp. 5-329, with 150 illustrations. It is an excellent monograph, quite in the Nordenskiöldian tradition, showing wide acquaintance with the literature and early sources for the region, and with the archaeology of other parts of America. It is a thorough study of a little-known region, and a very important contribution. La Candelaria is in the southeastern portion of Salta on the border of the Gran Chaco and the Andean foothills, east of the Calchaqui-Diaguite region. It is very rich in archaeological remains. Many sites were excavated and reported on in detail, and other collections made in the same region were studied. The finds resemble most those from Pampa Grande reported by Ambrosetti. The dependence on maize places the area in the Peruvian culture sphere, closely related to Diaguite, but older than the Diaguite of the horizon of Inca influence. The people were probably not the Guarani, as has been suggested. The closing chapters refer to modern pottery-making and modern grain-storage houses, and there is an appendix on the skeletal remains and a large bibliography.

Another archaeological article is that by Henry Wassén, *An Archaeological Study in the Western Colombian Cordillera* (Part 2, pp. 30-67). Though a smaller report on less extensive work, the excavations and the objects secured are carefully described, but no important conclusions suggested. The region is between Buenaventura and Buga in the *tierra templada* of the Valle del Cauca. The graves here are mainly deep shafts with a chamber at the bottom; some over six meters deep were found and there are reports of others up to seventeen meters. Their makers probably belonged to the Gorron or Timba tribe.

Ethnological studies include a short article on *Skalpierung bei den Tobaindianern* by Stig Rydén (Part 1, pp. 26-34) and *Notes on Southern Groups of the Chocó Indians in Colombia* by Henry Wassén (Part 1, pp. 35-182). The latter is not presented as a complete monograph, since Wassén spent only a short time with two Chocó groups, but the information is valuable and well presented, with the important comparative studies, references to early sources, and conclusions that we expect from the Nordenskiöld school. The Chocó were apparently originally a fluvatile, not a maritime people; their closest connections were probably with the now extinct Muzo and Panche who were the enemies of the highland Chibcha, and probably themselves of lowland culture. The Chocó show many elements in common with the tribes of northwestern Amazonia as opposed to the peoples of the highlands.

Wassén devotes considerable space to the question of arrow- and fish-poisons, especially to pacuru, a cardiac poison very different from curare. Further data on the same subject are given by C. G. Santesson in *Pfeil- und Fischgift aus Kolumbien und Ekuador* (Part 2, pp. 15-29). These gave rise to two other articles, *Arrow-poisons and Narcotics in Western Amazonas (A Reply to Dr. H. Wassén)*, by Rafael Karsten (Part 2, pp. 68-77), and *Some Observations on South American Arrow-poisons and Narcotics (A Rejoinder to Professor Rafael Karsten)* by C. G. Santesson and Henry Wassén (Part 3, pp. 330-58). The discussions are acrimonious, controversial, and polemic, and seem to me to be out of place in a scientific series; the points at issue are trivial compared with the amount of space devoted to them.

The translations from Swedish, at least those into English, are very well done, errors in orthography and faults in syntax few and excusable.

J. ALDEN MASON

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Suriname Folk-Lore. MELVILLE HERSKOVITS AND FRANCES S. HERSKOVITS. (Columbia University Contributions to Anthropology, Vol. 27. xx, 766 pp., 27 pls., 7 figs. \$5.00. New York: Columbia University Press, 1937.)

This voluminous work chiefly of two American scientists, husband and wife, mentions that the fieldwork from which the material in this volume derives was carried on in Dutch Guiana during the summers of 1928 and 1929. The book which is dedicated to Franz Boas, is, except for the Bush-Negro proverbs and songs, concerned with the Negroes of the city of Paramaribo, where the greatest portion of the ethnological information and the tales were gathered. Both field trips, made

under the auspices of the Columbia University Council for Research in the Social Sciences and of Northwestern University, were made possible by the generous support of Dr Elsie Clews Parsons, who gave the initial indication of Suriname as a fruitful area for research into African survivals in the New World. The authors express their deep appreciation to Dr Parsons, as well as to Professor Boas; to the latter for the endorsement of the Suriname project and counsel on method, and for much more, above all for the inspiration of his work.

The general impression which the book gives is one of astonishment and of admiration; the work is of great interest to all ethnologists and specially for students of the culture of the American Negro. We see in the "References" at the end of the book that Dr Herskovits has already described the *Social Organization of the Bush Negroes* (Twenty-Third International Congress of Americanists, New York, 1928) and that he and his wife wrote on religious belief and tales in Dahomey and Ashanti in Africa, countries which they visited in order to be able to undertake the publication of this work on Suriname. They had the experience, the ability, and the knowledge to perform what they have done.

The work is divided into three parts. Part I contains notes on the culture of the Paramaribo Negroes; Part II, Stories, Riddles, Proverbs, and Dreams. Part III, Music, is subdivided into a General Statement, Musicological Analysis, Ethnological Evaluation, Musical Instruments, Notes on the Recording of Songs, Bush-Negro Songs, Town Negro Songs, and Songs from Haiti. Dr Kolinsky gave extreme care to his excellent musicological studies, which took some years in preparation.

I presume that much that has been found by the authors was unknown to scientists and for the most part unknown to other civilized inhabitants of Suriname, the missionaries included. The researches of the authors bear the mark of real discoveries about the soul and the mind of the Black Man. Is this possible, people may ask, after seventy-five years of Christianization? Indeed, so it is, and this book gives the real situation.

The Netherlands State, and its interesting section, the country of Suriname, are much indebted for what has been wrought by American science, diligence, and knowledge, and for all the excellent work which the authors have done.

L. C. VAN PANHUYS

THE HAGUE, NETHERLANDS

AFRICA

Baluba et Balubaisés du Katanga. EDMUND VERHULPEN. (534 pp. 150 fr. Anvers, Belgium: L'Avenir Belge, 1936)

Baluba et Balubaisés is a study of the whole aggregate of Luba-speaking peoples now inhabiting the Katanga province of the Belgian Congo, together with notes on a number of the kindred peoples living to the northeast of the district towards the shores of Lake Tanganyika, or on its eastern and southern boundaries (N.E., and N.W. Rhodesia respectively). The book thus covers a wider field than P. Colle's work on the Baluba, which is mainly concerned with the eastern group of these Congo tribes (Luba-Hemba in his terminology). Verhulpen's study is based on an exhaustive survey of the existing material on the area with the addition of notes

collected on the spot by the author, who was himself an administrative officer in this region for some years. It is thus a very valuable contribution to the series of studies of the Central Bantu peoples now being published under the direction of the Université Coloniale de Belge in collaboration with the Museum at Tervueren near Brussels.

One of the author's objects is to provide a sort of handbook for Government officials now administering the territory under a policy which resembles that known in British colonies as Indirect Rule. Some of the copious genealogies and lists of titles of chiefs of the different tribes will in fact be of value chiefly to officers with this special interest. But as a contribution to the history of Central Africa the book raises wider issues. It sketches the probable movements of peoples through this part of the Congo from the days of the Broken Hill man and his contemporaries to those of the Bushmen and the different Bantu invaders from the earliest times down to the end of the 19th century. Verhulpen's special problem is, of course, to trace the origins of the present Katanga population which he believes to have resulted from successive invasions from the north, first by a patrilineal group with affinities to the present Basonge, and next by the ancestors of the matrilineal Babui. These different Luba-speaking peoples he believes to have been united under strong military rulers to form two empires, the first broken up by the second in about 1585. The evidence for these historical reconstructions consists largely of native traditions as to tribal origins together with genealogies of different chiefs, by which the dates of probable historical events are fixed, often to the nearest ten years. The author also discusses certain linguistic features, particularly the use of the noun prefix, which he considers diagnostic of different ethnic groups, and looks for the presence or absence of two or three cultural traits such as patrilineal or matrilineal organization or the attitude to the Supreme Being and the Land Spirits, which he uses as a further means of classifying the different tribes and sub-tribes now loosely united under the title "Luba."

It must be confessed at once that though Verhulpen's hypotheses are suggestive and his main contention that these Luba-speaking peoples came in successive waves from the north is convincing, yet some of the evidence he gives us for his more detailed reconstruction of migrations in this area is often rather thin. It is difficult to be clear from the text where the author is giving native traditions of origin collected by himself or statements taken from other writers, nor does he indicate the distribution of any such traditions over a given area or the type of informant questioned. It is almost impossible, too, to assess the historical value of tales of migration or lists of chiefs' titles without more social context than the book gives. This fact seems to me to be of particular importance when dealing with the history of immigrant tribes who have built up kingdoms by military conquest. Working in a contiguous area with many cultural similarities (the territory of the Babemba of Northeastern Rhodesia classed by Verhulpen among the "Lubaised" peoples) I found that variations in the legend of origin and the detailed traditions of occupation of the country were often of the utmost significance as bases for claims of the conquering groups to own land, rights over economic resources, ceremonial privilege, and spiritual powers. The same was also true of the small sub-

divisions of the kindred Baushi and Baunga now inhabiting islands in Lake Bangweulu. Nor do I believe that the genealogies of chiefs can be used to date events in an area such as this without a rather intensive first-hand study in the tribe concerned. Verhulpen gives a table of seventeen paramount chiefs of the Babemba, presumably collected from Congo natives, whereas the seven different versions I noted in the Bemba country itself varied from twenty-seven to thirty-one names all differing in important particulars from that used as a basis of calculation in this work. I believe in fact that a history as complex as that of Central Africa can only be built up gradually by means of intensive studies of contiguous areas, if indeed it can be reconstructed in a scientific manner at all. Verhulpen's interesting analysis of the previous theories of migration in this area seem only to add further evidence that the time is not ripe for such large scale reconstructions as he has undertaken in this work, for previous authors working with much the same material, have placed the origin of the Baluba alternatively in South Africa, Uganda, and the Lake Chad region.

It is unfortunate, too, that in reckoning the presence or absence of certain cultural traits as signs of tribal affinity the author should not have given us more exact definitions. The distribution of matrilineal and patrilineal peoples in Central Africa, and in particular of matrilocal and patrilocal marriage, is exceedingly interesting from a comparative point of view. But if we are to use this information to classify the peoples of the Belgian Congo these traits will have to be defined far more exactly. Such statements as: "Une assez grande liberté de femme" will not help us. Nor in an area where so much has been written on the attitude of Bantu peoples to the High God is it sufficiently exact to say of a people: "L'être suprême est connu à peu près de la même façon pour le plupart de ces peuples." The fact is that for many tribes in the Congo the existing material is insufficient to allow of a more accurate analysis.

In spite of these *caveats* readers will be grateful to Verhulpen for his enterprise, and the industry with which he has mustered the existing information on the area, as well as for some new material on the ceremonial aspect of chieftainship in the Congo, its insignia, mortuary ritual, and the hereditary officials connected with the court. His account of the relationship between local ancestral deities and individuals believed to be possessed by the latter (Vidie) throws new light on one or two characteristic features of Central Bantu religion. The linguistic tables comparing the different forms used in Luba and the kindred dialects will be of great interest to African specialists, and the tribal map of the whole area I have found invaluable.

A. I. RICHARDS

LONDON SCHOOL OF ECONOMICS

Reaction to Conquest. MONICA HUNTER. (xx, 582 pp., 28 pls., 4 maps. \$10.00. London: International Institute of African Languages and Cultures, Oxford University Press, 1936.)

The Pondo (Ama-Mpondo), a group of the Xosa Kafirs of southeastern Africa, inhabiting the coast land stretching away northeast from East London, were the

last of the tribes of the Cape Province to lose their independence (having come under Colonial administration in 1894). They number about 260,000, most of whom are agriculturists (with the plough as the successor of the digging-stick and the hoe) and cattle-breeders in a native reserve, while a proportion of them get their living in the local towns, or by working on farms as laborers. These three categories have all been carefully studied by Dr Hunter, but the greater part of her book she devotes to native life in the reserve. She took up this work of hers with especially great qualifications, being herself the daughter of a South African missionary and speaking the Xosa language, besides which there is also her scientific training. And, accordingly, an excellent work has been the result.

Her book, it seems to me, is first and foremost of sociological importance, but here I nevertheless propose to regard it from an exclusively ethnographical point of view. Even the ethnologist will find a great deal to learn from the book, in which the author has preserved what there still is left of the old culture that is now rapidly vanishing in spite of some ninety percent of the Pondo being said still to be pagans. Her description of life in the reserve is in certain departments so detailed and systematic that few monographs on African peoples will be found to equal it. This work will no doubt before very long become a classic. The only section that has been rather scantily dealt with—and here is where the ethnographer's point of view comes in—refers to certain aspects of the ancient material culture. Even though hunting and fishing may have lost their old importance, it would have been valuable to have obtained exact information as to the types of traps, fish spears, etc., that are still in use. The same may be said of the ancient type of hut—nowadays only occurring in the form of temporary shelters—the blacksmith's bellows, the sewing "needle," the penis sheath, and the "sacrificial spear" used for ritual killing. Is the latter a specific type of spear? And, is the rare shell, *iyila*, that the coast Pondo trade with the Xosa (p. 134) a plate from the *Conus* shell, or not? (This is an interesting point for the study of the distribution in South Africa of this shell ornament.) As a contribution to our knowledge of water burial in Africa it would be of further interest to learn something more as regards the burial of a certain chief at the bottom of a certain pool (p. 260). There presents itself also the question whether any specific significance attaches to color, in cases where this is given, in regard to animals and the like that are sacrificed (black bull sacrificed, p. 252; black beast for rain magic, p. 80; black sheep, pp. 77, 297; white goat, p. 331). It is interesting to learn that mutilation of the little finger still exists: Dr Hunter gives instances (pp. 264, 538) in valuable supplementation of earlier data as regards the occurrence of this custom in South Africa (cf. S. Lagercrantz' paper on finger mutilation in Africa, in *Zeitschrift für Ethnologie*, 1935).

To the bibliography I would add Kropf, *Das Volk der Xosa-Kaǀfjern* (Berlin 1889).

In making the above brief reflections I by no means wish to detract from the value of Dr Hunter's brilliant researches. I am fully alive to the fact that her book has run into such bulk that it could not very well have been made any larger in a single volume. Besides which she has not yet, by far, published all her material. May I be pardoned if I express a wish that at some time she may have the oppor-

tunity of working up her material also ethnologically, of giving a more detailed description—preferably accompanied by illustrations—of such material culture elements as those I have indicated above, and then, if possible, of coordinating both these elements and certain of the more important and typical details of the spiritual culture with the cultures of the neighboring tribes and peoples. Information as to Hottentot influence, if any, among the Pondo would also be welcomed.

Lack of space forbids me to enter more fully into Dr Hunter's book. In conclusion I will again emphasize that not only sociologists and native administrators, but also ethnologists, have every reason for being grateful to the author for her book, while their thanks are also due to the International Institute and the Carnegie Corporation, the institutions that have enabled her to carry out this exemplary piece of research.

GERHARD LINDBLOM

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Black and White in East Africa. RICHARD G. THURNWALD. (xxii, 419 pp., 16 pls. 21s. London: George Routledge and Sons, 1935.)

Die Glidi-Ewe in Togo. Züge aus ihrem Gesellschaftsleben. DIEDRICH WESTERMANN. (Mitteilungen des Seminars für Orientalische Sprachen an der Universität Berlin, Beiband zum Jahrg. 38. viii, 332 pp. Berlin: Walter de Gruyter and Co., 1935.)

The Social System of the Zulus. EILEEN JENSEN KRIGE. (xix, 420 pp. \$9.50. New York: Longmans-Green Company, 1936.)

The first of these books deals with the changing life of the East African, and represents an initial attempt to obtain a comprehensive picture of East African culture in terms of the outer forces that impinge upon it. The data were gathered by Professor and Mrs Thurnwald in 1930-31, during an extensive field survey of the peoples who inhabit what may roughly be described as the region about Lakes Victoria and Nyassa. No claim to exhaustive treatment of any one of the numerous cultures visited is put forward by the authors, who felt they could obtain the best results if they sampled widely and supplemented their observations with excerpts from "district books"—apparently accounts of native customs set down by District Commissioners from time to time—and with replies to questionnaires submitted to officials, missionaries, and traders.

After an introductory chapter, in which the need for a "realistic" approach to the problem is emphasized, the political scene is considered. As might be expected from so seasoned an ethnological campaigner as Professor Thurnwald, the background out of which the results of contact developed, both native and European, is carefully set forth; that of the an-Iramba, indeed, being a document of the first order of significance, though the remaining tribes are treated with somewhat more brevity. The second chapter is complementary to the first, discussing the present administrative status in the light of earlier conditions that obtained, and assessing the results of the various methods of European rule that are found today.

This is followed by consideration of economic conditions, both aboriginal and present-day.

The discussion of the changes in family life and in the status of women is an abstract *in extenso* of Mr Thurnwald's volume *Die schwarze Frau im Wandel Afrikas* which is welcome in this English form. It treats of the essentially feminine aspects of the native cultures visited; the motivations behind marriage customs, the position of women in the native family, and the changes in their life effected by contemporary forces are discussed with great effectiveness. Chapters on changes in religion, in the techniques and forms of education, in the nature of Asiatic influences brought to bear, and, finally, in "the African mind" bring the book to a close. In the first two of these chapters, the effects of the mission and European school are discussed; the chapter on Asiatics in East Africa—which is all too short—considers an aspect of culture contact that no one of the growing number of those who have attempted to assess cultural change in this area have more than mentioned. The final chapter consists mainly of a series of documents—compositions of school-boys, songs, and the life-stories of three individuals—that illustrate the author's statements regarding the effect on these natives of contact with European patterns of life.

To an American anthropologist, however, despite the good points that derive from its breadth of attack and the vast field experience of its author, the book has grave shortcomings. Should a study, designated as scientific, be so ready with prescriptions? Can research be objective when the bias in favor of the "civilized" as against the "savage" is as patent as in this book? These questions can only be raised; certainly the biases of the author are made amply evident, and can be discounted. The first point, however, leaves one somewhat uneasy, as, for instance, where praise is lavished on the Jeanes Schools, which are dedicated to a type of education with which this country has had anything but a happy experience. One becomes increasingly restive, moreover, at the lengths to which Professor Thurnwald goes in his care to offend no one—in the elaborateness with which he apologizes in advance for the unpalatable assertion that somehow almost never eventuates (cf. pp. 116, 216). "Ethnocentricity" is, indeed, an unfortunate trait of human beings; it is doubly unfortunate when it invades the thinking of one who professes a science that is dedicated to the study of human tradition with as little of the ethnocentric point of view as may humanly be possible.

Professor Westermann, in his contribution, continues the series of excellent expositions of Southeastern Togoland culture based on the texts and other information of the Glidyí Ewe native named Bonifatius Foli. His memoir is dedicated to a discussion of the social life of this group, who because of their geographical position are somewhat more influenced by Dahomean culture than they are by Gold Coast custom. Utilizing both translated text and ethnographic statement for which no text materials are provided, early life and adolescence, modes of marriage, inner organization of the family, and types of funerals are first described. Following this is a discussion of social organization in the strict sense of the term, wherein is contained one of the few detailed statements of a system of relationship terminology

from any West African tribe. The book ends with a description of the organization of the native state, and something of the legal system under which these folk lived before European control.

The presentation of the data is made more complete by means of running references in the text to correspondences in custom to the cultures of nearby folk. These are such as to indicate that in this aboriginal meeting-ground of two different cultures we have a group which represents one of the few available examples of acculturation on the primitive level. Thus, for example, it is of great interest to find that the Glidyi Ewe have the Gold Coast system of day-names, but that their system of relationship terminology is that of Dahomey. The discussion of age-classes points suggestions for further research; they are not, as a general rule, present in this area of West Africa, and one wonders whether the groups so designated by Professor Westermann are not perhaps non-institutionalized groupings of boys and girls of about the same age who, as in Dahomey, tend to live by themselves in an informal group that loses its identity when circumcision or marriage mark the attainment of the status of manhood or womanhood.

The Social System of the Zulus represents an attempt on the part of its author to present the scattered information that exists on these folk, supplementing her materials with data obtained from Zulus with whom she herself worked. As might be expected, the outstanding contributions are in those aspects of Zulu culture concerning which the author either gathered the data, or directed its acquisition. The section on behavior patterns, found in the chapter which treats of the kinship system and social organization, is one such contribution, while the analysis of how important, in this extreme patrilineal culture, are the attitudes of a person toward his maternal relatives is similarly of real usefulness to students not only of African custom but, since this is a point too often overlooked when dealing with any unilinear culture, to those concerned with obtaining an understanding of the realities of social organization anywhere. Of a similar living quality and value are the discussions of the transition of boys and girls from childhood to adulthood, and of marriage.

The book is introduced by a chapter on the history of the Zulu people, which, with the aid of maps, traces their earlier migrations in perhaps too great detail for any but the specialist in Zulu life—and this to little purpose, since such a specialist can obtain his information from Bryant, on whom Mrs Krige so largely draws. The book continues in conventional pattern—social organization, the life cycle of the individual, inheritance and other aspects of the economic system, the political alignments that obtained and the famous Zulu military organization, religion, medicine, magic, aesthetics, and folklore. These latter sections tend to be sketchy; it is obvious where Mrs Krige's real interests lie. Yet as a whole the study is a good one, and should take its place beside the other works of high quality we are coming to expect from the anthropologists of South Africa.

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OCEANIA AND ASIA

We, *The Tikopia*. RAYMOND FIRTH. (xxv, 605 pp., 25 pls., 5 figs., 2 maps. \$6.00. New York: American Book Company, 1937.)

This book is the first of a series on the culture of Tikopia. It is to be followed by *Rank and Religion in Tikopia*, probably *The History and Traditions of Tikopia*, and possibly more. The series, by a colleague and former student of Malinowski, promises to resemble Malinowski's works on the Trobriands in bulk and largely in range of interest. This first volume has the familiar preface by Malinowski, with telling jibes at recent works by British and American anthropologists not of the congregation. But this preface is unusual in being only five pages long; as if to say that good wine needs only a little bush.

Rivers, in *The History of Melanesian Society*, made Tikopia the best known, at least by name, among islands of Polynesian culture lying within the geographical bounds of Melanesia. Dr Firth rarely refers to Rivers' work, explaining in the introduction (p. xxiv) that

Rivers himself was there for only a single day and nearly the whole of his account, as he himself stresses, was derived from John Maresere, a native of Uvea who had lived for twenty years on the island . . . Hence the account is inaccurate in a great many details of custom and language.

Firth spent a full year on Tikopia. His factual data must supersede those of Rivers.

Clearly this beautifully dressed volume is aimed, like so many British publications in anthropology, at general circulation. To this end the functionalist plan of centering a book about one aspect of culture has a great advantage. Scientifically, too, much of it is illuminating. Yet some points presented at length are too obvious to need more than passing mention among anthropologists, while a few seem quite irrelevant. On this point Malinowski appears to feel the need of a defense. He says in his preface (p. xi): "The stories, anecdotes and incidents so abundantly given by the author are not one line too long; they impart the true scientific character to the book." While it is rash to deny possible scientific import to any detail, occasional bits of autobiography and landscape painting, though they make good reading in themselves, are not shown to have a bearing on kinship in Tikopia. However, none of this lyricism pervades the discussion of kinship itself, so no harm is done to science.

By its Polynesian subject matter, Firth's work invites comparison with the publications of the Bernice P. Bishop Museum of Honolulu. The author charges (p. 579, footnote) that many of these publications "give only the skeleton of the kinship structure of the community studied." Here he is on his own ground. Most regions of Polynesia have been poorly described in this respect, and no account yet published comes up to this work on Tikopia. But for one thing, few Polynesian islands have kept enough of the native culture to yield a picture like this. On the other hand, there is every probability that Firth's account, even when completed, will be weak where Bishop Museum publications are strong: notably in material

culture. In short, the comparison comes down to a special case of the issue between ethnology and (in the functionalist sense) social anthropology.

Yet at several points Firth betrays a rapprochement between the two officially hostile points of view. He gives much more comparative material than has been usual among functionalists. He occasionally ventures such sentences as these: "The distribution of this word is an indication of the Polynesian-Melanesian relationship in this area of the Pacific" (p. 76, footnote), and "This phenomenon appears to be characteristic of western Polynesia only, and its presence suggests possible Melanesian contacts" (p. 586). These are only side-lights. But one of the fundamental points, a theory of incest prohibitions, shows the same tendency. Two brief quotations cannot present it fairly, but can at least suggest its nature:

The widespread occurrence of the incest prohibition simply goes to show that the constitution of the individual family, and the personal needs of its members for a wider support and co-operation than the family itself affords, render it difficult as a rule for close consanguinity and affinity to coincide (p. 343) . . . The "horror of incest" then falls into place as one of those supernatural sanctions, the aura of which gives weight to so many useful social attitudes (p. 340).

In so far as unreasoned abhorrence, rather than consideration of the effects on the kinship complex, is admitted as the motive behind avoidance of incest in everyday life, this explanation of incest bans in terms of effects on the kinship complex becomes a theory of origin rather than of function. This comment, which the reviewer owes to Dr Ernest Beaglehole, by no means denies the inductive basis nor the possible validity of the theory. It merely points out that it is a theory of a kind which functionalists are not supposed to permit themselves.

Consideration of historical process also prompts the suggestion (pp. 370-71) of a new term for kinship groups of a kind prevalent in Polynesia and in some other areas: groups organized on the basis of "unilateral recognition of common descent," but not dependent on exogamy or totemism. After criticizing some other terms, as "joint family" and "lineage," Firth says:

As a rule by historical tradition, and presumably in actual social process, they have arisen through the branching and re-branching of the family structure, acquiring greater autonomy and independence the further they move away from the parent stem . . . One term which might be employed to characterize such kinship groups is "ramage" . . . This term has the advantage of suggesting immediately by its etymology the branching process by which these groups attain individuality and yet keep their connection with the parent stem.

Without questioning the soundness of this historical analysis, it may be pointed out that functionally this term is only appropriate where the "ramage" acts as a division of a larger unit. In some cultures—within Polynesia, Futuna and Uvea at least—the "ramage" or "lineage" is the largest functioning kinship unit. The parent stem may be traceable through genealogies, which confer prestige but function overtly only when a chiefly title falls vacant and closely related candidates for the succession are lacking. Relationship between two "ramages" is overtly recog-

nized only in the rare and specialized exercise of the *vasu* privilege. Otherwise the larger functioning units are territorial-political, organized without regard to kinship except for succession to the title of their chiefs. In such cases "lineage," despite its admitted disadvantages, remains the more appropriate term for the kinship group.

We have here, then, a functionalist with a weakness for history; or will it prove his strength? It is to be hoped that the volume on *History and Traditions* will develop the author's position in this respect.

What fault-finding there is here touches only the edges of Firth's work. It refers to what seems, from another point of view, a regrettable omission in scope; to occasional ornament, unnecessary from a grimly scientific point of view; and to incursions into what is professedly enemy territory. None of this attacks the fundamental worth of the book. *We, the Tikopia* is a report of conscientious field work, one of the richest accounts of kinship anywhere and decidedly the richest for Polynesia. It is an important addition to knowledge of Polynesian culture. Its successors promise a similar service.

E. G. BURROWS

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Art and Life in New Guinea. RAYMOND FIRTH. (126 pp., illus. \$3.50. New York: Studio Publications, Inc., 1936.)

This book is designed primarily for artists and art lovers, and the main function of the text is to supplement the truly splendid illustrations. There are 97 plates, most of which show objects now in various British museums. The subjects have been chosen for their aesthetic value, and the photographic work and reproduction are both unusually fine, making many of the illustrations works of art in themselves.

In spite of its brevity the text covers a wide field. There are discussions of physical and cultural types in New Guinea, of the relation of art to various phases of native life and a delimitation and characterization of the various art areas on the island. There is even an unusually clear and sane discussion of the primitive artist, his aims and limitations.

In nearly all "savage" art the artist is essentially and foremost a craftsman. The things that he makes are primarily objects of utility . . . That they receive aesthetic elaboration is due to two factors, the interest of the man in his work, and the dictates of a tradition which lays down rules for the form which the objects must take . . . In such communities the cry of "art for art's sake," though it might be tolerated would be meaningless. The superior craftsman receives his meed of admiration and reward, but there is no self-conscious separation of himself and his products from the utilitarian sphere of life, no divorce of the artist from his public . . . There is on the other hand no vaguely communistic sense of working for the public good. There is a sense of responsibility to others, and incentive to do good work, but these are motivated by personal pride, social rivalry and a desire for economic gain.

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Iban or Sea Dayak Fabrics and Their Patterns: A Descriptive Catalogue of the Iban Fabrics in the Museum of Archaeology and Ethnology, Cambridge. ALFRED C. HADDON AND LAURA E. START. (xv, 157 pages, 36 plates, 32 figures. \$8.75. Cambridge: University Press; New York: Macmillan Company, 1936.)

The authors base this preliminary study of Iban fabrics chiefly upon the collection purchased from Dr Charles Hose and the one owned by the British Museum. Added to those sources were data furnished by many photographs and sketches made by Professor Haddon in 1898 at the Sarawak Museum at Kuching. The technical portions of the study are the work of Miss Start, who is known through her careful analyses of Coptic and Burmese textiles in the series of *Notes* published by the Bankfield Museum of Halifax.

The book is much more than the descriptive catalogue it announces itself to be. The text is full of native terms and phrases as they apply to methods and designs. Wherever the authors have incorporated native terms in quantity they have rendered some of the passages considerably more difficult to follow, although in so doing they have broadened the scope and value of their work. A complete vocabulary of Iban words appearing in the text and their English equivalents occupies eight pages.

The general subject matter falls into several major divisions: men's and women's complete costumes; the production of cloth and the making of patterns; dyeing methods; the separate costume parts; an analysis of the patterns; Iban religion and its expression in decorative art. The most detailed of these divisions is the one within which are described the six principal costume parts. After a general statement covering type, use, and characteristic decoration, Miss Start gives a technical analysis of each of the specimens in the Cambridge Museum. Her terminology is clear-cut, her own detail drawings simple, and her explanations both within the text figures and in the legends are unusually concise.

Professor Haddon's section headed *An Analysis of the Patterns* is a partial realization of an "ambitious scheme" through which he has hoped to present a study of the decorative art of the natives of Sarawak. The 84 specimens in the Cambridge Museum bear names of more than 1500 patterns and designs. These are "traditional and hereditary, being handed down from mother to daughter" (p. 123). Among the illustrated motives are zoomorphs, tree and flower forms, objects in use in daily life, and rare examples of natural phenomena such as clouds, rivers, and the moon, "almost all intimately connected with their environment, pursuits, or beliefs" (p. 137).

Most of the Iban designs, says Professor Haddon, are so highly conventionalized that "the nearest approach to certainty [of their meaning] can be attained only by inquiry of the actual woman who made the pattern." Is the design as it appears, then, really a conventional or formalized one, or is it an individual weaver's interpretation of a conventional design?

Under *General Considerations*, the authors bring together a variety of published opinions as to the origin and antiquity of ikat or resist dyeing and of characteristic

Iban designs. Taking a number of points into consideration, Professor Haddon considers it "reasonable to suggest that the traditional designs used by the Iban women in their textiles have been developed since the Iban became separated from other peoples, and thus the design would be a native art peculiar to the Iban" (p. 141).

The textile patterns of Indonesian peoples are often cited as examples of unusual manipulative skill. With the quantity of descriptive detail presented in this study—Miss Start's plates often have a dozen to twenty unit drawings on them—it is possible to more fully appreciate the necessary practice and experience if the Iban woman would effectively render the traditional designs.

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Interracial Marriage in Hawaii. A Study of the Mutually Conditioned Processes of Acculturation and Amalgamation. ROMANZO ADAMS. (xvii, 353 pp. \$4.00. New York: Macmillan Company, 1937.)

In this volume Professor Adams has brought together the results of recent studies made at the University of Hawaii of intermarriage among the various racial groups on the Hawaiian Islands. The author's interest is primarily upon race relations, and his emphasis therefore is upon the sociological rather than the biological aspects of this problem.

Hawaii is an unusually satisfactory laboratory for the study of interracial marriage since unions of this kind have been formed for more than a hundred years and have come to be accepted not merely as inevitable but as a well established part of the social system. The study has been facilitated by the fact that marriage and birth data are recorded by the authorities in sufficient detail to throw a great deal of light on the extent and character of the process of amalgamation. The racial classification adopted by the author is that followed by the Bureau of the Census which distinguishes nine racial and two mixed racial groups. While such a classification accords with popular usage on the islands, it is based upon national origins rather than upon racial types. A reclassification, if possible, of at least some of the statistical data into more generally recognized racial groups would have been helpful to students of race problems.

The pattern of racial intermarriage in Hawaii was established first through the union of British and American men and Hawaiian women, and later through Chinese-Hawaiian marriages. Their offspring and descendants, the Caucasian-Hawaiian and Asiatic-Hawaiian, are today the most important groups of mixed bloods and are rapidly replacing the Hawaiian people. In 1910 there were 208 Hawaiians to 100 part-Hawaiians while in 1930 the Hawaiian proportion had decreased to 80. The majority of children born in recent years to Hawaiian women are part-Hawaiian.

The process of amalgamation in Hawaii is by no means proceeding at an equal rate among all the racial groups. The author constructed an index of in-marriage

preference based on 1930-1934 marriage data which indicated that the Japanese were the least favorably inclined toward racial intermarriage. The ranking of the racial groups, according to this index, is as follows: Japanese, 955; Filipino, 954; Korean, 799; Chinese, 780; Other Caucasian, 774; Porto Rican, 718; Portuguese, 634; Part-Hawaiian, 545; Hawaiian, 517; and Spanish, 287. For explanation of these wide variations the author states that consideration must be given to differences in sex ratios, group loyalties, and nationalistic sentiments rather than to differences in physical race traits.

The rates of out-marriages have been in general rising among the pure racial groups during the past twenty years, while the rates among the part-Hawaiians tend to remain constant at a fairly high level. It is interesting to note that the Japanese out-marriage rate, the lowest of all the racial groups, is showing a marked increase as the Hawaiian-born Japanese reach marriageable age.

This speeding up of the process of amalgamation is a natural outgrowth of the doctrine of racial equality which has long prevailed in Hawaii. It is a mistake, however, to assume that racial intermarriages are always accepted without criticism. Law and public opinion permit such marriages, but there still exists much adverse family sentiment as well as considerable opposition on the part of certain social sets and racial groups. Students of race relations interested in the much talked of intermingling of races in Hawaii will find in Professor Adams' volume a wealth of information upon this subject and a well balanced discussion of the many problems involved.

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Marriage Conditions in a Palestinian Village, II. HILMA GRANDQUIST. (Societas Scientiarum Fennica. Commentationes Humanarum Litterarum, Vol. 6, No. 8. 366 pp., 30 illus. 160 Fmk. Helsingfors: Akademische Buchhandlung, 1935.)

This is the second and concluding volume of Dr Grandquist's study, the first having appeared in 1931. In the earlier volume she dealt with the Age of Marriage, the Choice of a Bride, and with Marriage by Consideration. The two volumes are continuous, both dealing with the Arabic village of Artas, near Bethlehem.

The first half of the present volume deals in great detail with the betrothal and marriage ceremonies. The final half takes up in successive chapters the Woman in Her Husband's House; Polygyny; the Hardane Problem (the relation of the offended wife to her parental home); Divorce; and Widower and Widow. The text weaves together with great skill the astute observations of the author, brief case studies, and a mass of folk-talk, proverbs, and the formalized expressions appropriate to the various specific occasions. The treatment is somewhat fuller on the woman's side, where the author's contacts were closer, than on the men's.

Dr Grandquist's study has the great merit of confining itself to the specific aspects of her subject in a single small village. This narrowness of focus, which sharpens the detail and brings out the minor values in the personal behavior of

specific members of the village, is given perspective throughout by her footnotes which cover with great adequacy the literature in English, French, and German bearing on familial institutions in the Arabic and Muhammedan world. The advantage she gains by treating a specific village community appears in her discussion of divorce. The formal freedom of the Muhammedan husband is well known. This has led to the glossing over of the economic, social, and psychological factors that serve as brakes on this nominal freedom of the husband to divorce his wife at any time and for the airiest causes by merely expressing the customary legal formula. Actually, Dr Grandquist finds that, of the 199 married men in Artas, only 10 have divorced one or more wives. (Likewise, her house by house count showed only 26 men, or 13 percent, as polygynous.) She is careful to point out that Artas is a poor community, with a scarcity of women if one disregards available women in surrounding communities, and she specifically invites checks based on different types of communities. But, she adds, "I think that this would result in important corrections and reductions of our ideas of the powerful husband of the East, who only requires to express the formula of divorce to be rid of his wife, and who does it so frequently." Incidentally, her discussion of various concrete divorce situations is highly informative: cases in which an irate husband inadvertently utters the fatal words, to his own instant consternation; the slight or simple divorce in which a "dangerous corner" in marital life is turned; the highly useful conditional divorce in which the husband throws the full weight of his marriage into the scales to weight a maritally irrelevant matter in his favor; and, finally, the decisive divorce.

The picture presented is of husband and wife as "fundamental strangers," the husband bound the more closely to his parental home by his marriage and tied to his wife by his economic investment in her, the wife living on uneasy terms in the home of an "alien" family, her paternal home remaining her first and greatest source of security, and her brother her closest legal and affectional tie of her own generation. ("It is said that the husband is only a garment which a woman puts on or throws off again, or she herself can be 'thrown off' by her husband, but her brother is the one who is always there.") The woman's life involves greater complexity and exposure than does the man's. "A wise woman will never admit that she is more comfortably situated in her husband's home." She can always become *hardane* if the pressure becomes too great and abandon her children in her husband's home and return to her own parental home, but this state involves its own peculiar handicaps; and in the case of a "stranger wife" who has married into another village or a "cut-off wife" whose father and brothers are dead, her freedom to become *hardane* may decline to the vanishing point. Dr Grandquist is careful to point out that by "wife," especially if she is possessed of a "strong personality," the wife may manage to overcome some of her difficulties. She illustrates concretely the exercise of such wiles in situations involving co-wives.

Students of familial behavior will welcome this closely focussed monographic study. Even its wealth of specific detail, however, leaves the reader who is alive to problems involved in the interaction of culture and personality itching with

unanswered questions: What does it *feel* like to be a woman in Artas? What is going on in the bride's head as she hides from the village dancing and festivities over her own betrothal in her parents' house of sorrow? What is the complex of emotionality behind her gilded impassive face as she waits with closed eyes at her wedding for her first glimpse of the man she is marrying? Where, if at all, does mutuality break through in these marriages of "fundamental strangers"? In how many cases does genuinely recognizable satisfaction with one's marriage emerge, and how often is it present with the man and how often with the woman? If "the love between sister and brother, according to the conception of the *fellahin*, . . . is more beautiful than the love between wife and husband," in how many cases does marriage actually reverse this assumed condition? Does the woman's positive emotional affect tend to be heavily displaced to her children, and in how many cases does her brother take precedence over her emotional ties to her children? What are the overtones of small talk, if any, between spouses, say, in the early evening hours that precede coitus, and what are the thoughts and sentiments that accompany each spouse in such a culture in the midst of the recurrent experiences of intercourse? What precisely is the range of human tolerance as regards acts of personal intimacy in an atmosphere of institutionally enforced aloofness? And what does a lifetime of such living, if it occurs, do to the personalities involved and to the pattern of the culture which they carry about within their skins? If such questions seem to press for answers in terms of the differentness of a given culture from our own, they represent nevertheless urgent questions for which students of our own culture look for answers to the ethnologist dealing with other cultures.

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The Wilderness of Zin. LEONARD WOOLEY AND T. E. LAWRENCE. (166 pp., pls., maps. 18s. London: Johnathan Cape, 1936)

This is a reprinted edition of a report by the late Colonel Lawrence of a superficial archaeological survey of Sinai during a period of six weeks prior to 1914. The writers consider that Stone Age man did not inhabit the district. Lord Kitchener queried the prehistoric inhabitation of the Zin desert some years ago. The authors state that flints found previously were natural fractures and not artifacts (p. 38). Reference is made to Quarterly Report, Palestine Exploration Fund, 1879, p. 62. The earliest period for the flints found by the authors is given as Byzantine. Oval scrapers were seen by them in use by the Arabs for shearing sheep and flints are used by nomads to cut their hair and nails. These flints were discarded (p. 38). It is not unreasonable to assume that these practices are survivals of a very ancient custom and that the scepticism of French archaeologists regarding the antiquity of artifacts found on the surface is justified by this evidence. As brown patina develops on flints exposed to the weather in a few years (p. 38) and the effects of desert rain-storms are of a curious character, the authors are conservative in their conclusions. Possibly the statement (p. 36) "that at no time since man first settled in this land

has the rainfall been appreciably greater or more regular than now" is open to question.

The report on Esbeita (which was not abandoned until the 12th-14th century) is of considerable importance. This town was not encircled by a wall. It was similar to many ruined and mud built towns in Africa. The houses were enclosed in compounds (hoshes) by walls and the streets were few. Water was conserved in cisterns. Few wells were found. During war time the ends of the streets leading out of the town were closed by temporary walls. The city became then what was virtually a walled fortress town.

Figure 16 illustrates a headpost similar in outline to the minarets and domes of Persian and Indian mosques. These headposts for gates or graves have been described frequently by other writers as phallic. They are found in Christian and Moslem buildings of a religious character and the design is recognizable in the interior outlines of the well known Moorish arch. These headposts are not evidence of any phallic culture and those at Abda and Esbeita are dated at about the 6th century A.D. when the cities were presumably inhabited by Christians. The design of these headposts seems to be merely a form of the classical ball and pillar type. This was at a later period transformed by Moslems into a column surmounted by a turban.

At Abda there are ruins of a great pillared temple dated 2nd-3rd century B.C. which is surrounded by Nabatean tombs. Inscriptions to Aretas were found at Khalassa (Elusa of the Romans), which was founded in the 2nd century B.C. There are numerous illustrations of tombs, graves (rectangular and circular). Some of these resemble those in North Africa and the Sahara. It is interesting to note that the modern graves are placed near water for the purpose of the angel's inquisition.

ARTHUR E. ROBINSON

ST. ALBANS, HERTS., ENGLAND

Ideologiya sibirskogo shamanstva [*The Ideology of Siberian Shamanism*]. D. ZELENIN. (Bulletin, Academie des Sciences de l' U. R. S. S. Classe des Sciences Sociale, 1935, pp. 709-48.)

Reconstructing the earliest stage of shamanism on the basis of survivals, Professor Zelenin comes to the conclusion that the rise of shamanism is closely connected with its medical function, which exists even to the present, though it long ago ceased to be its sole function. Thus, initially, shamans were medicine men.

According to the primitive notions of the Siberian shamans, the cause of an illness lies in the fact that spirits possess the body of an ailing man, "gnaw his bowels," "suck his blood," etc. In spite of W. Bogoras' opinion, the notion of an evil spirit ravishing one of the souls of the ailing man as cause of his illness is one of the most ancient superstitions and is connected with the rise of hierarchical relations among the demons. Senior (chief) demons direct the junior ones; they send them after the soul of the patient. The idea of an evil spirit possessing the body of a patient is a direct continuation of the most ancient notion, that of a living animal, a totem,

possessing the body of an ailing man. For this reason many diseases of Siberian peoples are called after the names of animals. These zoological names of diseases were in accordance with the primitive conception of their nature, which regarded them as caused by animals possessing the body of a sick man: thus animals were regarded as bearers of diseases. The Buryat believed that during the rite performed by a shaman curing an abscess on the foot of a patient, a frog sprang out of the sore spot, whereupon the patient was cured. According to the notions of the Gilyak the cause of a fatal illness is a toad which steals upon a sleeping man, creeps into his mouth and penetrates his body.

The most ancient means of curing illness consisted in licking the sore spot with the tongue: this was an instinctive imitation of animals licking their wounds. Curing illness by means of sucking was also practised by Siberian shamans. As a proof that a disease was cured, the shaman showed to the patient and observers this or that animal (a worm, a lizard, sometimes a bowel or a hair). But when the cause of an illness was attributed to an evil spirit, it was believed that the shaman, sucking the sore spot, sucked up the spirit of the illness. This procedure led to the notion of a compulsory transference of an imaginary spirit of the illness from the patient's body into that of the medicine man. Besides, only such persons could become medicine men to whom the sucking up of a demon would not be fatal. Hence, neurotic and psychotic persons, who already were possessed by spirits and therefore were immune, were taken for the purpose of sucking out spirit illnesses. Zelenin regards this as the inception of the first phase of shamanism. After all abnormal individuals had been drawn into the medical practice they consciously pretended possession and thus became shamans.

During the epoch of the matriarchate women assumed the medical functions of shamans, a practice which continued thereafter as a survival. "Woman is a shaman by nature," the Chukchee told Bogoras. This transfer from men to women is attributed to the identification of a man possessed of a demon with a pregnant woman and to the prevalence of female sufferers from mental diseases.

In the course of time the shaman's spiritual power acquired economic force: the idea of chief and secondary spirits permitted the influencing of the lesser spirits by the greater, the shamans having the latter as spirit-protectors. At the same time (the epoch of the decline of the clan) ancestor-worship was developing, and the theory of disease changed. The shaman's function then was not to suck, but to exchange the soul of the patient for that of a sacrificed animal.

Although he confines his study to the peoples of Northern and Middle Asia, omitting relations with North America, the author has contributed a new conception and interpretation of the ideology of Siberian shamanism—a problem scarcely touched in special literature.¹ This work deserves to be noted by the historians of religion.

EUGEN KAGAROW

LENINGRAD

¹ In modern works only in the article by K. Meuli, *Scythia* (Hermes, Vol. 70, No. 2, p. 145, 1935) wherein he regards "egocentrism" as a basis of the psychology and ideology of shamanism.

PHYSICAL ANTHROPOLOGY

Man's Place among the Anthropoids. WILLIAM KING GREGORY. (vi, 119 pp., 11 figs. Oxford: Oxford University Press, 1934.)

Three lectures on the evolution of man from the lower vertebrates, delivered at University College, London, dealing successively with the locomotor skeleton, the jaws and teeth, and the face and braincase. Under each heading is first given an account of the major structural stages from fish to the generalized mammalian condition. Following this in each case is a more detailed consideration of evolution within the primate series. Here considerable attention is given to a refutation of attacks by Wood Jones on Gregory and other upholders of the "orthodox" views of human evolution and counter-criticism of the former's arguments for the tarsoid origin of man. The author concludes that it is

misapplication of the sound principle of "Irreversibility of Evolution" which has led the brilliant anatomist Professor Wood Jones to . . . prize man's few anatomical agreements with *Tarsius* as of far greater phylogenetic significance than the host of his positive agreements with one or another of the anthropoid apes.

A. S. ROMER

HARVARD UNIVERSITY

Skeletal Remains of the Central Eskimos. K. FISCHER-MØLLER. (Report of the Fifth Thule Expedition 1921-24, Vol. 3, No. 1. 96 pp., 3 figs., 33 tables, 52 charts, 8 pls. Kr. 5.00. Copenhagen: Gyldendalske Boghandel, Nordisk Forlag, 1937.)

Any report describing prehistoric skeletal material for which the accompanying culture already has been recorded marks a decided advance in basic anthropological knowledge. Heretofore the Far North could boast of but one such record, that for the longheaded Point Barrow "Igloo" people in northern Alaska. The present study concerns a second culturally identified (Thule) site, namely, Naujan in the Central Eskimo regions. This report, long delayed, has been eagerly awaited by those engaged in unfolding Eskimo prehistory, for reasons that follow.

Much confusion has arisen as regards the age of the Point Barrow remains, owing to a misinterpretation of the culture.¹ Originally the prehistoric Barrow people were thought to be Thule, but it has been shown since that they were much earlier. Unfortunately, Fischer-Møller does not seem to know of this re-interpretation, for he states:

The culture found in the houses at Barrow was on the whole identical with the Thule Culture—but with some variants which seemed to take it somewhat farther back than the Thule Culture at Naujan as described by Therkel Mathiassen; for instance an intrusion was found from the old Bering Sea Culture.

The new interpretation as regards the cultural position of the prehistoric Barrow people made it very desirable to have a description of the physical remains from a true Thule site. The present report is the first to fill this need.

¹ See Henry B. Collins, Jr., *Eskimo Archaeology and Somatology* (American Anthropologist, Vol. 36, pp. 309-13 1934).

The essential conclusion of this study, as already anticipated by Collins, is that the crania from Naujan are identical, not with the old Point Barrow remains, but with those from modern Point Barrow graves and with those reported by Hrdlička in 1910 from Southampton Island. In other words, they are dolichocranic, but not hyperdolichocranic like the old Barrow crania. The author, as is natural, due to this misinformation regarding the culture of the Point Barrow "Igloos," is completely puzzled by the lack of identity in physical type between the Naujan and old Barrow peoples.

As to the anthropometric details, there is little to criticize; indeed, the author is to be praised for giving full individual measurements, both for the crania and long-bones. The latter are too few to serve as the basis for trustworthy conclusions, as the author realizes. Nevertheless, he is fairly confident of his estimate of stature for the Naujan Eskimo: the males 161-162 cm., the females 150-151 cm.

In addition to the Naujan material a few crania are described from the following localities: Baffin Island, Eskimo Point, and Kuk on Southampton Island. Some of these crania also are Thule, but others are more recent.

It is to be regretted that the author has not given at least a summary of the nonmetrical observations. However, this omission is partly understandable because of the lack of similar data for the comparative material and also because of the present unsatisfactory status of this type of observation. Numerous craniograms and photographs compensate somewhat for the lack of observational detail. The photographs, unfortunately, appear to be in different scales.

T. D. STEWART

UNITED STATES NATIONAL MUSEUM

Studien an Skeletten aus dem Inneren Vitilecus. ELIZABETH WEBER. (Studien zur Rassenkunde, Vol. 1. 94 pp., 2 pls., 5 figs. Leipzig: Verlag der Werkgemeinschaft, 1934.)

Geographically, Fiji is situated between Polynesia and Melanesia, and culturally it reflects its eastern and western neighbors. For these reasons the classificatory fate of Fiji hung in the balance during part of the last century. One traveler impressed by the Fijian counterparts of Polynesian custom would include it among the islands of the Polynesian world, another struck by its Melanesian traits and dark-skinned people would bespeak it for Melanesia. Although now this archipelago is ordinarily listed as a Melanesian group, its blended culture still offers a fruitful field of inquiry and analysis.

Of the physical character of the Fijians little has been actually recorded of anthropometric value until quite recently. In 1933 Dr W. W. Howells published data collected by Dr William L. Moss on Viti Levu, in the western group of the archipelago. The subjects, although obtained at a mission school, originated from the interior. Dr Howells concluded from his study of these subjects that they represented a mixture of Polynesian and Melanesian characters.

The publication under review considers the population of the inner mountainous

reaches of the island of Viti Levu, specifically from Matavalu, Bukatine, Namoli, and Naloka. Miss Weber has, unfortunately, but a small series with which to work: 13 male and 5 female crania. For some of her measurements the series are even further reduced. On purely statistical grounds serious doubt might be entertained on the validity of comparisons resting on such limited numbers, yet the intrinsically great interest of her problem perhaps justifies her presentation of the series. Apparently, the type is homogeneous and easily recognizable. From comparisons with Melanesian and Polynesian data, Miss Weber concludes that the interior mountain-dwelling people of Viti were a distinct type produced, perhaps, by isolation but betraying affinities with eastern Melanesia and to a lesser extent with Sarasin's series of New Caledonians.

Although both Miss Weber's and Dr Howells' material are from the interior, their conclusions are at variance with each other. Miss Weber's crania were collected in 1876; the subjects measured by Dr Moss were living in 1929. The difference, therefore, between the two populations may represent a replacement of one by the other; it may be taken to disclose the contemporaneous existence of two strains in the interior; or, it may be interpreted to mean that Miss Weber's crania belong to an earlier type preserved and encysted among a divergent population.

H. L. SHAPIRO

AMERICAN MUSEUM OF NATURAL HISTORY

Anthropometry of the Natives of Arnhem Land and the Australian Race Problem. W. W. HOWELLS. (Papers, Peabody Museum of American Archaeology and Ethnology, Vol. 16, No. 1, 1937. 97 pp., 2 maps, 38 tables.)

In this excellent monograph Howells has contributed much to the elucidation of the problem of the status of the Australian physical type. The initial basis for the study consists of the measurement and observation of 240 adult males and 69 adult females undertaken by W. Lloyd Warner in 1927. The general locale is northeastern Arnhem Land but the material (males only) is subdivided geographically as follows: Northwest Arnhem Land, 99; Victoria River, 28; Melville and Bathurst Islands, 28; Northeast Arnhem Land, 77; Roper River, 8. Howells first considers these groups and concludes that they fall into two major categories; a northern, comprising Melville, Northeast, and Roper; a southern, including Northwest and Victoria. The latter group is set off by possessing taller stature, shorter and broader head, and higher and narrower face and nose. The author then compares the present material with Australian natives in general, pointing out that head length and breadth are least in North Australia, head height lowest in South Australia, face length and nose breadth greater in southern Australia, especially New South Wales, and stature greater in central North Australia. He agrees with Hrdlička and Morant on the essential homogeneity of the Australian aboriginal population. In a few physical characters, however, regional differences are observable. In general the population of North Australia is farthest removed from the general type. Howell attributes this not so much to a possible foreign strain as

to a long-standing isolation with subsequent emergence of a fairly distinct type.

The section on the comparison of the Australian with other racial groups assumes that "the Australian is a survivor from an actual level in the morphological development of man and [is] not merely a peripheral subracial type." The physical characteristics of such a developmental stage are: very black skin; wavy or curly hair (which, according to Howells, *antedates* negroid woolly hair); long narrow keeled skull; heavy supraorbital ridges; alveolar prognathism and a large palate; receding chin; short thick nose; and low broad face. This type Howells carefully traces in Melanesia, Indonesia, and Asia. He concludes that the Australian type is present on the coast of New Guinea, northeast New Britain, and New Caledonia. It is not found in Negrito, Indonesian, or Malaysian groups, though a close approach to the type is to be found in Timor and Flores. In Asia an Australian type is found in India, and especially among the Veddas of Ceylon. In his discussion of the Dravidians Howells follows Hooton in deriving the (pre-) Dravidians from an Australoid (or partly Negroid) plus a White (Mediterranean) type. The Tasmanians are concluded to be basically Australian with a tincture of woolly-haired Melaneseans. The ultimate home of the Australian type is traced to southern Asia, reaching Australia and Tasmania via Timor and (or) New Guinea. After this extensive comparative analysis, Howell affirms his earlier view of the Australian type when he concludes: "The Australian is not a blend but a major race, and is the most archaic race still surviving." In his treatment of the present distribution of the Australian type (complex is really more apt) the author adapts in principle, if not in fact, the "zone and strata" theory of Taylor.

It is perhaps not too much to hope that Howells will next undertake the problem of the "Australoid" in greater detail, as for example those reported by Guha from Mohenjo Daro (though Guha later dropped the term), Elliot Smith and Zuckerman from Aditanallur, Drennan from Cape Flats, and Sullivan and Hellman from Ecuador. There is an excellent bibliography, although Buxton's (1935) paper on the Negro-Australian problem generally, and Menghin's (1931) discussion of the relation of the Australian to early human types might well be added.

This review cannot in a few lines do full justice to an admirable paper. It really must be read in full to be appreciated, not once but several times—and by the cultural anthropologist as well as the physical anthropologist. Howells does not overburden his discussion with technical terms, and as far as is justifiable equates physical type with possible cultural trend or spread.

W. M. KROGMAN

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The Racial Characteristics of Syrians and Armenians, based upon data collected by W. B. Cline, C. S. Coon, J. M. Andrews, and W. C. Dupertuis. CARL C. SELTZER. (Papers, Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. 13, No. 3, 1936.)

Until recently the descriptive anthropological material relating to southwest Asia was less extensive than available for almost all other regions of a compar-

able size anywhere in the world. However, two notable contributions have recently appeared: Mr Henry Field's memoir on Arabs of Iraq, issued by the Field Museum of Natural History in 1935, and the paper reviewed.

The latter presents data relating to two series of men. The first of these comprises 263 Syrians, of whom the majority were students in the American University of Beirut, and the second 101 Armenians examined in Boston and all alleged to have been adult on their arrival in the United States. As all the observers were trained at Harvard, it is claimed that their records are truly comparable. This paper was apparently completed before Mr Field's data on Arabs of Iraq were available, and no reference is made to Professor Bunak's *Crania Armenica* published as a supplement to the Russian Anthropological Journal in 1927.

The measurements are treated singly and the usual statistical constants are given for each. The Syrian sample is divided into four sub-samples on a geographical basis, two being for coastal (Lebanon 164 men and Alawiya 53) and two for inland (Damascus 19 and Homs-Hama-Aleppo 17) districts. A comparison for 27 characters shows that the first two are so similar in type that they may almost be considered to represent the same population. The only clear distinction between them is for the horizontal circumference of the head, but this is a difficult measurement to take and it is liable to be affected appreciably by the length of the hair. Evidence of greater differentiation is found when the two inland sub-samples are compared with one another and with those representing the coastal regions, but the stress laid on this point is scarcely justified. The two former series are really too small to yield reliable conclusions, and it is not at all unusual to find that small series suggest distinctions which are not confirmed by more adequate material. Apart from the statistical estimates of their reliability, there is more danger of small series failing to give fair random representations of the populations they describe. It is certainly dangerous to use samples made up by fewer than 20 individuals for the purpose of judging the relative variabilities of different populations, as is done here. The safest conclusion is that there is no clear evidence to show that inland Syrians are of a different type from those native to the coastal districts.

Clear distinction is made between the Syrian type, deduced from the total sample, and the Armenian, the latter having the greater head and facial breadths and the longer face. Since stature and the cephalic index are frequently found to distinguish allied types more clearly than do other characters, it is curious that they fail to do so in the present case. The average statures of both types are unexceptional, but the cephalic indices (Syrians 85.1, Armenians 85.8) are nearly as high as any known. Data for a number of non-metrical characters are presented, but no exact statistical comparisons are made between the percentage frequencies for different groups. Too much stress appears to be laid, again, on the significance of the differences between the Syrian sub-samples. On the average the Armenians have rather lighter and more wavy hair and rather lighter eyes than the Syrians, while some other qualitative characters distinguish the two types. It is difficult, however, to ensure that the records of such characters made by different observers are truly comparable.

Certain of the qualitative features, and particularly eye color, suggested that the Armenian sample is particularly heterogeneous, though the variabilities of the measurements do not appear to support this view. Accordingly, the correlations between certain characters—particularly eye color with others—were examined. The statistical treatment of this topic is not convincing, and the ways in which “ultra-Armenoids” are selected from the Armenian series, and “Armenoids” from the Syrian, appear to be quite arbitrary. The reviewer adheres to the view, which is unfashionable at present, that attempts to partition a miscegenated group are unlikely to be profitable. Dr Seltzer’s paper is obviously a record of permanent value, whether this view be correct or not.

G. M. MORANT

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GENERAL

Anthropology. An Introduction of Primitive Culture. ALEXANDER GOLDENWEISER. (xxii, 550 pp., 30 pls., 80 figs. \$3.75. New York: F. S. Crofts & Co., 1937.)

If anthropology is a scientific holding-company for ethnology, archaeology, somatology, and linguistics, then the title of Dr Goldenweiser’s book is somewhat ambitious. Four ethnologic topics are systematically elucidated in the central section of the book (pp. 59–426).

This main section is followed by marginal essays concerning the plight of present day primitive groups (pp. 427–39; early man is conspicuously not considered in this volume); environment (pp. 443–54); various problems associated with diffusion (pp. 455–95) and evolution (pp. 496–526), and by a bibliography (pp. 529–37). Marginal essays also precede and are even occasionally interspersed in the central section of the book. These are concerned with the physical (pp. 3–12) and psychological (pp. 37–46) criteria of man, various racial problems (pp. 13–36); some startling as well as conventional recommendations for field-technique (pp. 47–56); and, e.g., invention (pp. 118–34).

The essays in question cover a sufficiently wide range to have encouraged their author to call his work *Anthropology*. They are necessarily slight in a volume attempting to preserve a sense of proportion which this volume does. They are necessarily trite, for the general viewpoints in anthropology were not formulated yesterday.¹ Do they advance the subjects treated? That is perhaps not their intention. Do they give beginning students a stimulating insight into the subjects treated;

¹ Dr Goldenweiser refrains from discussing problems in anthropology only now in process of formulation, as e.g., personality reactions to culture and psychological types of culture. Space is particularly devoted to reporting general viewpoints which anthropologists have developed in connection with ethnographic reports; no regret is expressed for important subjects neglected, no guess is made as to future findings. Compare, in this respect, Dr Linton’s recent text-book, *The Study of Man*; the two books are of course not otherwise comparable, for Dr Linton draws upon his own wide field experience for illustration and does not attempt an eclectic exposition of ethnologic literature.

for example into the relation of race, language, and culture? My own impression is that they supply to those approaching the subject with fresh interest a facile, official interpretation, dulling curiosity. Yet as representatives of popular explanatory lectures without much data the essays are lucid, succinct, fair to authorities cited, and non-partisan.²

The value of the book, however, lies in its ingenious treatment of (1) primitive economics, (2) art, (3) religion, (4) society. The ingenuity in treatment consists of an exposition which, while essentially topical in orientation, yet grasps the advantages of vivid and continuous ethnographic example, and avoids the emasculated tone which abbreviated digests of full ethnographies give when not so oriented.³ (1) Eskimo technology, detailed and well illustrated, is followed by exciting excerpts concerning an Eskimo whale hunt, a Plains buffalo hunt, a Maori fishing expedition; the Iroquois serve to illustrate agriculture, not the matriarchate; still, a matriarchate is found to conclude a chapter on division of labor; a Kwakiutl potlatch and a Maori feast conclude a chapter on property. (2) Problems of art and symbolism, illustrated by striking plates as well as by text-figures (the book as a whole is extraordinarily attractive), employ again the cultures which served to give point and color to (1) above; introduced for the first time are Southwest pueblos and a Northeast Siberian tribe. (3) While additional cultures south of the equator are drawn upon, the areas by now familiar to the reader serve again to illustrate an encyclopedic range of religious aspects. (4) And now for primitive society one approaches the inner shrine of the cultures introduced from the outside, as it were (with a few additional cultures, as usual for each section). The utter solemnity of the author's interest in this last section may be judged by the fact that he propounds here "a sociological principle" (p. 321; perhaps other italicised pronouncements, as, e.g., p. 314, should also be regarded as sociological principles).

A certain willingness to generalize about "primitives," more or less restrained in the exemplified treatment, is boldly unleashed in a final summary of industrial, religious, artistic, and social life. The skillful manner in which these four topics are developed—apparently in a whimsically informal, rambling manner, actually with a fairly systematic underlying plan—makes the book an extremely readable introduction to the material of which it treats.

Both the marginal essays and the central section of the book contain points and parts previously published by the author. In this sense the book under review is especially an outgrowth of *Early Civilization*. As an example of important advances

² Non-partisan in more than one sense. Obviously, Dr Goldenweiser is equally at home on all continents, not to mention islands. Yet his book could never be considered as a treatment of world ethnography, for it lacks entirely the feeling of an areal approach to data.

³ Of the several recent books giving in major part digests of full ethnographies, those avoiding the emasculated tone are oriented in one way or another. I should mention as examples of complete success C. Daryll Forde's *Habitat, Economy and Society* (geographic adjustment of material culture and basic subsistence techniques) and Ruth Benedict's *Patterns of Culture* (a pointed psychological type for each culture). Dr Goldenweiser, in contrast, rings topical changes on a more or less constant group of tribes.

made upon earlier presentations, we have totemism originally presented "analytically" (heterogeneous, unconnected phenomena with abstract attitudinal resemblances), then from the "historic-geographical" point of view (adhesion of totem and sib): neither of these viewpoints are given up—indeed, the historical perspective is emphasized. But now totemism is also regarded as a configuration: "As such it can, of course, not be proved but only apperceived . . ." (p. 327, fn. 15).

C. F. VOEGELIN

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Cooperation and Competition among Primitive Peoples. MARGARET MEAD (ed.) (ix, 531 pp. \$4.00. New York: McGraw-Hill, 1937.)

There are chapters on, respectively, Arapesh (Mead), Eskimo (Mirsky), Ojibwa (Landes), Bachiga (Edel), Ifugao (Goldman), Kwakiutl (Goldman), Manus (Mead), Iroquois (Quain), Samoans (Mead), Zuni (Goldman), Bathonga (Goldman), Dakota (Mirsky), Maori (Mishkin), in the order given. The editor has contributed an introductory chapter, and a final chapter entitled "Interpretive Statements." The book professes a two-fold purpose and may be estimated from two points of view. It "differs from the ordinary textbook" in being "an attempt to describe a piece of research which is just begun; a stage in thinking which is tentative, experimental, inconclusive, but which is hopefully pointed in a direction which will make better thinking possible" (p. 1). Why the above order of presenting the material describing these tribes was adopted is not explained. A regional grouping, if no other were preferable for a specific purpose, would seem to have some advantage over the present juxtaposition of topics. For Ojibwa, Teton Dakota, Bachiga previously unpublished materials are offered. The accounts of tribes are summary clear statements of economic status and activities, social and political organization, and treatment and training of children. These accounts seem to us excellent; and they point up the relevant information on cooperation, competition, and individualism. This portion of the task is excellently executed.

A second (? secondary) purpose was to answer an inquiry posed by a subcommittee of the Social Science Research Council regarding competition and cooperation in primitive society. The committee offered these definitions:

Competition: the act of seeking or endeavoring to gain what another is endeavoring to gain at the same time. *Cooperation*: the act of working together to one end. We are interested in competitive and cooperative *habits*, not in competition and cooperation in the abstract. *Habit*: a usual or customary mode of action (p. 8).

The Subcommittee's question is repeated jestingly by the editor—who will not condescend to answer: "For the vague terms *cooperative* and *competitive habits* we have substituted *cooperative social systems* and *competitive social systems*" (p. 462).

If the long "Interpretive Statement" (p. 458-511) had been written by any one except a proved member of the faith, it would be regarded as almost undiluted buncum. Nine-tenths of it is meaningless; the other tenth cannot be nine-tenths justifiable. This definiteness of assurance upon our part jibes well with the tenor of

that chapter. It is, of course, sometimes manifestly misleading to take statements out of their context. The following however, which are samples of numerous if not of the majority of last pages are certainly no more illuminating in their context than out of it. Three examples from three pages:

It proved unprofitable to attempt to isolate such an intangible as "cooperative habits" and correlate it with type of technology; it was more profitable to discuss whether or not techniques requiring overt cooperation occurred in each classification. In other words, for the vague term *cooperative and competitive habits* we have substituted *cooperative social systems* and *competitive social systems*—that is, societies in which distribution of goods is a major competitive activity and societies in which such goods enrich the whole group and contribute to its security—categories which are more subject to analysis. By doing this the original foci of inquiry—the natural environment, the state of technology, the techniques of production, the nature of the social structure, and the educational system—become possible clues to the total emphasis within each social system. If there is correspondence between the techniques of production and the degree of competition, cooperation, or individualism of the whole system, we can examine the mechanisms within these social systems by means of which such techniques either are brought into complete dependence upon the major social emphasis, or are dynamic in determining that emphasis. Even when a perfect correspondence is found between the major emphasis and, for example, the religious philosophy, it may be that the so perfectly correlated element is crucial in determining the emphasis, but it is much more likely that the element was involved in the classification that was used, and should be merely added to the definition in order to define it more completely (p. 462).

... the Kwakiutl are highly competitive in their emphasis while the Maori are cooperative. The knowledge of the usefulness of large fishtrap fences does not prevent the Manus from fitting these fences into a small partnership pattern, whereas the Samoans use the same fishing method as the basis for village-wide participation and cooperation (p. 463).

Zuni and Arapesh displace scarcity of land into scarcity of labor, and therefore into a field where they can compensate for this scarcity by cooperative activity (p. 466).

I have tried desperately to extract a crumb of meaning from these pages in which the editor seems to have struggled equally desperately to put some meaning which, I infer, the presented data scarcely justify. The reviewer takes comfort in the contemplation that these pages are "hopefully pointed in a direction which will make better thinking possible." The possibility awaits fulfilment.

WILSON D. WALLIS

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Aftermath. A Supplement to The Golden Bough. JAMES GEORGE FRAZER. (xx, 494 pp. \$3.00. New York: Macmillan Co., 1937.)

Frazer explains that

This book is in no sense an independent treatise; it is simply, as the title purports, a supplement intended to provide some fresh information on certain subjects which I have discussed more at large in *The Golden Bough*.

The topics considered cover almost the whole range of the parent work but most of these receive only scant attention. The spread of the work may be judged

by the "chapters," numbering seventy-five, many occupying but fractional pages. A discussion of magic, which introduces the text, and a more specific section on magical control of the weather, are the most extended portions. Much of the newly marshalled data refer to Africa, though all other parts of the world are well represented except the Americas. The almost complete neglect of the New World is hard to reconcile with the wealth of data therefrom which Frazer might have used for amplification of this final contribution to his voluminous comparative study.

In the preface to the first edition of *The Golden Bough*, which appeared in 1890, Frazer wrote:

I cannot but feel that in some places I may have pushed [my theory] too far. If this should prove to have been the case, I will readily acknowledge and retract my error as soon as it is brought home to me.¹

Today, in the preface to *Aftermath* he states:

In [*The Golden Bough*], as well as in all my other writings, I have sought to base my conclusions by strict induction on a broad and solid foundation of well-authenticated facts. In the present work I have extended and strengthened the foundation without remodelling the superstructure of theory, which on the whole *I have seen no reason to change. But now, as always, I hold all my theories very lightly, and am ever ready to modify or abandon them in the light of new evidence.*²

Yet it was in 1896 that Boas published his compelling critique, *The Limitations of the Comparative Method of Anthropology*,³ in which he concluded his arguments as follows:

Thus we have seen that the comparative method can hope to reach the grand results for which it is striving only when it bases its investigations on the historical results of researches which are devoted to laying clear the complex relations of each individual culture . . . The historical method has reached a sounder basis by abandoning the misleading principle of assuming connections wherever similarities of culture were found. The comparative method, notwithstanding all that has been said and written in its praise, has been remarkably barren of definite results . . .

The great and important function of the historical method of anthropology is thus seen to lie in its ability to discover the processes which in definite cases led to the development of certain customs. If anthropology desires to establish the laws governing the growth of culture it must not confine itself to comparing the results of the growth alone, but whenever such is feasible it must compare the processes of growth, and these can be discovered by means of studies of the cultures of small geographic areas.⁴

In fairness to Frazer it must be remarked his insistence upon readiness to abandon theories was doubtless intended only to express a willingness to embrace

¹ J. G. Frazer, *The Golden Bough* (London, 1890), Vol. 1, p. vii.

² Page v. Italics mine.

³ *Science*, Vol. 4, pp 901-908.

⁴ *Idem.*, p. 908. Order of paragraphs reversed.

more acceptable theories born of the comparative method. But never since 1896 has there been an excuse for uncritical adherence to the comparative method; nor has there been reason for confusing the use of comparisons in the historical method with their use by adherents of the comparative method.

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DISCUSSION AND CORRESPONDENCE

CRANIAL DEFORMITY IN THE PUEBLO AREA

In the January-March issue of this journal, T. D. Stewart¹ has pointed out certain peculiarities of the cranial deformation found in the skulls from the Lowry Ruins.² According to him, this deformation is of the "lambdoid," not of the "occipital" type. Different from the flattening found in skulls from Pecos Pueblo, it resembles closely that found in the Chaco Canyon. Thus, the cranial deformation "fits in with the cultural evidence."

It is always gratifying to learn that one's description has enabled others independently to draw conclusions about matters beyond the scope of his original text. For the problem foremost in the writer's mind was that of the racial affinities of the people inhabiting the Lowry area. This he has tried to solve as best he could by "the more spectacular methods of the Pearsonian school," to quote Stewart once more.

Next to racial affinities, however, cranial deformation did intrigue the writer and he discussed its mechanical aspects and its influence on the size of the brain on pp. 166-74. But following Stewart's suggestions, we find that the flattening of the Lowry skulls (as stated in the report, all of them were deformed, but only five were sufficiently well preserved for a detailed examination) does not entirely correspond to Hooton's description of "lambdoid" deformation in his report on the Pecos Pueblo.³ Hooton in fact said: "In most instances . . . [it] is certainly not caused by artificial deformation. . . . It can be seen in many crania from European and non-European peoples of the present day. It is especially noticeable in the skulls of Armenoids and Finns" (p. 38). Hooton further states that the flattened area forms an angle of about 35°-45° with the Frankfurt plane. The deformation of the Lowry skulls appeared to the writer to correspond better to Hooton's occipital flattening. A comparison of Lowry skull No. 47,619 with the Pecos Pueblo skulls Nos. 59,911, or 60,076 will make this clear. But since this flattening does not affect the occipital bone as much as the region above it, and since the term "lambdoid deformation" had already been used, "obelionic flattening" appeared best to differentiate the Lowry type from the true occipital flattening as shown, e.g. by Pecos Pueblo No. 60,320. If the obelion is marked on the contours of the Lowry skulls, it will be seen that it is closer to the center of the flattened area than the lambda. With the few skulls at the writer's disposal he did not feel justified in going deeper into an analysis of cranial deformation than he actually did. In explaining

¹ *Different Types of Cranial Deformity in the Pueblo Area* (American Anthropologist, Vol. 39, pp. 169-71, 1937).

² Paul S. Martin, *Lowry Ruin in Southwestern Colorado; Masonry of Lowry Ruin and of the Southwest*, by Lawrence Roys; *Skeletal Material from the Lowry Area*, by Gerhardt von Bonin (Anthropological Series, Field Museum of Natural History, Vol. 23, No. 1, 1936).

³ E. A. Hooton, *The Indians of Pecos Pueblo, A Study of Their Skeletal Remains* (New Haven, 1930).

it by the action of a cradleboard, he did feel uncomfortable, for, as Stewart rightly remarks, the obelion is "rather high."

I am grateful to Dr Stewart for having called the attention of anthropologists to an interesting point which had escaped my notice. It should be clearly understood, however, that such morphological observations will shed no light on problems of racial affinities, nor can it be said that the various types of artificial flattening have been defined in an entirely satisfactory way.

Obviously, we are in need of further data and more detailed methods of analysis. The promised report by Stewart on the human remains from Chaco Canyon will be eagerly awaited. In the meantime, Dr Stewart deserves our gratitude for having shown how fruitful a cooperation between cultural and physical anthropology can be.

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MAMMOTH OR "STIFF-LEGGED BEAR"

In consequence of Dr W. D. Strong's paper on *North American Traditions Suggesting a Knowledge of the Mammoth*,¹ Dr F. G. Speck² and Dr T. Michelson³ have discussed other versions of the myth among the Montagnais, Naskapi, Cree, Ojibwa, and Penobscot. Dr Strong considers the Naskapi monster Katcheetohúskw as reminiscent of the mammoth, because of his elephantine size, large ears, and man-eating tendencies. Drs Speck and Michelson cite versions among the Naskapi, Cree, and Montagnais showing that the monster was referable to the *Ursidæ*. He is described as a "stiff-jointed bear of very large size, being man-eating, and having a head and ears like a bear." Both Speck and Michelson agree that the monster bear is purely mythical, and not reminiscent of the mammoth or mastodon. However, the three writers do not attempt an explanation of the tale in terms of aboriginal beliefs.

In the course of field work⁴ last summer among the Penobscot Indians of Maine, the underlying concept of the whole series of myths seemingly became apparent. One of my Penobscot informants, Andrew Dana,⁵ dictated his grandfather's version of the myth. A synopsis of this is given in translation below.

The correct Penobscot term for the monster is Wə'sk'ekkehs (called by my informant the "Great Hairless Bear"), which I have been unable to analyze satisfactorily.⁶

¹ American Anthropologist, Vol. 36, pp. 81-84, 1934.

² *Ibid.*, Vol. 37, pp. 159-63, 1935.

³ *Ibid.*, Vol. 38, pp. 141-43, 1936.

⁴ I am indebted to Professor Edward Sapir and Dr Morris Swadesh of Yale University for encouragement in this work.

⁵ His knowledge of Penobscot culture was obtained largely from his grandfather, Frank Dana (1845-1924), a Penobscot of the old school.

⁶ Wə'sk'ekkehs is the shortened form of original *wə'sk'ekkehso; the dropping of the final vowel being the result of the change of an appellative into a proper or personal name.

The informant explained the basic concept of the myth in this manner:

The Penobscot belief was that when a common black bear had once eaten human flesh, the bear's hair or fur would fall out entirely, and the bear would become naked and hairless. Once having tasted human flesh, the bear henceforth became dangerous and would attack other human beings. At this time the bear became the possessor of magic or supernatural power, so that he could charm and subdue those who chanced to meet him in the woods.

Dana said that he had heard a story told by some old Malecite Indians who claimed that they had seen a woman eaten by such a bear near Woodstock, New Brunswick. He added that his grandfather and many other old Penobscot of the last generation at Lincoln, Maine, were still so immersed in native superstitions that they believed the above account. To the best of his knowledge they always referred Wə'sk^wekkehs to the *Ursidae*.

In summary, the fundamental Penobscot concept of Wə'sk^wekkehs is concerned with a huge mythological bear-like monster, that by once eating human flesh had becomes hairless, anthropophagous, and the possessor of supernatural powers. A synopsis of the Penobscot version follows:

Wə'sk^wekkehs, the Great Hairless Bear

It was always the custom among the Penobscot for nearly everyone to go hunting in the autumn to procure winter food supplies. At some station or camp within the family hunting territory each hunter would dry and store deer meat, muskrat meat, and moose meat.

So once it happened, that one man, his wife, and children left Old Town alone on their fall hunting excursion by poling up the Penobscot River. When they reached the headwaters of the river they stopped, and the hunter reconnoitred the hunting grounds. At length he selected for a camp a spot of high ground that formed a gradually rising hill that was covered with coniferous trees and afforded an abundant supply of good firewood. Then he built their wigwam, fireplace, and a landing place.

The man hunted every day. Whenever he killed game, he would dry, smoke, and store meat. In this manner he kept himself busy all the time. As time went on, he extended his hunting trips to an ever greater distance. On one occasion night overtook him and he was forced to improvise for himself a temporary shelter in which to stay for the night.

The following day when the hunter arrived at his principal camp, everything did not seem quite right to him. While looking about the camp he found his children lying about on the ground, dead for an unknown length of time. It seemed that they had been trodden upon. He prepared their bodies and buried them. Alas! He grieved very much at the loss. However, he immediately started to search for his former wife. *When he went out of the camp he saw the large footprints of some unknown animal. It is said that these footprints were nearly like those of a bear.*

Right away he began to track the creature as he advanced up into the woods in a direction where the forest undergrowth was dense and almost impenetrable. At length, shortly after noon, after he had eaten a little, he paused to smoke and take a brief rest. As he sat there and looked about him, his gaze fell upon a gigantic tree, which was so large that at first he was astonished. As he continued to look at the huge tree he knew that it was very old, since its limbs were all in a rotting condition. Far up on top there was only one large branch remain-

(Suffix -ehso, "living creature"; -sk^w-, "bear" (?); my informant thought there was some relationship with wə'ske, "hock")

ing in place. There standing on top of it was an unknown animal who appeared like none he had ever seen before. Oh! He was very horrible looking.

Well then, he immediately turned to go back, since he knew beforehand that he would not be able to kill this animal. So he continued back until he arrived at the camp. He at once prepared a light pack with which to travel quickly, and proceeded downstream by canoe.

When he arrived at the Penobscot village (Old Town), he told what had happened and what he had seen. Everyone was greatly surprised, and right away the war chief went about gathering his men. That evening a council was held, and after a thorough discussion a plan was adopted. It was decided to leave early the following morning.

At daybreak, after they had eaten a large breakfast, the party began to pole up the river in canoes. When they arrived at the headwaters, they made camp, ate, and went to sleep early. As soon as morning came, they were very busy preparing their bows, darts, and large spears. They also made ready their stone axes, knives, and arrows, and started off as a group. At length they arrived at the place where the Indian hunter had seen this great beast. Immediately they surrounded the great tree and prepared for fighting, the bravest men standing in advanced position.

Then a warrior pounded the tree and said, "Well now, come forth! What do you look like?" Right away, they heard the beast growling. For a considerable time, he gave sound to a most horrifying noise—so that it resembled the ground trembling every time he growled. Then the next moment there was a sound of dry crackling wood within the tree, and the beast poked his head out from the entrance of his den. *He was built like a bear who had eaten human beings—the kind of bear that is hairless and fierce in appearance.* So these men were very frightened, but they stood firm and not one was stricken with a senseless fear.

Then the beast came forth and hitched down the bark of the tree towards the ground. After he had come into range of arrow shot, they all fired arrows at him as fast as they could bend their bows. Soon hundreds of arrows had pierced him. Then after he had climbed down below, there were so many arrows protruding from his body that he looked just like a porcupine. He started off through the woods, while the Indians followed him, continually plying him with arrows. They thought it peculiar that they could not deliver him a fatal blow. One man, a great shaman, from the start knew the beast to be of supernatural power, and so stepped forward and spoke in this manner: "Well now, O men, perhaps this creature possesses magic power. Let me see what I can do."

It had come about that nearly all their arrows were used up—only this shaman had one in his fingers.

Suddenly, they all looked, and Chickadee came flying forth. He perched himself upon the shoulder of the wizard and proceeded to whisper rapidly, "Chickadee-dee-dee—at his heel; Chickadee-dee-dee—at his heel." Then as this shaman looked he saw something hanging there and beating at the hock of this large beast. "It is his heart," thought the wizard. Right away he aimed and shot an arrow at the middle of it. Ah! His arrow pierced his heel exactly in the center.

"Come on! You ought to be starting away from here now, because this animal has exceedingly great magic power. I will need to prey upon him alone." So therefore, those other men immediately began to return.

Then the shaman began to trail him until he had overtaken this great evil creature near the border of a great sea. Whoops! The beast could not walk very well. He was stepping along feebly until he arrived at the edge of the water. There he hesitated and spoke to the shaman, saying: "Well, so now I have been killed by you. Never again shall I be a source of dread apprehension to you. It is now that you have overpowered me. You have made me very glad, because now I can go away where I belong—here in the sea where I ought to stay."

Thereupon this Wə'sk'ekkehs waded out slowly into the water. At length, his head disappeared beneath the surface.

Well then, this wizard immediately began to return. At last, he returned to his village again. So this is the end of the story of Wə'sk'ekkahs—except it is said that this great supernatural animal was never again seen at that place.

There are several novel features in this version that are worth commenting upon. The fact that the monster climbs trees shows very clearly that the Penobscot conception is that of an ursine animal, not a mammoth. Then too, the fact that the beast enters the sea and is apparently amphibious, points to a mythological aspect. This however, is not a feature unique to Penobscot mythology, since Skinner⁷ records a Potawatomi myth in which great enchanted bears live both on land and in water. When such a bear desired to sleep, he would walk into the water and lie beneath the surface.

From the limited source material available it is apparent that the other eastern tribes narrated versions which were very similar to that of the Penobscot. In Seneca mythology,⁸ Ganiagwaihegowa, a monster bear of enormous size, returns to kill any Indians that look at his tracks. The monster had no hair on its body; only a little on the end of its tail. The vital organs were believed to be protected from injury by a peculiar arrangement of the ribs, the monster being vulnerable only in the soles of its feet. An Indian boy killed it by shooting an arrow into the middle of one of the animal's forefeet.

According to Heckewelder⁹ the Delaware and Mohican both believed in the Big Naked Bear. In Delaware he was called Amangachktiát ("Big Rump") and in Mohican Ahamagachktiát Mechquá ("Huge Rump Bear"). The monster was described by the Delaware and Mohican as much larger than the common bear, being long-bodied, and naked all over, except for a spot of white hair on his back. He was very ferocious and devoured both man and beast. The heart was small so that the bear could seldom be killed with an arrow. This version seems to be mythological since Heckewelder states that the old Indians said they were "relying on the authenticity of their forefather's relations." Zeisberger¹⁰ says concerning the Delaware version:

There is likewise a kind of bear, much larger than the common bear, with much hair on the legs, but little on the bodies, which appear quite smooth. The Indians call it the king of bears, for they have found by experience that many bears will willingly follow it. While all

⁷ A. Skinner, *The Mascoutens or Prairie Potawatomi Indians, III* (Bulletin, Public Museum of Milwaukee, Vol. 6, No. 3, 1927), pp. 367-68.

⁸ J. Curtin and J. N. B. Hewitt, *Seneca Fiction, Legends, and Myths* (Thirty-second Annual Report, Bureau of American Ethnology, 1918), pp. 342-43.

⁹ *Letter of Heckewelder to Dr Benjamin S. Barton on the Big Naked Bear* (Transactions, American Philosophical Society, Vol. 4, Philadelphia, 1799), pp. 260-62.

¹⁰ *David Zeisberger's History of the Northern American Indians*, ed. by Hulbert and Schwarze (Ohio State Archaeological and Historical Society, 1910), p. 58.

The identical account is also found in G. H. Loskiel, *History of the Mission of the United Brethren among the Indians in North America* (London, 1794), Part 1, p. 80.

the bears are carnivorous . . . this kind of bear is particularly voracious. Many instances are known where they have seized upon even defenseless Indian women and children. In more northerly regions, as, e.g. in the country of the Mingoes [Iroquois], these are more frequently found and they have killed many Indians.

From the Lake St. John Montagnais, Dr Speck¹¹ gives a description of the "great bear" that may or may not be related to this discussion. It may be mythological or may refer to the Barren Ground bear. This bear does not hibernate in a den, has a "very savage humor," and has very tender flesh.

Such beliefs in the transformation of ordinary bears into ferocious monsters do not appear to be limited to Algonkian peoples. Dr Sapir¹² has made the following note from the distant Kutchin:

Sometimes a bear will crawl out of his den in the winter and will roll himself in the snow, which freezes to a hard crust. Then he will go ravaging the country fiercely in search of food. Such a bear was dangerous because arrows could not pierce the ice crust which covered him. The term for this type of bear in Kutchin is *t'a't'ánh*.

In conclusion, the basic element of the Eastern Algonkian myth is the supernatural transformation of a common black bear by the eating of human flesh into a large hairless, anthropophagous, supernatural being. It is possible to hypothesize a relationship of this belief with the Windigo or fabulous cannibal giant concept of the Ojibwa, the Wítigo of the Montagnais, and the Kivahk^{wé} of the Penobscot. A human being, often a shaman, who has once eaten human flesh becomes a supernatural cannibal giant or Windigo, since the eating of such a powerful creature as man gives his soul-spirit great strength. Henceforth he continues to live on human flesh, and can only be killed by sorcery, not by natural means.¹³ Bears were held in great respect by Algonkian peoples, and since they were to them the largest and the most formidable and intelligent of all North American animals, they were held by the aborigines to be a close second in the animate world to man himself. Since both beliefs have much in common, I therefore suspect an extension of the human Windigo concept to the bear species, or else the existence of some identical proto-concept.

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A CRITICISM OF A CHEMICAL METHOD FOR DETERMINING THE ANTIQUITY OF BONES

On pages 34 and 35 of A. E. Jenks' *Pleistocene Man in Minnesota*¹ a number of chemical and optical tests are cited which are claimed to prove that the bones are "fossilized" and that the fossilizing mineral is collophane.

¹¹ F. G. Speck, *Naskapi* (Norman, 1935), p. 109.

¹² E. Sapir, Kutchin mss. I am indebted to Dr Mary Haas of the Institute of Human Relations for this reference.

¹³ For example, see Speck, *op. cit.*, p. 73.

¹ Minneapolis, 1936.

Optical tests might or might not prove the antiquity of these bones, but the chemical tests quoted prove exactly nothing.

It is stated that the samples "turn dark and give up water in the closed tube, effervesce in nitric acid and furnish tests for calcium and for the phosphate radical. These tests prove that the bones consist of the calcium carbonate-phosphate mineral known as *collophane* and hence are fossilized" (p. 35).

The fallacy in this proof lies in the fact that recent bones, as soon as the fat is gone enough to allow the nitric acid to affect the surface, will give these tests as well as very old bones partly changed to collophane.

The mineral collophane, stated to be present, is practically the same chemically as the mineral matter in fresh bone, except for the addition of chemically combined water. It is not surprising, therefore, that recent bones and "fossilized" bones containing collophane both effervesce when treated with nitric acid and give tests for calcium when heated in the closed tube because of carbonization of organic matter (formation of charcoal); "fossilized" bones may turn dark because of carbonization of residual organic matter, always present, or from the presence of deposited iron hydroxide or of manganese. Bones from the the Pueblo III and the Pueblo IV ruins of the Southwest likewise commonly contain iron and manganese salts.

A test for combined water will always be obtained from either fresh or from "fossilized" bones. In dry "fossilized" bones, water could be derived from the mineral collophane or from hydrated oxides of iron or of manganese, or from gypsum or from clay (silt) deposited in the cavities. In testing modern but dried bone, water would be given off from heat decomposition of organic matter; this water could come, also, from clay or from a number of quick forming hydrated mineral salts.

The effervescence is due to carbonates, which would be present in either case; calcium and the phosphate radical are present in both the original bone and in those which are mineralized with collophane.

In other words, the bones in question may be from an ancient or from a recent period, but the chemical tests quoted can not be used as proof for the age of these or of other bones.

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SWIMMING-PADDLES AMONG NORTHERN INDIANS

Among interests aroused in connection with travel by water and the intimacy with this element attained by peoples whose lives and activities are marked by its proximity, we find a point overlooked. It has been taken for granted that familiarity with water travel would lead to proficiency in the arts of swimming among people who find the water to "be their element"—as one thinks of the tribes of Oceania. There is a contrast in the situation. Tribes of northern North America whose hunting pursuits and transportation facilities are so largely determined by

river, lake, and sea, have indeed achieved marvels in the construction of craft and equipment in navigation. But the association of swimming proficiency is wanting. This means something in the history of cultural development—a duality in adaptation to being “in” or “on” the element.

A glance over the literature dealing with tribes of the north reveals a somewhat convincing phase of the difference between the two mediums of water-progression. The ability to swim is not a characteristic of the Eskimo nor of the Indians of the north. It has received frequent comment on the part of ethnologists. Little mention has been made of inventions to insure safety among these peoples in cases of accident upon the water or when it becomes necessary to traverse water zones even though they may be narrow in extent. Two citations which are known to American ethnologists might be mentioned as instances where some attention has been given to the native method of supplementing their helplessness in the water with an invention known as the “swimming paddle.” Turner describes a swimming board “used to assist in swimming. . . . One board is held in each hand and used as a paddle to push the swimmer along. Indians able to swim are scarce.”¹ A. B. Skinner also referred to a similar device in use among some Saulteaux or Cree guides with whom he traveled in 1908. My own observations cover the Montagnais of Lake St. John and adjacent regions. Here a cedar hand paddle is used, ovoid in shape, rounded on the bottom and with a hollowed margin on the upper side to fit the grasp of the hand. The paddles average six inches in breadth and three in height. The paddle is held between the thumb and the closed fingers, one paddle in each hand, and used when crossing narrow streams by men who are not able to keep themselves afloat by the “dog fashion” swimming stroke alone. The paddles are not kept in the hunter’s outfit but are made when occasion arises. They do not indulge in the sport of bathing or swimming, and practically all disclaim any ability to “swim,” for which act, incidentally, they possess a term (*təpokácəmo*). The coldness of northern waters is a consideration of weight in the question at issue, but to my mind not a final and decisive one. The northern men are not so sensitive to cold as to jeopardize their lives through neglect of an invention which to employ would merely make them uncomfortable in an element so prevalent in their daily environment. That the use of hand-paddles did not develop into a sport among these people is evidently due to the exhausting strain exerted upon the deltoid muscles when they are employed.

It appears, in short, that a distinction may be observed in the varied manners by which natives in various parts of the world have succeeded in or failed in mastering the watering spaces over which their paths lead them in their constant movements for food and security.

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¹ Lucien M. Turner, *Ethnology of the Ungava District, Hudson Bay Territory* (Eleventh Annual Report, Bureau of American Ethnology, pp. 159–350, 1894), p. 321, fig. 148.

A METHOD FOR PHONETIC ACCURACY AND SPEED

An essential to adequate phonetic recording is a plan of approach that will bring the different problems into focus. The "ear" may be no more than average in acuteness and one may have little or no formal training. Given a good method and a certain amount of patience, a relatively unequipped person can do good work. Without method, no matter how well equipped, one is likely to fail to discover some important phonetic distinctions in the given language. I discuss here some general considerations and then give a specific procedure which may be followed in the field situation. The procedure may take from a couple of days to a week or so, depending on the difficulty of the language and the ability of the student. When he has followed it through, he will have a complete or nearly complete knowledge of the phonetics of the language, and be able to do further recording both accurately and at a rate that will justify the initial investment of time.

The ear, like all the sensory mechanisms, is most effective in the discrimination of immediately present stimuli. If a Frenchman working on English phonetics has his informant pronounce *lip* and *leap* in succession, he will be able to hear the difference, though he might otherwise be baffled by the sounds. Contrasts stand in relief to the extent that the compared words are otherwise alike; pairs of words like *lip*: *leap*, differing in only one respect (minimally different words), are most effective. The contrast is more fully perceived in a pair of words which contain as few strange sounds as possible. Once a difference is found to be consistently present in one pair of words, other words may be compared with the key words and classed with one or the other.

Each language has a limited number of types of sounds occurring in a limited number of permutations. Discover just what the speech-sound types are and the sequences in which they occur, and your ability to identify the sounds begins to approach the facility of the native speaker. Instead of thinking in terms of the endless number of possible types of sounds, one needs to think only of a few possible interpretations. For example, a nasal sound at the beginning of a syllable in English can be either *m* or *n* but nothing else. The phonetician who does not take the nature of the language into consideration has to think of a number of cardinal types and the possibility of intermediate or aberrant types. One who has a conventional training in phonetics must realize that the "cardinal types" recognized in phonetic science are only convenient points of reference, and should school himself to think in terms of the speech-sound types of the language he is working on.

The sounds of a language are reducible to a set of phonemes. A phoneme may be defined as one of the limited number of classes of speech sounds which make up the simplest exhaustive analysis of the phonetics of a given language. Like all empiric types, there is a certain amount of latitude within each. One kind of latitude may be called free variation, and represents the range found in different instances of the same phoneme in the same phonetic surroundings. Another kind of latitude is correlated with differences in phonetic surroundings, and may be called positional variation; for example, English vowels tend to be shorter before voiceless consonants

(e.g., *beat, leaf*) than before the corresponding voiced consonants (e.g., *bead, leave*). Variation due to emotional and other circumstances may also have to be recognized.

One may test the accuracy of one's observations by pronouncing words and having the informant listen and pass judgment. But this device should be used sparingly, and with tact and a realistic sense of what the informant's reaction may mean. This depends on how good you are in pronouncing what you intend to, how critical the informant is, and the extent to which you are both paying attention to the same features. The method of pronouncing something different from what you hear as a negative approach is confusing and meaningless to the informant, unless you already know the phonemic possibilities and substitute one actual phoneme for another. In general, never accept a conclusion solely on the basis of your own pronunciation or the informant's reaction to your pronunciation.

Avoid making your informant speak so slowly as to make his speech unnatural. Or, having listened to something said too slowly, do not assume that it is correct until you have again compared it with more natural speech.

Distinguish between free variation and actual alternates. Sometimes there is more than one way of saying a word, and the informant may give something that is actually phonemically different when asked to repeat (e.g., *don't* and *do not*). The informant is more or less aware of alternation, almost never aware of free variation. Free variation applies to all instances of a phoneme in a given position, alternation generally only in certain words (thus, *wont* is not replaceable by *wo not*).

It is sometimes easier to identify sounds in certain phonetic contexts than in other. This being so, the phonetics of the same morphological element in a more favorable position may sometimes be used as an aid in a difficult case. If one is in doubt as to whether the last sound in *bands* is *s* or *z*, one might compare the semantically equivalent final of *rans* (*z* shown clearly if one contrasts with *Vance*). However, this method is not always trustworthy, since morphemes may vary in phonemic make-up; thus note *-s* in *backs*. The morphemic test is not an essential part of the suggested procedure.

The steps of the suggested field procedure are:

1. *Record a few hundred short utterances.* In the initial list each utterance may consist of a single word. Longer utterances and text may be reserved for later (see step 8).

It saves time to have a list of English words made out in advance. Any list will do. If the informant cannot give you the equivalent of one of the words in your list without long thinking, skip it. It is a good idea to have the words in the list numbered, so that they can be easily referred to.

Do not take too much time trying to get accurate recordings at this stage, but make as good a record as you can on the basis of three to a dozen repetitions of the utterance. Speed saves your time and your informant's patience.

If in the repetition of an utterance you hear something different, indicate the variants if convenient. You will have to determine later what is free variation and what is alternation.

If you can, try to indicate syllable division, since it may prove important later. Likewise, stress and (or) pitch may have to be taken into account.

2. *Make an analytic list of the sounds recorded.* (a) Recognize certain "positions" within the word; for example, initial consonants and consonant-clusters, medial consonants and clusters, final consonants and clusters, initial vowels and vowel clusters (diphthongs, triphthongs, etc.), medial vowels and clusters, final vowels and clusters. (b) Make a table of the sounds occurring in each position. (c) Make a list of the examples.

The table of sounds is made by placing the sounds in rows and columns according to their quality. The rows may be according to type of sound (stop, spirant; voiced, voiceless), the columns according to position (labial, dental, etc.). Clusters are best tabulated separately.

The division into initial: medial: final is fairly good to begin with, but other divisions may have to be recognized, for example, stressed and unstressed, syllabic initial and syllabic final, position before or after given phonemes or types of phonemes. Any dichotomy that brings out differences of occurrence should be used.

3. *Test out the phonetic distinctions.* For each contrast (e.g., voiced stop versus voiceless stop, stop versus spirant, dental versus alveolar, etc.) shown in the tabulation of sounds for a given position, select words having the given sounds and have your informant pronounce them several times in succession while you listen attentively. In some cases you will find that there is really no distinction, or that both utterances vary within a range that includes your different recordings. Where the distinction is indubitable, listen attentively and try to form some notion of precisely what the difference is. Use your eye as well as your ear, by looking into the informant's mouth (in so far as you can and he will permit) to observe the mechanism of production.

Use simple cases as much as possible in testing contrasts. Make a collection of minimally different utterance pairs.

Make several tests of each contrast. Otherwise you may make premature decisions, either positive or negative.

4. *Study the symmetry of the tables.* Since phonetic systems are largely symmetrical, investigate any apparent non-symmetries. If you have recorded *b*, *p*, and *t*, look for a *d*. It is possible that the language lacks one, but it is also possible that your list did not happen to include an example or that some of the words in which you wrote *t* really had a *d*. Another possibility is that the *b:p* distinction is erroneous. Never hesitate to re-test a distinction.

5. *Compare the range of sounds heard in the different positions.* Note whether some positions have more different types of sounds or combinations than others. For example, one finds that some languages do not have consonant clusters in initial position. In some languages, there are more consonantic distinctions in medial than in initial position, more consonantic distinctions in syllabic initial than in syllabic final, more vocalic distinctions in accented than in unaccented syllables.

6. *Make a phonemic identification of the sounds recorded.* These principles may be followed:

(a) Sounds or sound complexes found in contrast with each other in the same position are phonemically different.

(b) Relatively similar sounds in different positions may be phonemically the same, provided they are in complementary distribution, that is, if they contrast with each other in none of the positions in which they occur. Similarity has to be judged by reference to the total range of sounds in the given position; thus, if *i*, *e*, *ε* occur in one position and *ι*, *ε*, *ä* in another, the relative alignment is *i*:*u*::*ε*:*ε*:*ä*.

(c) To determine how much is to be included in each phonemic unit, compare the different complexes. Any complex may be either: (1) a part of a phoneme; (2) a phoneme; (3) a combination of phonemes; (4) a part of a phoneme plus a phoneme—this possibility is precluded at the beginning of an utterance; (5) a phoneme plus a part of a phoneme—this possibility is precluded at the end of an utterance. The choice is made on the principle that that is classed as a unit phoneme which shares broad phonetic and distributional characteristics with other unit phonemes; that is classified as a cluster which shares characteristics with other complexes which have to be considered clusters. Nothing is a fragment of a phoneme which cannot be taken to be a part of what immediately precedes or follows. Nothing is taken to be a unit unless it cannot be taken to be a fragment or a combination, for one tries to do with as few discrete units as possible. If something is to be considered a cluster, it must be made up of units which are also otherwise represented, and there must be other clusters with which it is comparable. Syllabic division is frequently a good test of clusters, since the phonemes in a cluster frequently fall into separate syllables; unit phonemes tend to be unisyllabic. This test, like all others, must be used as a relative criterion by reference to the particular language, since in some languages single phonemes may be ambisyllabic or clusters unisyllabic.

The following are some possible interpretations of given phonetic complexes. A diphthong may be either a positional variant, a distinct unit phoneme, a cluster of two vowels, a vowel plus a semi-vowel. An affricate may be a positional variant, a distinct unit phoneme, a stop plus a spirant. A long consonant may be a positional variant, a distinct unit phoneme, a cluster of identical phonemes. An aspirate may be a positional variant, a distinct unit phoneme, a stop plus *h*. Studies showing in detail how the choice of interpretation is made are contained in Nos. 2 and 3 of the bibliography.

7. *Determine what prosodic phonemes need to be recognized.* Prosodic features (stress, pitch, intonation, length) may be either (a) correlated with position in the word, (b) associated with given qualitative phonemes, (c) independently variable. If utterances which are otherwise of the same or comparable phonemic make-up are different prosodically, a set of prosodic phonemes must be recognized.

8. *Make a formulation of the facts and test its completeness.* Make a list of phonemes, indicating for each the positions in which it occurs, and its phonetic value in each position. Then test out your formulation, by seeing whether you can completely analyze the words of the original list, using only the phonemes you recognize with the values you recognize. Finally test out your formulation on longer utterances. Correct or change the formulation as necessary.

When you have the final list of phonemes, fix an alphabet by taking a single character for each phoneme. The test of the completeness of the formulation can best be done with the alphabet by trying to record with the alphabet without using additional symbols or diacritics and without entailing any ambiguity.

While the procedure described contains all that is necessary to attain a phonemic analysis, the application to some of the specific problems that are found may not be self evident to one who has not had experience with phonemic analysis. However, even if the final solution is not clear in some points, the method of working with the sounds of the language as a related body of facts should bring one much nearer to an adequate understanding of the phonetics than the method of recording each word separately as one gets to it. One may make up for lack of experience by reading descriptions of different phonetic systems that have been worked out along phonemic lines.

Speed and precision in identifying strange sounds comes only with practice. The practice gained in following through the described procedure is enough to greatly improve one's ability to identify sounds, even those which are quite new at first. But sometimes it may be necessary to continue cautiously with some of the more difficult sounds until one becomes fully at home with them.

So far I have talked chiefly in terms of the field worker who is specializing on language or who is going to spend enough time with a tribe to make several days devoted to phonetics worth-while. The field ethnologist who is going to spend a very short time with a tribe might prefer to use parts of the method outlined without trying to carry it out systematically and completely.¹

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¹ To the one who is pressed for time, I should like to make the suggestion that (unless he is very well trained in phonetics) he avoid over-nice phonetic distinctions. Ethnographic writings are often cluttered with a lot of fancy symbols that are not as significant as they might seem. If one does not have the time it takes to get an approximately correct phonemic alphabet, one might just as well undercut as err on the other side.

NOTES AND NEWS

INTERIM REPORT OF THE COMMITTEE FOR AMERICAN INDIAN LANGUAGES

The Committee for American Indian Languages appointed by the President at the 1936 meeting of the American Anthropological Association met with a similar committee of the Linguistic Society of America on April 25-26, 1937, at Columbia University. Franz Boas presided and Morris Swadesh acted as secretary. A Continuing Committee, consisting of Boas, *chairman*, Bloomfield, Kroeber, Sapir, and Swadesh, was elected to carry out the proposals of the combined committees.

It was decided that some sort of organization, formal or informal, is needed to stimulate interest and to coordinate the efforts of scientists. On the invitation of Professor Kent, secretary of the Linguistic Society of America, it was agreed that a Group for American Indian Languages be formed under the auspices of the Linguistic Society. It is hoped that a similar arrangement can be made with the American Anthropological Association. According to the arrangement with the Society members of the Group are not required to become members of that Society. It is planned that a session of the 1937 meeting of the Society be devoted to Indian languages.

It was agreed that a regularly appearing journal is essential, and the Continuing Committee is seeking to make this possible.

It was agreed that a critical bibliography would be of great value. The Continuing Committee is considering this matter. At present it is gathering data on unpublished material. Anyone who has manuscript material is asked to communicate with Franz Boas, Columbia University, giving the nature (vocabulary, texts, grammatical notes) and quantity of the material.

The importance of increased study of South American languages was stressed.

It was agreed that the scientific value of studying Indian languages, because of their great number and variety, is so great as to justify considerably more attention than is now given, and it was therefore decided that steps should be taken to bring the facts to the attention of scholars, learned societies, and foundations. Language being a large and essential part of human behavior, it follows that all the social sciences need the data that can be gained from a careful study of as many varieties of languages and language situations as possible. Philosophy and logic too are benefited by such studies. Arrangements are now under way to prepare a bulletin of information on the study of American Indian languages.

Those wishing to join the Group for American Indian Languages or who desire information are asked to communicate with Morris Swadesh, University of Wisconsin.

BRINTON CENTENARY CELEBRATION

The centenary of the birth of Daniel Garrison Brinton was held by the Delaware County Institute of Science at Media, Pennsylvania, on May 13, 1937. Among the addresses commemorating Dr Brinton, who was Professor of American Archae-

ology and Linguistics, University of Pennsylvania, from 1886 until his death in 1899, were "D. G. Brinton's Contributions to Science" by Dr Edwin G. Conklin and "D. G. Brinton and Anthropology" by Dr Clark Wissler. Announcement was made by Dr Frank G. Speck of a contemplated D. G. Brinton Centenary Volume.

RESEARCH TRAINING FELLOWSHIPS AND GRANTS-IN-AID FOR 1937-38 are offered by the Social Science Research Council. Application for information should be addressed to the Secretary for Fellowships and Grants-in-Aid, John E. Pomfret, 230 Park Avenue, New York City.

MONOGRAPHS OF THE AMERICAN ETHNOLOGICAL SOCIETY

The American Ethnological Society announces the establishment of a new series, *Monographs of the American Ethnological Society*, to be issued annually, beginning in 1938. The object of the society in establishing this series is to furnish a new medium for the publication of longer studies. The undersigned Board of Editors will select the study to be published in 1938 from manuscripts submitted on or before February 1, 1938.

Manuscripts should not exceed 75,000 words (about 175 to 200 pages of print); must be in the field of ethnology; and should be in condition for printing. They should be addressed to the Secretary, American Ethnological Society, c/o Department of Anthropology, Columbia University, New York City. Manuscripts not accepted for publication will be returned to the authors after the selection has been made.

CLARK WISSLER, *Chairman*

A. I. HALLOWELL

ALEXANDER LESSER

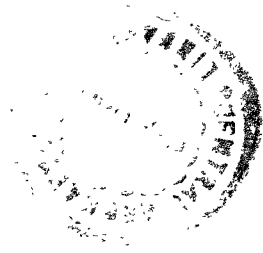
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